INSTITUT INTERNATIONAL DE PHYSIQUE

fondé par Ernest Solvay

DOUZIEME CONSEIL DE PHYSIQUE

tenu à l'Université Libre de Bruxelles

du 9 au 14 octobre 1961

LA THEORIE QUANTIQUE DES CHAMPS

INTERSCIENCE PUBLISHERS,
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fondé à l'initiative de Monsieur E. Solvay

le 1er mai 1912

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THE SOLVAY MEETINGS AND THE DEVELOPMENT OF QUANTUM PHYSICS

by **N. BOHR**

The series of conferences originally convened, just fifty years ago, at the far-sighted initiative of Ernest Solvay and continued under the auspices of the International Institute of Physics founded by him, have been unique occasions for physicists to discuss the fundamental problems which were at the centre of interest at the different periods, and have thereby in many ways stimulated modern development of physical science.

The careful recording of the reports and of the subsequent discussions at each of these meetings will in the future be a most valuable source of information for students of the history of science wishing to gain an impression of the grappling with the new problems raised in the beginning of our century. Indeed, the gradual clarification of these problems through the combined effort of a whole generation of physicists was in the following decades not only so largely to augment our insight in the atomic constitution of matter, but even to lead to a new outlook as regards the comprehension of physical experience.

As one of those who in the course of time have attended several of the Solvay conferences and have had personal contact with many of the participants in the earliest of these meetings, I have welcomed the invitation on this occasion to recall some of my reminiscences of the part played by the discussions for the elucidation of the problems confronting us. In approaching this task I shall endeavour to present these discussions against the background of the many-sided development which atomic physics has undergone in the last fifty years.

I

The very theme of the first Solvay conference in 1911, Radiation Theory and Quanta, indicates the background for the discussions in those days. The most important advances in physics in the former century were perhaps the development of Maxwell's electromagnetic theory, which offered so far-reaching an explanation of radiative phenomena, and the statistical interpretation of the thermo-dynamical principles culminating in Boltzmann's recognition of the relation between the entropy and probability of the state of a complex mechanical system. Still, the account of the spectral distribution of cavity radiation in thermal equilibrium with the enclosing walls presented unsuspected difficulties, especially brought out by Rayleigh's masterly analysis.

A turning point in the development was reached by Planck's discovery, in the first year of our century, of the universal quantum of action revealing a feature of wholeness in atomic processes completely foreign to classical physical ideas and even transcending the ancient doctrine of the limited divisibility of matter. On this new background the apparent paradoxes involved in any attempt at a detailed description of the interaction between radiation and matter were early stressed by Einstein, who did not only call attention to the support for Planck's ideas offered by investigations of the specific heat of solids at low temperature but, in connection with his original treatment of the photoelectric effect, also introduced the idea of light quanta or photons as carriers of energy and momentum in elementary radiative processes.

Indeed the introduction of the photon concept meant a revival of the old dilemma from Newton's and Huygens' days of the corpuscular or undulatory constitution of light, which had seemed resolved in favour of the latter by the establishment of the electromagnetic theory of radiation. The situation was most peculiar since the very definition of the energy or momentum of the photon, given by the product of Planck's constant and the frequency or wave number of the radiation, directly refers to the characteristics of a wave picture. We were thus confronted with a novel kind of complementary relationship between the applications of different fundamental concepts of classical physics, the study of which in the course of time was to make the limited scope of deterministic description evident and to call for an essentially statistical account of even the most elementary atomic processes.

The discussions at the meeting were initiated by a brilliant exposition by Lorentz of the argumentation based on classical ideas leading to the principle of equipartition of energy between the various degrees of freedom of a physical system, including not only the motion of its constituent material particles but also the normal modes of vibration of the electromagnetic field associated with the electric charge of the particles. This argumentation, following the lines of Rayleigh's analysis of thermal radiative equilibrium led, however, to the well known paradoxical result that no temperature equilibrium was possible, since the whole energy of the system would be gradually transferred to electromagnetic vibrations of steadily increasing frequencies.

Apparently the only way to reconcile radiation theory with the principles of ordinary statistical mechanics was the suggestion by Jeans that under the experimental conditions one did not have to do with a true equilibrium but with a quasi-stationary state, in which the production of high frequency radiation escaped notice. A testimony to the acuteness with which the difficulties in radiation theory were felt was a letter from Lord Rayleigh, read at the conference, in which he admonishes to take Jeans' suggestion into careful consideration. Still, by closer examination it was soon to become evident that Jeans' argument could not be upheld.

In many respects the reports and discussions at the conference were most illuminating. Thus, after reports by Warburg and Rubens of the experimental evidence supporting Planck's law of temperature radiation, Planck himself gave an exposition of the arguments which had led him to the discovery of the quantum of action. In commenting on the difficulties of harmonizing this new feature with the conceptual framework of classical physics, he stressed that the essential point was not the introduction of a new hypothesis of energy quanta, but rather a remoulding of the very concept of action, and expressed the conviction that the principle of least action, which was also upheld in relativity theory, would prove a guidance for the further development of quantum theory.

In the last report at the conference, Einstein summarized many applications of the quantum concept and dealt in particular with the fundamental arguments used in his explanation of the anomalies of specific heats at low temperatures. The discussions of these phenomena had been introduced at the meeting in a report by Nernst

on the application of quantum theory to different problems of physics and chemistry, in which he especially considered the properties of matter at very low temperatures. It is of great interest to read how Nernst in his report remarked that the well known theorem regarding the entropy at absolute zero, of which since 1906 he had made important applications, now appeared as a special case of a more general law derived from the theory of quanta. Still, the phenomenon of the superconductivity of certain metals at extremely low temperatures, on the discovery of which Kamerlingh Onnes reported, presented a great puzzle, which should first many years later find its explanation.

A new feature, commented upon from various sides, was Nernst's idea of quantized rotations of gas molecules, which was eventually to receive such beautiful confirmation in the measurements of the fine structure of infra-red absorption lines. Similar use of quantum theory was suggested in the report by Langevin on his successful theory of the variation of the magnetic properties of matter with temperature, in which he made special reference to the idea of the magneton, introduced by Weiss to explain the remarkable numerical relations between the strength of the elementary magnetic moments of atoms deduced from the analysis of his measurements. Indeed, as Langevin showed, the value of the magneton could at any rate be approximately derived on the assumption that the electrons in atoms were rotating with angular momenta corresponding to a Planck quantum.

Other spirited and heuristic attempts at exploring quantum features in many properties of matter were described by Sommerfeld, who especially discussed the production of x-rays by high speed electrons as well as problems involving the ionization of atoms in the photo-effect and by electronic impact. In commenting upon the latter problem, Sommerfeld called attention to the resemblance of some of his considerations with those exposed in a recent paper by Haas, who in an attempt at applying quantum ideas to the electron binding in an atomic model like that suggested by J.J. Thomson, involving a sphere of uniform positive electrification, had obtained rotational frequencies of the same order of magnitude as the frequencies in optical spectra. As regards his own attitude, Sommerfeld added that instead of trying from such considerations to deduce Planck's constant, he would rather take the existence of the quantum of action as the fundament for any approach to questions of the constitution of atoms

and molecules. On the background of the most recent trend of the development this utterance has indeed an almost prophetic character.

Although at the time of the meeting there could, of course, be no question of a comprehensive treatment of the problems raised by Planck's discovery, there was a general understanding that great new prospects had arisen for physical science. Still, notwithstanding the radical revision of the foundation for the unambiguous application of elementary physical concepts, which was here needed, it was an encouragement to all that the firmness of the building ground was just in those years so strikingly illustrated by new triumphs for the classical approach in dealing with the properties of rarefied gases and the use of statistical fluctuations for the counting of atoms. Most appropriately, detailed reports on these advances were in the course of the conference given by Martin Knudsen and Jean Perrin.

A vivid account of the discussions at the first Solvay meeting I got from Rutherford, when I met him in Manchester in 1911, shortly after his return from Brussels. On that occasion, however, Rutherford did not tell me, what I only realized some months ago by looking through the report of the meeting, that no mention was made during the discussions at the conference of a recent event which was to influence the following development so deeply, namely his own discovery of the atomic nucleus. Indeed, by completing in such unsuspected manner the evidence about the structure of the atom, interpretable by simple mechanical concepts, and at the same time revealing the inadequacy of such concepts for any problem related to the stability of atomic systems, Rutherford's discovery should not only serve as a guidance, but also remain a challenge at many later stages of the development of quantum physics.

II

By the time of the next Solvay conference in 1913, the subject of which was the Structure of Matter, most important new information had been obtained by Laue's discovery in 1912 of the diffraction of Röntgen rays in crystals. The discovery removed indeed all doubts about the necessity of ascribing wave-properties to this penetrating radiation, the corpuscular features of which in its interaction with matter, as especially stressed by William Bragg, had been so strikingly illustrated by Wilson's cloud chamber pictures showing the tracks of

high speed electrons liberated by the absorption of the radiation in gases. As is well known, Laue's discovery was the direct incentive to the brilliant explorations of crystalline structures by William and Lawrence Bragg, who by analyzing the reflection of monochromatic radiation from the various sequences of parallel plane configurations of atoms in crystal lattices were able both to determine the wave length of the radiation and deduce the type of symmetry of the lattice.

The discussion of these developments, which formed the main topic of the conference, was preceded by a report by J.J. Thomson about the ingenious conceptions regarding the electronic constitution of atoms, by which without departing from classical physical principles he had been able, at least in a qualitative way, to explore many general properties of matter. It is illuminating for the understanding of the general attitude of physicists at that time that the uniqueness of the fundament for such exploration given by Rutherford's discovery of the atomic nucleus was not yet generally appreciated. The only reference to this discovery was made by Rutherford himself, who in the discussion following Thomson's report insisted on the abundance and accuracy of the experimental evidence underlying the nuclear model of the atom.

Actually, a few months before the conference my first paper on the quantum theory of atomic constitution had been published, in which initial steps had been taken to use the Rutherford atomic model for the explanation of specific properties of the elements, depending on the binding of the electrons surrounding the nucleus. As already indicated, this question presented unsurmountable difficulties when treated on ordinary ideas of mechanics and electrodynamics, according to which no system of point charges admits of stable static equilibrium, and any motion of the electrons around the nucleus would give rise to a dissipation of energy through electromagnetic radiation accompanied by a rapid contraction of the electron orbits into a neutral system far smaller than the size of atoms derived from general physical and chemical experience. This situation therefore suggested that the treatment of the stability problems be based directly on the individual character of the atomic processes demonstrated by the discovery of the quantum of action.

A starting point was offered by the empirical regularities exhibited by the optical spectra of the elements, which, as first recognized by Rydberg, could be expressed by the combination principle, according

to which the frequency of any spectral line was represented with extreme accuracy as the difference between two members of a set of terms characteristic for the element. Leaning directly on Einstein's treatment of the photo-effect, it was in fact possible to interpret the combination law as evidence of elementary processes in which the atom under emission or absorption of monochromatic radiation was transferred from one to another of the so-called stationary states of the atom. This view, which permitted the product of Planck's constant and any of the spectral terms to be identified with the binding energy of the electrons in the corresponding stationary state, also offered a simple explanation of the apparently capricious relationship between emission and absorption lines in series spectra, since in the former we are confronted with transitions from an excited state of the atom to some state of lower energy, while in the latter we generally have to do with a transition process from the ground state with the lowest energy to one of the excited states.

Provisionally picturing such states of the electron system as planetary motions obeying Keplerian laws, it was found possible to deduce the Rydberg constant by suitable comparison with Planck's original expression for the energy states of a harmonic oscillator. The intimate relation with Rutherford's atomic model appeared not least in the simple relationship between the spectrum of the hydrogen atom and that of the helium ion, in which one has to do with systems consisting of an electron bound to a nucleus of minute extension and carrying one and two elementary electric charges, respectively. In this connection it is of interest to recall that at the very time of the conference, Moseley was studying the high frequency spectra of the elements by the Laue-Bragg method, and had already discovered the remarkably simple laws which not only allowed the identification of the nuclear charge of any element, but even were to give the first direct indication of the shell-structure of the electronic configuration in the atom responsible for the peculiar periodicity exhibited in Mendeleev's famous table.

III

Owing to the upsetting of international scientific collaboration by the first world war, the Solvay meetings were not resumed until the spring of 1921. The conference, entitled Atoms and Electrons, was opened by Lorentz with a lucid survey of the principles of classical electron theory, which in particular had offered the explanation of

essential features of the Zeeman effect, pointing so directly to electron motions in the atom as the origin of spectra.

As the next speaker, Rutherford gave a detailed account of the numerous phenomena which in the meantime had received such convincing interpretation by his atomic model. Apart from the immediate understanding of essential features of radioactive transformations and of the existence of isotopes which the model provided, the application of quantum theory to the electron binding in the atom had then made considerable progress. Especially the more complete classification of stationary quantum states by the use of invariant action integrals had, in the hands of Sommerfeld and his school, led to an explanation of many details in the structure of spectra and especially of the Stark effect, the discovery of which had so definitely excluded the possibility of tracing the appearance of line spectra to harmonic vibrations of the electrons in the atom.

In the next following years it should indeed be possible through the continued study of high frequency and optical spectra by Siegbahn, Catalan and others, to arrive at a detailed picture of the shell-structure of the electron distribution in the ground state of the atom, which clearly reflected the periodicity features of Mendeleev's table. Such advances implied the clarification of several significant points, like the Pauli principle of mutual exclusion of equivalent quantum states, and the discovery of the intrinsic electron spin involving a departure from central symmetry in the states of electron binding necessary to account for the anomalous Zeeman effect on the basis of the Rutherford atomic model.

While such developments of theoretical conceptions were still to come, reports were given at the conference of recent experimental progress regarding characteristic features of the interaction between radiation and matter. Thus Maurice de Broglie discussed some most interesting effects encountered in his experiments with x-rays, which in particular revealed a relationship between absorption and emission processes reminding of that exhibited by spectra in the optical region. Moreover, Millikan reported about the continuation of his systematic investigations on the photo-electric effect which, as is well known, led to such improvement in the accuracy of the empirical determination of Planck's constant.

A contribution of fundamental importance to the foundation of quantum theory was already during the war given by Einstein, who

showed how the Planck formula of radiation could be simply derived by the same assumptions that had proved fruitful for the explanation of spectral regularities, and had found such striking support in the famous investigations by Franck and Hertz on the excitation of atoms by electron bombardment. Indeed, Einstein's ingenious formulation of general probability laws for the occurrence of the spontaneous radiative transitions between stationary states as well as of radiation induced transitions, and not least his analysis of the conservation of energy and momentum in the emission and absorption processes, was to prove basic for future developments.

At the time of the conference, preliminary progress had been made by the utilization of general arguments to ensure the upholding of thermodynamical principles and the asymptotic approach of the description of the classical physical theories in the limit where the action involved is sufficiently large to permit the neglect of the individual quantum. In the first respect, Ehrenfest had introduced the principle of adiabatic invariance of stationary states. The latter demand had come to expression through the formulation of the so-called correspondence principle, which from the beginning had offered guidance for a qualitative exploration of many different atomic phenomena, and the aim of which was to let a statistical account of the individual quantum processes appear as a rational generalization of the deterministic description of classical physics.

For the occasion I was invited to give a general survey of these recent developments of quantum theory, but as I was prevented by illness from taking part in the conference, Ehrenfest kindly undertook the task of presenting my paper, to which he added a very clear summary of the essential points of the correspondence argument. Through the acute awareness of deficiencies and warm enthusiasm for any even modest advance, characteristic of Ehrenfest's whole attitude, his exposition faithfully reflects the state of flux of our ideas at that time, as well as the feeling of expectation of approaching decisive progress.

IV

How much remained to be done before appropriate methods could be developed for a more comprehensive description of the properties of matter was illustrated by the discussions at the next Solvay conference in 1924, devoted to the problem of metallic conduction. A survey of the procedures by which this problem could be treated on

the principles of classical physics was given by Lorentz, who in a series of famous papers had traced the consequences of the assumption that the electrons in metals behaved like a gas obeying the Maxwell velocity distribution law. In spite of the initial success of such considerations, serious doubts about the adequacy of the underlying assumptions had, however, gradually arisen. These difficulties were further stressed during the discussions at the conference, at which reports on the experimental progress were given by experts as Bridgman, Kamerlingh Onnes, Rosenhain and Hall, and the theoretical aspects of the situation were commented upon especially by Richardson, who also tentatively applied quantum theory on the lines utilized in atomic problems.

Still, at the time of the conference it had become more and more evident that even such limited use of mechanical pictures as was so far retained in the correspondence approach could not be upheld when dealing with more complicated problems. Looking back on those days, it is indeed interesting to recall that various progress, which should be of great importance for the subsequent development, was already initiated. Thus Arthur Compton had in 1923 discovered the change in frequency of x-rays by scattering from free electrons and had himself, as well as Debye, stressed the support which this discovery gave for Einstein's conception of the photon, notwithstanding the increased difficulties of picturing the correlation between the processes of absorption and emission of photons by the electron in the simple manner used for the interpretation of atomic spectra.

Within a year such problems were, however, brought in a new light by Louis de Broglie's pertinent comparison of particle motion and wave propagation, which soon was to find striking confirmation in the experiments by Davisson and Germer and George Thomson on the diffraction of electrons in crystals. I need not at this place remind in detail how de Broglie's original idea in the hands of Schrödinger should prove basic for the establishment of a general wave equation, which by a novel application of the highly developed methods of mathematical physics was to afford such a powerful tool for the elucidation of multifarious atomic problems.

As everyone knows another approach to the fundamental problem of quantum physics had been initiated in 1924 by Kramers, who a month before the conference had succeeded in developing a general theory of dispersion of radiation by atomic systems. The treatment of dispersion had from the beginning been an essential part of the

classical approach to radiation problems, and it is interesting to recall that Lorentz had himself repeatedly called attention to the lack of such guidance in quantum theory. Leaning on correspondence arguments Kramers showed, however, how the dispersion effects could be brought in direct connection with the laws formulated by Einstein for the probabilities of spontaneous and induced individual radiative processes.

It was in fact in the dispersion theory, further developed by Kramers and Heisenberg to include new effects originating in the perturbation of the states of atomic systems produced by electromagnetic fields, that Heisenberg should find a stepping stone for the development of a formalism of quantum mechanics, from which all reference to classical pictures beyond the asymptotic correspondence was completely eliminated. Through the work of Born, Heisenberg and Jordan as well as Dirac this bold and ingenious conception was soon given a general formulation in which the classical kinematic and dynamical variables are replaced by symbolic operators obeying a non-commutative algebra involving Planck's constant.

The relationship between Heisenberg's and Schrödinger's approaches to the problems of quantum theory and the full scope of the interpretation of the formalisms were shortly after most instructively elucidated by Dirac and Jordan with the help of canonical transformations of variables on the lines of Hamilton's original treatment of classical mechanical problems. In particular, such considerations served to clarify the apparent contrast between the superposition principle in wave mechanics and the postulate of the individuality of the elementary quantum processes. Dirac even succeeded in applying such considerations to the problems of electromagnetic fields and, by using as conjugate variables the amplitudes and phases of the constituent harmonic components, developed a quantum theory of radiation, in which Einstein's original photon concept was consistently incorporated. This whole revolutionary development should form the background for the next conference, which was the first of the Solvay meetings I was able to attend.

V

The conference of 1927, the theme of which was Electrons and Photons, was opened by reports by Lawrence Bragg and Arthur Compton about the rich new experimental evidence regarding scat-

tering of high frequency radiation by electrons exhibiting widely different features when firmly bound in crystalline structures of heavy substances and when practically free in atoms of light gases. These reports were followed by most instructive expositions by Louis de Broglie, Born and Heisenberg as well as by Schrödinger about the great advances as regards the consistent formulation of quantum theory, to which I have already alluded.

A main theme for the discussion was the renunciation of pictorial deterministic description implied in the new methods. A particular point was the question, to what extent the wave mechanics indicated possibilities of a less radical departure from ordinary physical description than hitherto envisaged in all attempts at solving the paradoxes to which the discovery of the quantum of action had from the beginning given rise. Still, the essentially statistical character of the interpretation of physical experience by wave pictures was not only evident from Born's successful treatment of collision problems, but the symbolic character of the whole conception appeared perhaps most strikingly in the necessity of replacing ordinary three-dimensional space coordination by a representation of the state of a system containing several particles as a wave function in a configuration space with as many coordinates as the total number of degrees of freedom of the system.

In the course of the discussions the last point was in particular stressed in connection with the great progress already achieved as regards the treatment of systems involving particles of the same mass, charge and spin, revealing in the case of such " identical " particles a limitation of the individuality implied in classical corpuscular concepts. Indications of such novel features as regards electrons were already contained in Pauli's formulation of the exclusion principle, and in connection with the particle concept of radiation quanta Bose had at an even earlier stage called attention to a simple possibility of deriving Planck's formula for temperature radiation by the application of a statistics involving a departure from the way followed by Bolzmann in the counting of complexions of a many-particle system, which had proved so adequate for numerous applications of classical statistical mechanics.

Already in 1926 a decisive contribution to the treatment of atoms with more than one electron had been made by Heisenberg's explanation of the peculiar duplexity of the helium spectrum, which

through many years had remained one of the main obstacles for the quantum theory of atomic constitution. By exploring the symmetry properties of the wave function in configuration space, considerations independently taken up by Dirac and subsequently pursued by Fermi, Heisenberg succeeded in showing that the stationary states of the helium atom fall into two classes, corresponding to two non-combining sets of spectral terms and represented by symmetrical and antisymmetrical spatial wave functions associated with opposite and parallel electron spins, respectively.

I need hardly recall how this remarkable achievement initiated a true avalanche of further progress, and how within a year Heitler and London's analogous treatment of the electronic constitution of the hydrogen molecule gave the first clue to the understanding of nonpolar chemical bonds. Moreover, similar considerations of the proton wave function of the rotating hydrogen molecule led to the assignment of a spin to the proton and thereby to an understanding of the separation between ortho and para states, which, as shown by Dennison, supplied an explanation of the hitherto mysterious anomalies in the specific heat of hydrogen gas at low temperature.

This whole development culminated in the recognition of the existence of two families of particles, now referred to as fermions and bosons. Thus, any state of a system composed of particles with half-integral spin like electrons or protons is to be represented by a wave function which is antisymmetrical in the sense that it changes its sign, when the coordinates of two particles of the same kind are interchanged. Conversely, only symmetrical wave functions come into consideration for photons, to which according to Dirac's theory of radiation the spin one has to be ascribed, and for entities like α-particles without spin.

This situation was soon beautifully illustrated by Mott's explanation of the marked deviations from Rutherford's famous scattering formula, in the case of collisions between identical particles like α-particles and helium nuclei, or protons and hydrogen nuclei. With such applications of the formalism we are indeed not only faced with the inadequacy of orbital pictures, but even with a renunciation of the distinction between the particles involved. Indeed, whenever customary ideas of the individuality of the particles can be upheld by ascertaining their location in separate spatial domains, all application

of Fermi-Dirac and Bose-Einstein statistics is irrelevant in the sense that they lead to the same expression for the probability density of the particles.

Only a few months before the conference Heisenberg had made a most significant contribution to the elucidation of the physical content of quantum mechanics by the formulation of the so-called indeterminacy principle, expressing the reciprocal limitation of the fixation of canonically conjugate variables. This limitation appears not only as an immediate consequence of the commutation relations between such variables, but also directly reflects the interaction between the system under observation and the tools of measurement. The full recognition of the last crucial point involves, however, the question of the scope of unambiguous application of classical physical concepts in accounting for atomic phenomena.

To introduce the discussion on such points, I was asked at the conference to give a report on the epistemological problems confronting us in quantum physics, and took the opportunity to enter upon the question of an appropriate terminology and to stress the viewpoint of complementarity. The main argument was that unambiguous communication of physical evidence demands that the experimental arrangement as well as the recording of the observations be expressed in common language, suitably refined by the vocabulary of classical physics. In all actual experimentation this demand is fulfilled by using as measuring instruments bodies like diaphragms, lenses and photographic plates so large and heavy that, notwithstanding the decisive role of the quantum of action for the stability and properties of such bodies, all quantum effects can be disregarded in the account of their position and motion.

While within the scope of classical physics we are dealing with an idealization, according to which all phenomena can be arbitrarily subdivided, and the interaction between the measuring instruments and the object under observation neglected, or at any rate compensated for it was stressed that such interaction represents in quantum physics an integral part of the phenomena, for which no separate account can be given if the instruments shall serve the purpose of defining the conditions under which the observations are obtained. In this connection it must also be remembered that recording of observations ultimately rests on the production of permanent marks on the measur-

ing instruments, like the spot produced on a photographic plate by impact of a photon or an electron. That such recording involves essentially irreversible physical and chemical processes does not introduce any special intricacy, but rather stresses the element of irreversibility implied in the very concept of observation. The characteristic new feature in quantum physics is merely the restricted divisibility of the phenomena, which for unambiguous description demands a specification of all significant parts of the experimental arrangement.

Since in one and the same arrangement several different individual effects will in general be observed, the recourse to statistics in quantum physics is therefore in principle unavoidable. Moreover, evidence obtained under different conditions and rejecting comprehension in a single picture must, notwithstanding any apparent contrast, be regarded as complementary in the sense that together they exhaust all well defined information about the atomic object. From this point of view, the whole purpose of the formalism of quantum theory is to derive expectations for observations obtained under given experimental conditions. In this connection it was emphasized that the elimination of all contradictions is secured by the mathematical consistency of the formalism, and the exhaustive character of the description within its scope indicated by its adaptability to any imaginable experimental arrangement.

In the very lively discussions on such points, which Lorentz with his openness of mind and balanced attitude managed to conduct in fruitful directions, ambiguities of terminology presented great difficulties for agreement regarding the epistemological problems. This situation was humorously expressed by Ehrenfest who wrote on the blackboard the sentence from the Bible, describing the confusion of languages that disturbed the building of the Babel tower.

The exchanges of views started at the sessions were eagerly continued within smaller groups during the evenings, and to me the opportunity of longer talks with Einstein and Ehrenfest was a most welcome experience. Reluctance to renounce deterministic description in principle was especially expressed by Einstein, who challenged us with arguments suggesting the possibility of taking the interaction between the atomic objects and the measuring instruments more explicitly into account. Although our answers regarding the futility

of this prospect did not convince Einstein, who returned to the problems at the next conference, the discussions were an inspiration further to explore the situation as regards analysis and synthesis in quantum physics and its analogies in other fields of human knowledge, where customary terminology implies attention to the conditions under which experience is gained.

VI

At the meeting of 1930, Langevin presided for the first time, after the demise of Lorentz, and spoke of the loss sustained by the Solvay Institute through the death of Ernest Solvay, by whose initiative and generosity the Institute was created. The President also dwelled on the unique way in which Lorentz had assumed the leading of all previous Solvay meetings and on the vigour with which he had continued his brilliant scientific researches until his last days. The subject of the meeting was the Magnetic Properties of Matter, to the understanding of which Langevin himself had given such important contributions, and the experimental knowledge of which had been so much augmented in those years, especially through the studies of Weiss and his school.

The conference was opened by a report by Sommerfeld on magnetism and spectroscopy, in which he in particular discussed the knowledge of the angular momenta and magnetic moments, which had been derived from the investigations of the electron constitution of atoms, resulting in the explanation of the periodic table. As to the interesting point of the peculiar variation of the magnetic moments within the family of rare earths, van Vleck reported about the latest results and their theoretical interpretation. A report was also given by Fermi on the magnetic moments of atomic nuclei, in which, as first pointed out by Pauli, the origin of the so-called hyperfine structure of spectral lines was to be found.

General surveys of the rapidly increasing experimental evidence about the magnetic properties of matter were given in reports by Cabrera and Weiss, who discussed the equation of state of ferromagnetic materials, comprising the abrupt changes of the properties of such substances at definite temperatures like the Curie point. In spite of earlier attempts at correlating such effects, especially by Weiss' introduction of an interior magnetic field associated with the

ferromagnetic state, a clue to the understanding of the phenomena had first recently been found by Heisenberg's original comparison of the alignment of the electron spins in ferromagnetic substances with the quantum statistics governing the symmetry properties of the wave functions responsible for the chemical bonds in Heitler and London's theory of molecular formation.

At the conference a comprehensive exposition of the theoretical treatment of magnetic phenomena was given in a report by Pauli. With characteristic clearness and emphasis on essentials he also discussed the problems raised by Dirac's ingenious quantum theory of the electron, in which the relativistic wave equation proposed by Klein and Gordon was replaced by a set of first order equations allowing the harmonious incorporation of the intrinsic spin and magnetic moment of the electron. A special point discussed in this connection was the question, how far one can regard such quantities as measurable in the same sense as the electron mass and charge whose definition rests on the analysis of phenomena which can be entirely accounted for in classical terms. Any consistent use of the concept of spin, just as that of the quantum of action itself, refers, however, to phenomena resisting such analysis, and in particular the spin concept is an abstraction permitting a generalized formulation of the conservation of angular momentum. This situation is borne out by the impossibility, discussed in detail in Pauli's report, of measuring the magnetic moment of a free electron.

The prospects which recent development of experimental technique opened for further investigations of magnetic phenomena were at the meeting reported upon by Cotton and Kapitza. While by Kapitza's bold constructions it had become possible to produce magnetic fields of unsurpassed strength within limited spatial extensions and time intervals, the ingenious design by Cotton of huge permanent magnets permitted to obtain fields of a constancy and extension greater than hitherto available. In a complement to Cotton's report, Madame Curie drew special attention to the use of such magnets for the investigations of radioactive processes, which especially through Rosenblum's work should give important new results as regards the fine structure of α-ray spectra.

While the principal theme of the meeting was the phenomena of magnetism, it is interesting to recall that at that time great advances

had also been made in the treatment of other aspects of the properties of matter. Thus many of the difficulties hampering the understanding of electric conduction in metals, so acutely felt in the discussions at the conference in 1924, had in the meantime been overcome. Already in 1928 Sommerfeld had, by replacing the Maxwell velocity distribution of the electrons by a Fermi distribution, obtained most promising results in the elucidation of this problem. As is well known, Bloch succeeded on this basis by appropriate use of wave mechanics in developing a detailed theory of metallic conduction explaining many features, especially regarding the temperature dependence of the phenomena. Still, the theory failed in accounting for the superconductivity, to the understanding of which a clue has been found only in the last years by the development of refined methods for treating interactions in many-body systems. Such methods also seem suitable to account for the remarkable evidence recently obtained about the quantized character of the supercurrents.

A special reminiscence, however, from the meeting in 1930 is connected with the opportunity it gave to resume the discussion of the epistemological problems debated at the conference in 1927. At the occasion Einstein brought up new arguments, by which he tried to circumvent the indeterminacy principle by utilizing the equivalence of energy and mass derived from relativity theory. Thus he suggested that it should be possible to determine the energy of a timed pulse of radiation with unlimited accuracy by the weighing of an apparatus containing a clock connected with a shutter releasing the pulse. However, by closer consideration the apparent paradox found its solution in the influence of a gravitational field on the timing of a clock, by which Einstein himself had early predicted the red-shift in the spectral distribution of light emitted by heavy celestial systems. Still the problem, which most instructively emphasized the necessity in quantum physics of the sharp distinction between objects and measuring instruments, remained for several years a matter of lively controversy, especially in philosophical circles.

It was the last meeting which Einstein attended, before the political developments in Germany forced him to emigrate to the United States. Shortly before the following meeting in 1933 we were all shocked by the news of the untimely death of Ehrenfest, of whose inspiring personality Langevin spoke in moving terms when we were again assembled.

VII

The conference of 1933, especially devoted to the Structure and Properties of Atomic Nuclei, took place at a time when this subject was in a stage of most rapid and eventful development. The meeting was opened by a report by Cockcroft, in which, after briefly referring to the rich evidence about nuclear disintegrations by impact of α-particles obtained in the preceding years by Rutherford and his co-workers, he described in detail the important new results obtained by bombardment of nuclei with protons accelerated to great velocities with appropriate high voltage equipment.

As is well known, Cockcroft and Walton's initial experiments on the production of high speed α-particles by the impact of protons on lithium nuclei gave the first direct verification of Einstein's formula for the general relation between energy and mass which in the following years afforded constant guidance in nuclear research. Moreover, Cockcroft described how closely the measurements of the variations of the cross section for the process with proton velocity confirmed the predictions of wave mechanics, to which Gamow was led in connection with the theory of spontaneous α-decay developed by himself and others. In the report comprising the whole evidence available at that time as regards so-called artificial nuclear disintegrations, Cockcroft also compared the results of the experiments in Cambridge with proton bombardment with those just obtained in Berkeley with deuterons accelerated in the cyclotron newly constructed by Lawrence.

The following discussion was opened by Rutherford who, after giving expression for the great pleasure that the recent development of what he used to call modern alchemy had given him, told about some most interesting new results, which he and Oliphant had just obtained by the bombardment of lithium with protons and deuterons. Indeed, these experiments yielded evidence about the existence of hitherto unknown isotopes of hydrogen and helium with atomic mass 3, the properties of which have in recent years attracted so much attention. Also Lawrence, who in more detail described his cyclotron construction, gave an account of the latest investigations of the Berkeley group.

Another progress of the utmost consequence was Chadwick's discovery of the neutron, which represented so dramatic a development, resulting in the confirmation of Rutherford's anticipation of

a heavy neutral constituent of atomic nuclei. Chadwick's report, beginning with a description of the purposeful search in Cambridge for anomalies in α-ray scattering, ended up by some most pertinent considerations of the part played by the neutron in nuclear structure, as well as of its important role in inducing nuclear transmutations. Before the theoretical aspects of this development were discussed at the conference, the participants had been told about another decisive progress, namely the discovery of so-called artificial radioactivity, produced by controlled nuclear disintegrations.

An account of this discovery, which was made only a few months before the conference, was included in a report by Frédéric Joliot and Irène Curie, containing a survey of many aspects of their fruitful researches, in which processes of β-ray decay with emission of positive as well as negative electrons were ascertained. In the discussion following this report, Blackett told the story of the discovery of the positron by Anderson and himself in cosmic ray researches and its interpretation in terms of Dirac's relativistic electron theory. One was indeed here confronted with the beginning of a new stage in the development of quantum physics, concerned with the creation and annihilation of material particles analogous to the processes of emission and absorption of radiation in which photons are formed and disappear.

As is well known, the starting point for Dirac was his recognition that his relativistically invariant formulation of quantum mechanics applied to electrons included, besides the probabilities of transition processes between ordinary physical states, also expectations of transitions from such states to states of neagtive energy. To avoid such undesired consequences he introduced the ingenious idea of the so-called Dirac sea, in which all states of negative energy are filled up to the full extent reconcilable with the exclusion principle of equivalent stationary states. In this picture the creation of electrons takes place in pairs, of which the one with usual charge is simply lifted out of the sea, while the other with opposite charge is represented by a hole in the sea. This conception was, as is well known, to prepare the idea of antiparticles with opposite charge and reversed magnetic moment relative to the spin axis, proving to be a fundamental property of matter.

At the conference, many features of radioactive processes were discussed, and a most instructive report was given by Gamow on the

interpretation of γ-ray spectra, based on his theory of spontaneous and induced α-ray and proton emission and their relation to the fine structure in α-ray spectra. A special point, which was eagerly discussed, was the problem of continuous β-ray spectra. Especially Ellis' investigations of the thermal effects produced by absorption of the emitted electrons seemed irreconcilable with detailed energy and momentum balance in the β-decay process. Moreover, evidence on the spins of the nuclei involved in the process seemed contradictory to the conservation of angular momentum. It was, in fact, to evade such difficulties that Pauli introduced the bold idea, which should be most fruitful for the later development, that a very penetrating radiation, consisting of particles with vanishing rest mass and spin one-half, the so-called neutrinos, were emitted in β-decay together with the electrons.

The whole question of the structure and stability of atomic nuclei was dealt with in a most weighty report by Heisenberg. From the point of view of the uncertainty principle he had acutely felt the difficulties of assuming the presence of particles as light as electrons within the small spatial extensions of atomic nuclei. He therefore grasped the discovery of the neutron as foundation for the view of considering only neutrons and protons as proper nuclear constituents, and on this basis developed explanations of many properties of nuclei. In particular Heisenberg's conception implied that the phenomenon of β-ray decay be considered as evidence of the creation of positive or negative electrons and neutrinos under release of energy in the accompanying change of a neutron to a proton, or vice versa. In fact, great progress in this direction was soon after the conference achieved by Fermi who on this basis developed a consistent theory of β-decay, which in subsequent developments should prove a most important guidance.

Rutherford, who with usual vigour took part in many of the discussions, was of course a central figure at the Solvay meeting in 1933, which should be the last he had the opportunity to attend before his death in 1937 ended a life-work of a richness with few counterparts in the history of physical science.

VIII

The political events leading to the second world war interrupted for many years the regular succession of the Solvay meetings, which

were only resumed in 1948. In those troubled years, the progress of nuclear physics had not relented and had even resulted in the realization of the possibilities of liberation of the immense energy stored in atomic nuclei. Though the serious implications of this development were in everybody's mind, no mention of them was made at the conference, which dealt with the problem of Elementary Particles, a domain in which new prospects had been opened by the discovery of particles with rest mass between that of the electron and the nucleons. As is well known, the existence of such mesons was already before their detection in cosmic radiation by Anderson in 1937 anticipated by Yukawa as quanta for the short range force fields between the nucleons, differing so essentially from the electromagnetic fields studied in the first approach to quantum physics.

The richness of these new aspects of the particle problem had just before the conference been revealed by the systematic investigations by Powell and his collaborators in Bristol of the tracks in photographic plates exposed to cosmic radiation, and by the study of the effects of high energy nucleon collisions first produced in the giant cyclotron in Berkeley. In fact, it had become clear that such collisions lead directly to the creation of so-called π-mesons which subsequently decay under neutrino emission into μ-mesons. In contrast to the π-mesons, the μ-mesons were found to exhibit no strong coupling to the nucleons and to decay, themselves, into electrons under emission of two neutrinos. At the conference, detailed reports on the new experimental evidence were followed by most interesting comments from many sides on its theoretical interpretation. In spite of promising advances in various directions there was, however, a general understanding that one stood before the beginning of a development where new theoretical viewpoints were needed.

A special point discussed was how to overcome the difficulties connected with the appearance of divergencies in quantum electrodynamics, not least conspicuous in the question of the self-energy of charged particles. Attempts at solving the problem by a reformulation of classical electron theory, fundamental for the correspondence treatment, were clearly frustrated by the dependence of the strength of the singularities on the kind of quantum statistics obeyed by the particle in question. In fact, as first pointed out by Weisskopf, the singularities in quantum electrodynamics were largely reduced in the case of fermions, whereas in the case of bosons the self-energy diverges

even more strongly than in classical electrodynamics, within the frame of which, as was already stressed in the discussions at the conference in 1927, all distinction between different quantum statistics is excluded.

Notwithstanding the radical departure from deterministic pictorial description, with which we are here concerned, basic features of customary ideas of causality are upheld in the correspondence approach by referring the competing individual processes to a simple superposition of wave functions defined within a common space-time-extension. The possibility of such treatment rests, however, as was stressed during the discussions, on the comparatively weak coupling between the particles and the fields expressed by the smallness of the non-dimensional constant $\alpha = e^2/\hbar c$, which permits a distinction with high degree of approximation between the state of a system of electrons and its radiative reaction with an electromagnetic field. As regards quantum electrodynamics, great progress was just at that time initiated by the work of Schwinger and Tomonaga, leading to the so-called renormalization procedure involving corrections of the same order as α, especially conspicuous in the discovery of the Lamb-effect.

The strong coupling between the nucleons and the pion fields prevented, however, adequate application of simple correspondence arguments, and especially the study of collision processes, in which a large number of pions are created, indicated the necessity of a departure from linearity in the fundamental equations and even, as suggested by Heisenberg, the introduction of an elementary length representing the ultimate limit of space-time-coordination itself. From the observational point of view such limitations might be closely related to the restrictions imposed on space-time measurements by the atomic constitution of all apparatus. Of course, far from conflicting with the argument of the impossibility in any well-defined description of physical experience of taking the interaction between the atomic objects under investigation and the tools of observation explicitly into account, such a situation would only give this argumentation sufficient scope for the logical comprehension of further regularities.

The realization of prospects involving, as condition of the consistency of the whole approach, the possibility of the fixation of the constant α, as well as the derivation of other non-dimensional rela-

tions between the masses of elementary particles and coupling constants, was at the time of the conference hardly yet attempted. Meanwhile, however, a way to progress was sought in the study of symmetry relations, and has since been brought to the fore by the rapid succession of discoveries of a manifold of particles exhibiting a behaviour so unexpected that it was even characterized by various degrees of " strangeness ". Thinking of the very latest developments, a great advance has, as is well known, been initiated by the bold suggestion by Lee and Yang in 1957 of the limited scope of the conservation of parity, verified by the beautiful experiments by Mrs Wu and her collaborators. The demonstration of the helicity of the neutrino was indeed anew to raise the old question of a distinction between right and left in the description of natural phenomena. Still, the avoidance of an epistemological paradox in such respect was achieved by the recognition of the relationship between reflection symmetry in space and time and the symmetries between particles and antiparticles.

Of course it is not my intention with such cursory remarks in any way to anticipate problems which will form the main theme for the discussions at the present conference, taking place at a time of new momentous empirical and theoretical advances, about which we are all eager to learn from the participants of the younger generation. Yet we shall often miss the assistance of our deceased colleagues and friends, like Kramers, Pauli and Schrödinger, who all took part in the conference of 1948, which was the last one I, so far, attended. Likewise we deplore the illness that has prevented the presence of Max Born among us.

In concluding, I want to express the hope that this review of some features of the historical development may have given an indication of the debt which the community of physicists owe to the Solvay Institute, and of the expectations which we all share for its future activity.

PHYSICAL ASPECTS OF QUANTUM-FIELD THEORY

by **W. HEITLER**

HISTORICAL INTRODUCTION

Field theory, whether classical or quantum, was handicapped from the very start by one, as must appear now, very profound difficulty. If one pictured the particles producing the field as point particles, the field energy (called self energy) of such a particle turns out to be infinite. This did not prevent one from making numerous sucessful applications, because this self energy could be thought as constant in all processes and therefore could be ignored — a predecessor of the later renormalization theory. Nevertheless, the theory could not be accepted as final. Therefore, Lorentz, Abraham and others have tried to avoid the difficulty by assigning a finite size to the particle. But this idea is immediately faced with another difficulty : the field of a finite sized particle has not the correct properties from the standpoint of the special theory of relativity. The field energy of the moving particle is the space integral $-\int T_{44}\, d^3x$ of a 44-component of a tensor and this is not the fourth component of a vector P_4, as it should be, if the particle plus its field should behave correctly according to special relativity. Attempts made by Poincaré at solving the difficulty by introducing an internal mechanical stress inside the particle are too artificial to be accepted.

There has been one, to some extend sucessful, attempt at creating a Lorentz-invariant and at the same time finite electrodynamics. That is the theory of Born and Infeld ([1]). However, it has not been possible to translate this theory into a quantum theory and therefore, this theory was not developed much further.

The difficulty of reconciling convergence with relativistic covariance has essentially remained unchanged to this day. It reoccurs in quantum theory and constitutes *the* problem, as far as I can see, to be solved in future. In view of this difficulty which is now more than 50 years old, one may well be justified in wondering whether Lorentz-invariance in the present form can be maintained or whether the Lorentz-transformation should not be replaced by some generalization which takes account of the very fact that the " inside " of a particle with finite " size " may not be accessible to a detailed space-time description and that a local Lorentz-transformation may, therefore, not make sense in that " region ". However, no useful attempt in this direction has yet been made and we must leave the question open, but we shall return to it.

Already in the early days of electron theory the idea has been put forward that the self-field of the particle is — via the relativistic energy-mass relation — responsible for the whole mass of the particle. Looking apart from the above relativistic difficulty one obtains, indeed, a plausible value for the " radius " r_0 of the particle, by putting $e^2/r_0 = mc^2$, namely the classical electronic radius, $r_0 = e^2/mc^2$. The advent of quantum theory has made this, however, improbable. The self energy diverges here only logarithmically (for particles with spin 1/2) and this would require an extremely small and improbable radius. The pendulum of the development has then swung in the opposite direction : the self-field of the particle was credited with no contribution to the mass of the particle at all. This was done in the so-called renormalization theory. The great improvement in the mathematical technique for handling quantum field theory has made it possible to obtain — for a *point particle* — an expression for the self energy which has formally the correct relativistic properties. The expression obtained, for a moving particle, is of the form (*),

$$W = \frac{m_0 c^2}{\sqrt{1 - v^2}} \times J \qquad (1)$$

(*) In this formula the velocity v is kept fixed. Usually the momentum is regarded as fixed because it is the momentum which does not change when the interaction with the field is gradually " switched on ", whereas the velocity changes. We can, of course, also compare a particle without field and with field at the same velocity v. Then (1) applies and the momentum is different in the two cases.

m_0 is the original or mechanical mass. J is a certain integral with upper limit ∞, which can be represented *formally* as an invariant. Of course, J itself diverges. If we ignore this fact, in the hope that a later stage of the theory may yield a finite (yet invariant!) value for J, we may put

$$\delta m = m_0 \times \mathrm{J}$$

and regard δm as the electromagnetic contribution to the mass. The total mass is $m_{ex} = m_0 + \delta m$ and this is, of course, the actual mass observed. The theory is then reformulated in such a way that it is expressed in terms of m_{ex} only and neither m_0 nor δm occur explicitly anywhere. For m_{ex}, of course, the experimental value is inserted.

This renormalization procedure is physically sound but it has nothing to do with the fact that δm is infinite. If δm were finite it could be carried out just the same, or we would also be free not to carry it out. But in the latter case we would have to be careful to use the correct numerical value for m_0. It is a fortunate peculiarity of quantum electrodynamics that by carrying it out (together with one or two more of such renormalizations) *all* the unwanted infinites are removed. However, the hope that eventually a finite yet invariant value for J could be found has not materialized so far. No general way has been found yet which would make J finite *and* invariant.

The invariant property of J rests on the fact that it is infinite, i.e. that the particle is a point particle.

In much the same way another infinite quantity occurring in the theory is removed, namely the self charge. The possibility of polarizing the vacuum leads to an infinite self-charge of the particle. This again is invariant only as long as it is infinite. It is removed by saying that the observed elementary charge is the original charge plus the self charge and only this total charge occurs, anywhere.

More interesting even from our point of view is the photon-self energy. In a theory which is both Lorentz and gauge invariant this should be zero. The theory yields a divergent integral which indeed can be made to vanish by a transformation of variables, in accordance with Lorentz- and gauge invariance. This transformation, however, is only possible so long as the integral is divergent. As we shall see, no way of making the quantity convergent

can make the integral zero. We meet here another difficulty similar to the one discussed above : it is equally difficult to reconcile convergence with gauge invariance as it is to reconcile it with Lorentz-invariance. In fact it is easy to prove (see § 4 below) that under some very general conditions (all fulfilled in standard quantum-electrodynamics) the photon self energy is negative definite and *non-zero*, if it is finite at all. So the well known — and very great — success of renormalized quantum electrodynamics rests on the occurrence of infinite quantities. Only so long as they are given by *infinite* expressions can they be removed in a Lorentz- and gauge invariant manner.

The renormalization theory has been criticized from various points of view. First of all, it may be said that a theory which contains infinite quantities to start with is hardly acceptable as final. It is true that the infinities occur primarily in the perturbation expansions. One may well ask if they are not merely the result of unpermissible expansions (*). However, the successful results of quantum electrodynamics also rest on expansions and they should then be criticized on the same grounds. Besides no " exact " treatment has been found which would yield finite values. Secondly, it has been made probable that such a renormalized theory contains severe mathematical inconsistencies such as states with negative probability or a continuous range of masses (" ghost states "), although this has been proved to be the case only for simplified models of field theory. But it is hardly likely that such difficulties can be avoided in quantum electrodynamics. Thirdly, one can point out that not all field theories are renormalizable at all. This applies for example to the theory of β-decay which undoubtedly contains physical truth and has been very successful when used in first approximation. This also applies to the pseudovector coupling of meson theory which also contains a certain measure of truth in the low energy region. At any rate it is closer to the truth than the renormalizable pseudoscalar coupling (§ 2).

And to this we now add a further and more physical argument. In the renormalization theory the self energies which should be

(*) Källén (*Handb. d. Physik*, vol. V, 1958) has claimed to have proved that not all renormalization constants can be finite. His proof has been criticized but I do not wish to take a definite view in this controversy.

self-masses are unobservable, or what comes to the same, are put strictly equal to zero. This may be plausible enough for the electron, because we do not observe the electron without its charge, but there are groups of particles which are identical except for the charge. The discoveries of recent years have shown up quite a number of such groups of particles : the π-mesons, K-mesons, the nucleons, the Σ-hyperons and the Ξ-hyperons. If renormalization theory were strictly exact, the particles in each group should have exactly the same mass. This is not the case. We have every reason to think that the mass differences in each group are self-energies. The calculations reported in section 3 corroborate this view, although it is not yet possible to calculate them reliably with great accuracy. So we have been led back to a modification of the Lorentz-Abraham idea : it is not the masses themselves, but the mass differences in each charge group which are accounted for by self-energies.

The renormalization theory, therefore, is faced with the criticism that a whole group of observable and observed phenomena lies outside its range.

This situation reinforces the necessity of achieving convergence as a *primary* demand to be made. We are then at once confronted with the difficulty of reconciling this convergence with Lorentz-invariance as well as with gauge invariance in the case of electrodynamics. In a later section (§ 4) we shall describe an attempt at such a convergent field theory with a certain claim of extending into the relativistic region. From what was said before, it is not surprising that we find that such a theory is not strictly Lorentz-invariant, nor, in the case of quantum electrodynamics, strictly gauge invariant.

The difficulty of achieving invariance in any finite field theory may suggest that we take a critical attitude towards the a priori postulate we usually make, namely that of exact Lorentz-invariance in the present local sense. We may well imagine that eventually a generalization of the Lorentz- transformation will be required that takes account of something like a " smallest space and time region ". It is perhaps not out of the question that this will exhibit itself at present in a small departure from results derived by the postulate of exact invariance. With this in view the convergent theory mentioned above was developed and we shall confront it

below (§ 4) with the experimental facts. One result may be anticipated. Whilst the mechanical mass m_o is invariant δm turns out not to be so and to depend on v. However, δm is so small and the v-dependence so slight that this does not so far contradict the evidence concerning Einsteins mass-velocity relation. The experiments extant are far from accurate and an improvement and extension to higher energies is highly desirable. An accurate test of the invariance of δm would be very valuable.

The convergent theory mentioned does not, of course, embody any generalization of the Lorentz-transformation in the above sense. On the other hand it has the one advantage, that it can account, at least qualitatively, for the mass differences.

Thus the purpose of this report is to compare, and confront with the evidence, two kinds of field theory which are available at present : the strictly invariant but divergent renormalization theory and a convergent but not strictly invariant theory.

We confine ourselves here to complete quantum field theories. In recent years a great deal of admirable work has been done on *exact* relations, which follow not so much from a field theory but rather from an as small as possible number of assumptions, such as causality and Lorentz-invariance. The dispersion relations are an example. However useful they may be, they do not replace a complete field theory and not all legimitate questions are answered by them. In spite of the hopes placed on them, we must finally insist on a proper field theory. After all, such a field theory exists in the classical limit for electrodynamics, and undoubtedly also to some extent in quantum theory. Any complete quantum field theory must contain the classical theory as a limiting case. Even in meson theory it is likely that a kind of field exists, although its use may be more limited than in electrodynamics. The analysis of electron-nucleon scattering does give some information about the charge *distribution* in a nucleon which means that the field concept has some limited meaning even here. We, therefore, confine this report to field theories only. Problems connected with dispersion relations, etc., are dealt with in other reports.

1. THE RENORMALIZATION THEORY

It is hardly necessary to go into details of the successful applications of the quantum electrodynamics of electrons. They are well known. But some remarks have to be made concerning the probable limitations of this theory and, therefore, we briefly summarize its achievements.

Even before the renormalization technique was developped, this theory could boast of considerable success. When used in the sense of a perturbation expansions radiative processes could be treated, including the damping phenomena. The Compton effect, pair production and annihilation processes, all kinds of line breadth phenomena turned out to be in excellent agrement with the facts, as far as could be ascertained. After the development of the renormalization technique it turned out, as Dyson was the first to show, that no infinities were left in the theory. Therefore, there was no limit in the perturbation expansions where one had to stop and radiative corrections could be treated. Of course, the outstanding success in this direction is the calculation of the Lamb shift and the anomalous magnetic moment of the electron. In view of what follows we must make a few analyzing remarks about these achievements :

We said that the infinities had disappeared from the theory, but not so certain ambiguities. They appear for example in form of finite differences of diverging integrals, and a priori any finite value could be assigned to them. This, for example, is true for the magnetic moment of the electron. A remnant of the divergences occurs even here. The ambiguities cannot be settled on any mathematical ground but they are settled by enforcing once again the correct relativistic behaviour. Although the starting point of the theory is strictly relativistic (and also gauge invariant) its consequences are, owing to the inherent diverging and therefore ambiguous character of the theory, not necessarily so. What is, however, important, is the fact that it is just the repeated insistence on Lorentz-invariance which has yielded the experimentally correct results. The same applies to the photon self energy where not only Lorentz-invariance but also gauge invariance have to be *enforced* before it really vanishes. This perhaps, more than any other evidence, is to be regarded as proof for the correctness of the

Lorentz and gauge groups. Nevertheless, the extreme difficulty (so far impossibility) of reconciling the existence of these groups with convergence may induce us to look for other ways to achieve the same results. More will be said about this later on.

Since we can hardly entertain the idea that renormalized quantum electrodynamics is finally correct, the question arises how a departure from it would affect the above good results. As will be shown below, there are strong arguments in favour of the assumption that such a departure will be connected, quite universally, so with a " cut-off " — distance presumably of the order of $\hbar/K_o \sim \hbar/Mc$, the Compton wave length of the nucleon. The word cut-off is used here, of course, merely to delimit the region where the present theory is more or less correct from a region where we know nothing at all. We wish to formulate our question in two ways :

Let us first suppose that in future some way *will* be found to introduce such a cut-off in a perfectly relativistic and gauge invariant manner. The answer to our question is then very easy. Since K_o or M is a relativistic invariant we must expect that all two particle collision processes will show a departure from the present formulae (probably a diminuation of cross section) when the energy in the centre of mass system passes beyond Mc^2. This would be true for the Compton-effect, the electron-electron scattering, the positron-electron annihilation, etc. So far no experimental data are available in that region but we suggest that such experiments should be made. They would be highly instructive. Pair production and Bremsstrahlung will hardly be affected at all at any energies. This of course, is important in view of the cascade showers which, even at extremely high energies, have exhibited no drastic departure from theory.

Furthermore, it is very easy to estimate the effect of such a cut-off on the magnetic moment and the Lamb-shift. The effect would show in the 7th significant decimal. So far these effects are known up to four or five decimals both experimentally and theoretically. Therefore, no discrepancy with present data is to be expected. It will be very hard to examine the question, because this would require an extension of the theoretical calculations to higher orders still, — an almost impossible task. — Therefore, we conclude that such a theory would not be in contradiction to what is known at present.

However, the supposed theory which combines Lorentz and gauge invariance with convergence does not exist so far. We shall discuss below a non-local *convergent field theory*, but this is neither exactly Lorentz-invariant nor exactly gauge invariant. In this case we must indeed fear to encounter difficulties concerning the magnetic moment of the electron, the photon self energy, etc. However, the theory leaves considerable freedom in the choice of certain parameters without jeopardizing convergence, and this can be used to (i) make the photon self energy vanish (ii) give the correct magnetic moment and Lamb-shift and (iii) be otherwise in no contradiction with facts established so far. The disadvantage of such an approach is its ad hoc character. On the other hand it is so far the only possibility for accounting for the mass differences.

Apart from quantum electrodynamics there exists one more renormalizable field theory which has been applied to physical phenomena and that is meson theory with pseudoscalar meson-nucleon coupling, — in contrast to pseudovector coupling which is not renormalizable. It cannot be said that this theory has met with any success. It is true that there are cases where both couplings lead to identical results in the low orders of perturbation theory (for example the magnetic moment of the nucleons) and in these cases one cannot distinguish between the two couplings. In other cases the results are widely different and in all these cases the experimental evidence is in favour of the non-renormalizable pseudovector coupling. Examples will be given below.

Thus, the conclusions to be drawn from this section are these : renormalized quantum electrodynamics in spite of its success cannot pass as final. Renormalization is not a basic principle of physics, and renormalizability is not a feature that particularly recommends a certain theory. Above all we must look for ways of making the theory finite.

2. THE NON-RELATIVISTIC EXTENDED SOURCE

This is in a sense a counter pole to renormalization theory. It is finite throughout but strictly non-relativistic. Consider an infinitely heavy (and therefore fixed) source, which somehow extends over a finite *space* region, producing a field. We take as an example the nucleon with its meson field. The space distribution is characterized

by a form factor (let it be spherically symmetrical) G(r). Let the extensions be of the order R. For reasons explained below, we use the pseudovector meson-nucleon interaction so that the interaction Hamiltonian is

$$H_{int} = -\frac{\sqrt{4\pi}}{\mu} f \sum_i \int \tau_i(\sigma \operatorname{grad} \varphi_i(\mathbf{r})\, G(|\mathbf{r} - \mathbf{r}_o|)\, d\tau \qquad (2)$$

where $\mathbf{r}_o$ is the position of the nucleon. This is the simplest case of a non-local interaction. G(r) may be thought to be different from unity and decrease for r smaller that the critical length R. After Fouriertransformation G(r) becomes a G(k) and we may choose G(k) such that it decreases for $k > \frac{R}{\hbar} \equiv K_o$. We shall see that K_o is of the order of the nucleon mass Mc. We may, for instance, choose a step function G(k) = 1 or 0 for $k \lesseqgtr K_o$.

In (1) the nucleon is kept fixed at position $\mathbf{r}_o$ and (1) is, therefore, strictly non-relativistic. It can, therefore, only be applied to low energy phenomena but there is enough scope here to test the physical truth or untruth of a theory involving such a form factor. The interaction (1) has in fact been used for the treatment of low energy (i.e. $\varepsilon \lesssim Mc^2$) meson scattering by nucleons, photoproduction of mesons, etc. As is always the case in field theory, some kind of approximation has to be used, and this always empedes a proper judgement of the theory : we are never quite sure whether certain discrepancies are due to a fault of the basic theory or the approximation used. However, if an application is successful, we have good reasons to think that the basic theory contains a measure of truth (otherwise hardly any progress could be made in physics). The approximation used here (for scattering processes) is essentially a one meson approximation, i.e. at some stage of the mathematical procedure states with several mesons are neglected. It is plausible enough that this may be good enough for sufficiently low energies. This does not mean that a straightforward expansion in the coupling constant f is made. The latter procedure, when applied in first approximation to the reaction matrix K [so that the S-matrix is $S = (1 - i\pi K)/(1 + i\pi K)$], does give a crude qualitative approximation to the correct cross sections, but it is quantitatively too far off to be satisfactory. So certain parts of the higher orders of K,

involving virtual particles, are used and it is here, where the form factor comes into action, or in other words, a cut-off is introduced. In particular all higher orders are used for the renormalization of the coupling constant. As was mentioned above, this is not a physical necessity, but a matter of mathematical convenience. It is introduced in a natural way as follows : let ψ_o be the wave function of a bare nucleon (without interaction with mesons) and let ψ be the wave function of the same nucleon in its true physical state which includes, in the language of non-interacting particles, the meson cloud. Then it is a fact that the matrix elements of $\sigma_i \tau_k$ between two states ψ, ψ' are related by

$$Z<\psi_o|\sigma_i \tau_k|\psi'_o> = <\psi|\sigma_i \tau_k|\psi'> \tag{3}$$

where Z is a numerical factor. As in (2) $\sigma_i \tau_k$ occurs in connexion with f we put

$$f Z = f_r \tag{4}$$

and call f_r the renormalized coupling constant. The comparison with the experiments leads to a fairly small value

$$f_r^2 = 0.08 \tag{4'}$$

On the other hand f is much larger, probably in the neighbourhood of 0.2 (unpublished calculations). Thus, after condensing certain higher order effects in f_r, the use of some kind of expansion is more justifiable than if f where used.

The results of such a theory are well known and a brief summary may suffice. When the source is kept fixed, the interaction (1) gives rise to p-wave scattering only. After decomposition into eigenstates of the nucleon-meson system, 4 states are responsible for the scattering, with angular momentum 3/2 and 1/2 and isobaric spin 3/2 and 1/2. By far the strongest contribution to the scattering is due to the 3/2, 3/2 states and it is here where the theory has undoubtedly been successful. It has been possible to account for the sphase shift $\delta_{\frac{3}{2}\frac{3}{2}}$ up to energies of 200-300 Mev in rather close agreement with the observations. Only two parameters are available for adjustment, namely f_r^2 (given by (3)) and the cut-off-constant K_o which turned out to be of the order

$$K_o \sim Mc \tag{4}$$

In particular the resonance (where $\delta_{\frac{3}{2}\frac{3}{2}}$ exceeds the value $\pi/2$) is well represented by the theory.

The theory has been less successful in explaining the phases $\delta_{\frac{3}{2}\frac{1}{2}} = \delta_{\frac{1}{2}\frac{3}{2}}$ and $\delta_{\frac{1}{2}\frac{1}{2}}$ and of course, also for the *s*-scattering. The only " success " here lies in the fact that these phases turn out to be small, in agreement with the observations. The failure in this respect may be due to several causes : (i) the approximations used may not be good enough for the " small effects ", (ii) the strictly non relativistic model may not be good enough (this certainly applies to *s*-scattering) or (iii) the simple (linear) form of the meson-nucleon interaction may not be sufficient (or there may be further interactions, for example a direct π-π-interaction).

However that may be, it is very probable that the interaction (2) *with* the cut-off contains a certain amount of truth.

We have used the non-renormalizable pseudovector interaction, and it is this interaction which has been, partially, successful. It is often stated that for *p*-scattering this interaction is equivalent with pseudoscalar interaction, and that, therefore, for this purpose the renormalizable interaction can be used. However, this is not a correct argument. The pseudoscalar interaction leads to a very large and dominant *s*-scattering, in addition to the *p*-scattering (which is the same for both couplings), in contradiction to the facts. The separation of *s*- and *p*-scattering is an artificial device made for convenience and not an inherent feature of the theory. The results of a given theory must be taken as a whole. In this sense it is the p.v.-coupling only which has been successful. This, of course, is an additional strong argument for the necessity of the introduction of a form factor.

3. THE MASS DIFFERENCES

The most obvious piece of evidence pointing in the direction of finite sized particles is the fact that the masses of particles having identical strong interactions but differing by their charge are not the same but differ by a small fraction of their own mass. It is almost evident that these mass differences are nothing but self energies.

The simplest and most easily explained case is the π-meson. The mass of $\pi\pm$ is larger by about 10 electron masses than that of the π^o. We can straightaway explain this as an electromagnetic self-energy. The latter is indeed positive. We can regard the meson to be at rest, and in this case we can use an easy generalization of the extended source model (§ 2). Of course, the treatment has to be relativistic. In first approximation of perturbation theory the self-energy is due to virtual photons emitted and reabsorbed by the π-meson. The latter receives a recoil. The form factor is now chosen such that it cuts off the momenta $\vec{k}$ of the virtual photons at energies $|\vec{k}| < K_o$. This means that we cut off in the *centre of mass system* in a spherically symmetrical way. Identifying the self energy W thus obtained with the change of mass

$$W = \delta m c^2$$

we have an unambiguous prescription for the calculation of δm, which is obviously a generalization of the extended source model into the relativistic region. If K_o is chosen an *invariant* quantity — which we are free to do — then we even obtain an invariant expression for δm. This does not mean that we are already in possession of a general theory embodying a form factor which would lead to a general covariant finite expression for the self energy of a moving particle. In fact, this is not the case as will be explained in § 4. Our simple prescription is rather ad hoc and confined to the first approximation of perturbation theory. At any rate the prescription is good enough as a preliminary.

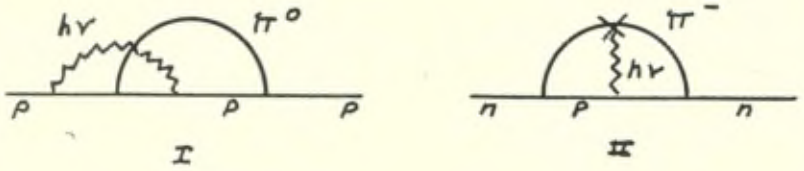
We can choose K_o so that the observed mass difference is obtained and we find

$$K_o \sim 0.65\ \mathrm{Mc} \qquad (5)$$

The relatively large value of δm is due to the quadratic dependence $\sim K_o^2$. The order of magnitude of $K_o \sim$ Mc is the same as in the case of the meson-nucleon scattering (§ 2). The fact that two quite different interactions, the meson-nucleon interaction and meson-electromagnetic interaction, are cut off at about the same value, namely at the Compton wave length of the heaviest particle, the nucleon, is surely of profound significance. The Compton wave length of the nucleon is probably the smallest length measurable and it is quite likely that the " inside " of a particle, the region with space dimensions less than $\hbar$/Mc escapes a detailed space-time

description using present concepts. We may suppose that K_o has universal significance.

Let us now consider other charge groups of particles. In the next example, the nucleon ([3]), we meet already a difficulty. The neutron is heavier than the proton, and the mass difference cannot be due to a purely electromagnetic self energy (which is positive also here). It cannot be due to a purely mesonic interaction either, because charge symmetry yields the same values for the proton and neutron. The hope lies in the mixed mesonic-electromagnetic interaction. In the language of perturbation theory it would be an effect of order e^2f^2, or higher powers, especially in f. This effect comprises the electromagnetic interaction of the meson cloud (electrostatic and magnetic) with the bare nucleon, but other effects as well. As an illustration we show two Feynman diagrams representing the effect (there exist a large number of diagrams even in order e^2f^2) :



The diagrams, of course, are not the same for the proton and neutron. Calculations have been performed to the lowest order of perturbation theory on a strictly field theoretical basis. Even in this order the calculations are hopelessly complicated and further drastic approximations had to be made, — an expansion in powers 1/M, where M is the nucleon mass. As in the case of the π-meson self energy, we may assume the nucleon to be at rest. The momenta of the virtual particles, meson and photon, are then restricted through the form factor to values $|\vec{k}|<K_o$, and we shall, of course, again assume that $K_o \sim Mc$. The approximation mentioned really boils down to assuming that $|\vec{k}| \ll K_o$. This is certainly a crude procedure from which we cannot expect very accurate results, but it is feasable, and at any rate gives an indication as to whether the *p-n* mass difference can be explained or not in this way. The mass difference obtained is of order 1/M.

A further difficulty occurs in the theory. In the vertices of diagram I the virtual lines are joined to the nucleon which is at rest in the beginning, and remains nearly so during the virtual process, because the virtual momenta are all less than Mc. Here we can, with confidence, use the same procedure as for the π-meson, a spherically symmetrical cut off $|\vec{k}|<K_o$, etc. In diagram II, however, a vertex occurs, marked x which is truly relativistic. The virtual π-meson as well as the photon have momenta up to $\sim$ Mc and there is no way of bringing the π-meson to rest. We are, therefore, forced to use a relativistic theory embodying a form factor. Needless to say that a covariant theory of this kind does not exist so far. What has been done here will be described in the following section. It is a theory which is convergent throughout and may have some claim of extending into the relativistic region but it is not strictly covariant. The effect of the form factor in the vertex x of diagram II is, therefore, more of the nature of a guess than a reliable theoretical result. However, the convergence of the result, does not depend on this, nor does the sign, and not even the numerical value is very much affected.

When pseudoscalar coupling is used, the result is of the order of 1/100 electron mass and, therefore, far too small. This is a further argument in favour of the non-renormalizable pseudovector coupling. For p-v-coupling the result is the following :

$$\left(\delta M_p - \delta M_n\right)_{e^2 f^2} = C f^2 \left[-1 + 0.65\,\bar{\rho}\right]\left(\frac{K_o}{Mc}\right)^3 \qquad (6)$$

$$C \sim 10 \text{ electron masses}$$

The first term (— 1) in the bracket arises from diagrams of type I, the second term + 0.65 from type II. $\bar{\rho}$ describes the effect of the form factor in the vertex x. The theory mentioned above yields $\bar{\rho} = 0.6$. At any rate $\bar{\rho}$ must lie between 0 and 1, but both extremes are unlikely. So the value of the bracket [] does not vary to more than a factor 2 or so. A further uncertainty lies in the value to be taken for f. When perturbation theory is used, one may be in doubt whether to use the renormalized or unrenormalized coupling constant. The latter is much larger than $f_r^2 = 0.08$.

Using f_r^2 and $K_o = M$, we obtain $\left(\delta M_p - \delta M_n\right)_{e^2 f^2} \sim -\frac{1}{2} m_{el}$. The sign and the order of magnitude are correct. But the numerical value is too small. The effect should, of course, be large enough

to overcompensate the purely electromagnetic self energy of the proton which is about $+ 4\, m_{el}$ (it depends on K_o like log. K_o). If we use $f^2 = 0.2$ instead of f_r^2 we obtain $\sim -1\, m_{el}$ which is better but still too small. There may be several causes for the small value obtained. (i) K_o is, of course, only known in order of magnitude, and if we would raise its value only by a factor 2 or so, ($K_o \sim 2$ Mc) we can easily get the correct value. But then, of course, our expansion $\sim 1/M$ would no longer be justifiable. (ii) It is very probable that effects $1/M^2$, etc. contribute with the same sign, as a closer inspection shows (see below). It is quite likely that even in $e^2 f^2$-approximation, a much larger value would result if we would not use the crude 1/M expansion. (iii) Finally it is not to be expected that the $e^2 f^2$ term alone will suffice. But so far no prospect exists for a more rigorous treatment not involving expansions in f^2.

Taking all this into account, the result is certainly encouraging enough to justify the statement, that the *p-n* mass difference can be understood as an effect of self-energies resulting from a convergent field theory, although up to now no quantitatively correct treatment has been given.

It is interesting to compare this field theoretical treatment with different attempts made to understand the *p-n*-mass difference. Feynman and Speisman (2) have calculated the electromagnetic self energy of a nucleon described by a charge and the correct magnetic moment (Pauli term), both extended through a cut-off of order $\sim$ Mc. They could obtain, by suitable adjustments, the correct value. The magnetic moment is, of course, a mesonic effect. A comparison of the two methods shows that the field theoretical treatment (given here in first approximation) does not reduce in any way to such a phenomenological description in terms of charge and magnetic moment clouds. There are further contributions (*) and besides, the meson cloud is frequency dependent and does not contribute to our effect in the same way as to the static charge and magnetic moment cloud. The Feynman-Speisman effect turns out (when analyzed) to be of order $1/M^2$ and the order 1/M considered here is not contained in it at all. This, however, may be an indication that the terms $1/M^2$ (and perhaps higher) contribute with

(*) For example, diagram II would not occur in the phenomenological treatment.

the same sign and order of magnitude. Later attempts (4) have been based on the charge and current distribution as obtained from electron-nucleon scattering (with or without the use of dispersion relations; the latter are quite irrelevant in this connexion). The calculation leads to the wrong sign. Also here the field theoretical treatment is quite at variance with such semi-phenomenological treatments. Field theory does not reduce merely to charge and current distributions.

Finally let us consider the mass differences of the K-mesons and the Σ-hyperons (5). We are here even on less safe grounds because the interactions of these particles are less well known than for π-mesons. For the treatment of the K-mesons interactions similar to those for π-mesons are used, namely

$$K \rightarrow n + \text{hyperon } (\Lambda \text{ or } \Sigma)$$

The coupling chosen is again of the p-v-type but with a coupling constant $f_1^2 = 1/5 f^2$. Otherwise the treatment is quite similar to that for the *p-n*-system. The mass of the K-meson is taken to be of the order of M rather than small compared with M. The result is for $K_o = Mc$

$$\left(\delta\mu_{k^+} - \delta\mu_{k^o}\right) \underset{e^2 f_1^2}{\sim} -\left(1 \text{ or } 2\right) m_{el}$$

Again the sign and order of magnitude are correct but the numerical value too small. The electromagnetic self-energy (which makes the $K^{\pm}$ heavier) is about 3 m_{el} and the $e^2 f_1^2$-effect does not overcompensate it. The fact that the numerical value is too small is probably due to the same causes as for the nucleons. The correct value could be obtained for $K_o \sim 1{,}5$ Mc, or so.

If we assume, as we may well do, that the $e^2 f^2$ effects (and also the $e^2 f^4$, etc., of course) in reality overcompensate the e^2 effects for the nucleon and the K-mesons, one may well ask, why this is not so for π-mesons, where the $\pi^{\pm}$ is indeed heavier than π^o. This is easily explained. Calculations of the $e^2 f^2$ effect for π-mesons show that this does not depend much on the mass of the π-meson. It is much the same as for the K's. On the other hand, the electromagnetic self energy strongly depends on μ, it is proportional to $1/\mu$. Whereas this effect is $\sim 10\, m_{el}$ for the π's, it is only $3\, m_{el}$ for the K's. It is, therefore, understandable that the effects considered here $\sim e^2 f^2$, etc. overcompensate the e^2-effect for the heavy K-particles but not for the π's.

For the Σ-hyperons three interactions contribute. (i) The purely electromagnetic (ii) the interaction with π-mesons ($\Sigma \to \Sigma + \pi$ or $\Sigma \to \Lambda + \pi$) and (iii) with K-mesons. ($\Sigma \to$ nucl. + K or Ξ + K). (i), of course, tends to make $\Sigma^{\pm}$ heavier than Σ^{0}, (ii) tends to make Σ^{0} heavier than $\Sigma^{\pm}$ but does not lead to a splitting of Σ^{+} and Σ^{-}. The mass difference of Σ^{+} and Σ^{-} is due to (iii) only. The calculations are based on the Gell-Mann scheme of interaction. The result is (with the same approximations as before) that Σ^{-} is indeed heavier than Σ^{+} (as is true experimentally), but the mass difference $M_{\Sigma^-} - M_{\Sigma^+}$ is far too small (by a factor 20 at least). In view of the better results obtained for the nucleons, π's and K's, it seems more likely that in this case the numerical failure is due to our insufficient knowledge of the interaction (perhaps the Gell-Mann scheme is incorrect or there is a further unknown interaction) rather than to the poor approximations used or to a fundamental fault of theory.

Taking the evidence of this section as a whole it is safe to say that self energies are finite and observable quantities. The renormalization theories cannot, therefore, be correct, they ignore this class of important phenomena. At best, and this refers to the electrodynamics, they are a very good approximation to the truth, offering a very accurate (but certainly not exact) treatment of a large class but not of all phenomena.

4. A CONVERGENT FIELD THEORY (ref. (6) - (9))

We are thus taken back to the old problem of making quantum field theory convergent, — at all costs. The attempt we are going to discuss now is designed to achieve convergence in *all* orders of perturbation theory, whereas exact Lorentz-invariance as well as gauge invariance is relegated to second place in the sense that we examine the possibility of invariance or the effects of a lack of invariance after convergence has been achieved. We let ourselves be guided by the one convergent theory we have, that of § 2. Non-local field theories which start from exact Lorentz-invariance have already been studied in detail, and in all cases it has turned out, that the divergencies reappear in higher approximations. They are, therefore, useless (gauge invariance is not fulfilled either). The theory to be discussed is a generalization of the non-relativistic

extended source model into the relativistic region. As we shall see, convergence *can* be achieved and we shall state the general conditions for exact Lorentz- and gauge invariance. If we discuss here a particular non-local theory, the form factor is, of course, merely thought to replace an unknown region. It is not the idea that such a theory could be final. The theory which follows turns out to be convergent, but it is certainly not the only way to achieve convergence. Our purpose is merely to show that convergent quantum field theories do exist, and on the basis of such a theory, to try to delimit the region of validity of the present theory.

We work in interaction representation. The Fermi field ψ as well as the Bose field φ are assumed to satisfy the usual commutation relations of free fields. The Hamiltonian of the free fields H_0 is also assumed to remain unchanged. Therefore the *free fields are Lorentz-covariant exactly*. What is changed is the interaction H_{int}. This interaction Hamiltonian is generalized to include a form factor

$$\mathscr{H}_{int} = g \int \bar{\psi}(x') \, F(x-x', x-x'', x-x''') \, \psi(x'') \, \varphi(x''') \, d^4(x'..x''') \quad (7)$$

$$H_{int} = \int \mathscr{H}_{int} \, d^3x$$

In the special case of quantum electrodynamics

$$\mathscr{H}_{int} = -ie \int \bar{\psi}(x') \, \gamma_\mu \, F(x-x' \ ...) \, \psi(x'') \, A_\mu(x''') \, d^4(x'...x''') \quad (8)$$

F is assumed to be independent of the field operators but it may well contain Dirac γ's. It is translational invariant, but otherwise it is so far quite open, and we do not insist that it shall be a relativistic invariant, although we may well choose it to be so. The only condition to be imposed on F is a normalization condition (*)

$$\int F(x-x', x-x'', x-x''') \, d^4x'' d^4x = \delta^4(x'-x'') \quad (9)$$

The Schrödinger equation is, as usual

$$i \dot{\Psi} = H_{int} \, \Psi \quad (10)$$

So far we have assumed that H_{int} is linear in φ or A_μ but there is no reason to restrict ourselves to this simple form. In fact we shall be forced (in order to avoid discrepancies with experimental facts) to add a bilinear term in A_μ, but let us start with (7) or (8).

We do not wish to enter into a detailed discussion of the formalism ensuing from (7), (8). We refer the reader to the publications. Only a few points may be stated.

(*) After Fourier transformation F becomes $f(p, q, k)$ and this condition becomes $f(p, p, 0) = 1$. It merely means that f is different from unity only when a real interaction takes place, i.e. in vertices; the propagators are unchanged.

It is easy to see that the usual total charge

$$Q(t) = \int d^3x \, \bar{\psi}(x) \, \gamma_4 \, \psi(x)$$

(if φ is a neutral field) remains an integral and is conserved.

In general $\mathscr{H}_{int}$ in two points with space like connexion will no longer commute. It is, therefore, not possible to generalize (12) into the Tomonaga equation (referring to particular space-time points) but the transition to a " restricted Tomonaga equation " is possible. Let n_μ be the normal vector of a space like *plane*, and let τ be a parameter numbering these planes (τ replaces the time). We define then

$$H_{int}(\tau, n) = \int d^4x \, \delta(nx + \tau) \, \mathscr{H}_{int}(x) \tag{11}$$

and assume

$$i \frac{\partial \Psi(n, \tau)}{\partial \tau} = H_{int}(\tau, n) \, \Psi(\tau, n) \tag{12}$$

This equation can be stated to be relativistically invariant, in the following sense : if x and n are transformed by a Lorentz-transformation, a corresponding transformation of the state vector Ψ exists, such that (12) is valid again. The covariance of the Schrödinger equation alone, however, is not of much value. It does not, for instance, imply that the S-matrix is invariant. For this a much stronger condition is required, namely (up to second order) :

$$\int d^4x \, d^4y \, [\mathscr{H}(x) \, \mathscr{H}(y)] \, (n_\nu x_\lambda - n_\lambda x_\nu) \, \delta(n(x\text{-}y)) = 0 \tag{13}$$

Of course, the vanishing of the commutator on a space like surface ($n(x\text{-}y) = 0$ means that x and y lie on a space like surface) is sufficient to fulfil (13), but (13) is weaker. So far no form factor different from the local one $F = \delta^4(x\text{-}x') \ldots \delta^4(x\text{-}x'')$ is known for which (13) is fulfilled exactly.

The situation with regard to gauge invariance in electrodynamics is very similar : the Schrödinger equation can be stated to be invariant, if during a gauge transformation Ψ is suitably transformed but the invariance of the S-matrix requires the fulfillment of a strong condition. We need not go into details because a simple example will show below that at any rate Lorentz- *and* gauge invariance cannot be fulfilled simultaneously for any form factor differing from the local case (i.e. $F = \delta^4(x\text{-}x') \, \delta^4(x\text{-}x'') \, \delta^4(x\text{-}x''')$).

What are now the main physical features of such a theory ? The most outstanding property of this theory is the fact that it offers the possibility of general convergence in all orders. This has been proved for a specific simple form factor. We simplify F by putting

$$F(x\text{-}x', x\text{-}x'', x\text{-}x''') = G(x\text{-}x', x\text{-}x'')\ \delta^4(x\text{-}x''') \tag{14}$$

So H_{int} is only partially non-local but it seems that not much is lost by the specialisation (14). We consider then the Fourier-transform of G viz. $g(p, q)$. For $g(p, q)$ we choose

$$g(p, q) = \frac{\lambda^4}{\lambda^4 + p^2q^2 - (pq)^2} \tag{15}$$

(15) can also be raised to any power if necessary. λ is an invariant cut-off constant, so G or g is relativistically invariant. (15) reduces to the cut-off of the non-relativistic extended source in the case of a particle at rest $\vec{p} = 0$, $p_o = m$, emitting virtual Bose-particles with momentum $\vec{k}$, k_o. In this case $\vec{q} = -\vec{k}$ and (15) reduces to

$$g = \frac{\lambda^4}{\lambda^4 + m^2 |\vec{k}|^2} \tag{16}$$

which means that $\vec{k}$ is cut off at $|\vec{k}| < \lambda^2/m$, which quantity is then to be identified with the previous K_o. With (15) it has been proved that the S-matrix of quantum electrodynamics is *convergent in all orders of the power expansion*, but there is little doubt that the same is true in meson theory with p-v-coupling. Apart from the strictly non-relativistic extended source model this is, so far, the only convergent quantum field theory. There are undoubtedly numerous other types of form factors for which the theory is also convergent.

It is interesting to study $G(x\text{-}x', x\text{-}x'')$ following from (15) in coordinate space. At first sight G extends over the whole space-time region. As soon, however, as one averages G over a small region of $\vec{x}''$ and x_o'', of order λ^2/m, — and it is in the spirit of this theory that one should do so —, $\overline{G}$ reduces to a smeared out δ^4-function : $\overline{G}$ is different from zero only when $\vec{x}\text{-}\vec{x}'$ as well as $x_o\text{-}x_o'$ are small.

However, the price for the advantage of convergence is high. So long as we stick to the above formalism, it is easy to prove that

Lorentz- and gauge invariance cannot be both fulfilled together for any form factor that produces convergence. For this purpose we consider the photon self-energy. We make the following assumptions. (i) H_{int} is hermitian. (ii) H_{int} is linear in A_μ. (iii) The theory is convergent. (iv) The vacuum is stable and the lowest energy state. These are all fulfilled in the above theory. It is then easily proved that the photon self energy in second order

$$W_{ph} = \sum_i \frac{H_{int}\, o/i\; H_{int}\, i/o}{(E_o - E_i)} < 0 \; (\neq 0) \tag{17}$$

The proof is easiest when elementary perturbation is used which, of course, is permissible in a convergent theory. If Lorentz- and gauge invariance were fulfilled W would vanish. The vanishing of W_{ph} in the local renormalized theory rests essentially on the fact that it is really *divergent* and therefore ambiguous.

Also for the anomalous magnetic moment of the electron the correct value is only obtained from the local theory by an enforcement of the invariance principles. The above theory does not lead to the correct value of the magnetic moment either.

If we ask now what can be done in order to remedy the contradictions with the experimental facts ensuing from the lack of Lorentz- and gauge invariance without giving up convergence, it seems that the easiest way is to give up condition (ii) above. There is no reason why H_{int} should be linear in A_μ as was so far supposed in (8). We, therefore, add to $\overset{(1)}{H}_{int}$ a bilinear term

$$H_{int} = \overset{(1)}{H}_{int} + \overset{(2)}{H}_{int} \tag{18}$$

$$\mathscr{H}^2_{int} = e^2 \int f_{\mu\nu}(x\text{-}x') \left[A_\mu(x)\, A_\nu(x') - \langle A_\mu(x)\, A_\nu(x') \rangle_{vac}\right] d^4x' \tag{19}$$

where $f_{\mu\nu}$ is to be determined suitably. Of course, $f_{\mu\nu}$ must vanish in the local limit. One can now determine $f_{\mu\nu}$ so that :

(i) The photon self energy is zero.

(ii) The anomalous magnetic moment of the electron is correct.

(iii) The Lamb shift is correct.

The disadvantage of this procedure is that it is completely ad hoc and, therefore, very unsatisfactory. The convergence of the theory is not endangered by the addition of $\overset{(2)}{H}_{int}$.

The convergent theory, on the other hand, with all its faults, can be used to calculate self energies and, therefore, at least permits a treatment of mass differences. In fact it was the form factor (15) which has been used in the calculations of § 3.

It is interesting to see what we obtain for the " self mass " of a particle from such a non-invariant theory. For an electron nearly at rest ($|\vec{p}| \ll mc$) the result is

$$\delta m = \delta m(0)\,(1 - \frac{1}{72}\frac{p^2}{m^2} + \ldots) \qquad (20)$$

$m(0)$ is the value for $p = 0$. Of course, δm is no longer invariant. The factor of p^2/m^2 in (20) depends on the choice of the form factor. If we take (20) for a moment seriously, Einstein's formula for the inert mass would no longer be quite correct. It would now read $m = (m_o + \delta m)/\sqrt{1 - v^2}$ where δm is given by (20). Of course, $\delta m(o)$ is always a small fraction of $m_o (\sim 3\,\%)$. It is astonishing that such a departure from Einstein's formula would not be in contradiction with experiments extant. The experiments referring to the mass-velocity relation are very far from accurate, (as Janossy [12] has recently pointed out), and if it was not for the quantum electrodynamical effects (magnetic moment, etc.) the experimental evidence for exact Lorentz-invariance would be very weak indeed.

A further interesting result of such a theory embodying a form factor concerns the nucleon-nucleon forces. It has been known for some time from scattering data that the nucleon forces have a " hard core " i.e. become repulsive at small distances. The elementary non-relativistic treatment according to the local theory leads to the well known Yukawa potential plus a repulsive δ-function at distance zero. The latter has usually been ignored as being irrelevant. The form factor has the effect that this δ-function is spread out over a distance of order $\hbar/K_o = \hbar/Mc$ and thus gives rise to the hard core. This again is a qualitative indication for the existence of something like a form factor. ([11])

CONCLUSIONS

It cannot be said that we are at present in possession of a workable quantum field theory. We have the choice between two extremes : (i) We have the strictly local and, therefore, divergent, but Lorentz-

and gauge invariant renormalized theory. Quantum electrodynamics with its outstanding success in the treatment of collision processes and static effects like Lamb-shift, belongs here but no other field theory of this category can claim much success. This type of theory, however, ignores the mass differences and, therefore, excludes the treatment of a class of important phenomena. (ii) We can establish convergent field theories, — the one discussed in § 4 is certainly not the only possibility — which at least permits a treatment of all phenomena. With the help of ad hoc ammendments we can even avoid discrepancies with facts known at present. So far no way is known of combining convergence with exact Lorentz- and gauge invariance. Although no proof exists, — it does look as if the two demands, convergence and exact invariance, were incompatible, on the basis of present concepts, that is to say on the basis of a space-time continuum, in which Lorentz-transformations can be carried out, and of the basic concepts of quantum mechanics. It was the purpose of the work in § 4 to find a limit for the validity of the local theory. Since the latter is invariant one might have thought that such a limit could be established in an invariant manner. But even that does not seem to be possible. It is likely that all of the above mentioned concepts, including the Lorentz-transformation, will have to undergo changes and generalizations before the problem of quantum field theory can be finally solved.

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THE PRESENT STATUS OF QUANTUM ELECTRODYNAMICS

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Fifty years ago at this Conference one of the problems most energetically discussed was the apparent quantum nature of the interaction of light and matter. It is a privilege to be able, after half a century, to give a report on the progress that has been made in its solution. No problem can be solved without it dragging in its wake new problems to be solved. But the incompleteness of our present view of quantum electrodynamics, although presenting us with the most interesting challenges, should not blind us to the enormous progress that has been made. With the exception of gravitation and radioactivity, all of the phenomena known to physicists and chemists in 1911 have their ultimate explanation in the laws of quantum electrodynamics.

Stricktly speaking, " quantum electrodynamics " might be expected to deal only with the quantum theory of the electromagnetic field, and not with the theory of the motion of the matter which generates it or reacts to it. But conventionally, the motion of that matter whose motion is understood, namely electrons and possibly muons, is included, while the motion of baryons and mesons is not. I will use the term in the conventional sense here. If I wish to refer to the narrower field I will call it simply the " quantum theory of the electromagnetic field ".

Lorentz [1] showed in his 1911 report at this conference that beside an instantaneous coulomb interaction the electromagnetic field could be represented as a set of harmonic oscillators, each

driven by the transverse component of the current produced by matter in the corresponding mode. That the quantum theory of electromagnetic interaction results directly from the simple assumption that these oscillators are quantum oscillators obeying a Schrodinger equation was noted by Dirac [2]. Since that time a bewildering variety of mathematically equivalent formulations of that idea have been made. These are published [3] in many articles and text books and I will assume that you are familiar with some of them, and will not discuss them further. Here I shall simply report first on the comparison of quantum electrodynamic calculations with experiment, and second on some of the unanswered theoretical questions in this field.

COMPARISON TO EXPERIMENT

General remarks.

Considerable evidence for the general validity of Q.E.D. is, of course, provided by the enormous variety of ordinary phenomena which, under rough calculation, are seen to be consistent with it. The superfluidity of helium and the superconductivity of metals having recently been explained, there are to my knowledge no phenomena occuring under known conditions, where quantum electrodynamics should provide an explanation, and where at least a qualitative explanation in these terms has not been found. The search for discrepancies has turned from looking for gross deviations in complex situations to looking either for large discrepancies at very high energies, or by looking for tiny deviations from the theory in very simple, but very accurately measured situations.

High energy experiments.

The experiments at high energy which are most significant for us here are those of the elastic scattering of energetic electrons (up to 1 Gev) by protons at appreciable angles [4]. The scattering is very different from what it would be for an unstructured proton. The proton should have some structure, however, as a result of the unknown strong interactions between mesons and baryons. One usually interprets all the deviation as due to this structure. On the other hand some of it may be a failure of quantum electrodynamics.

According to quantum electrodynamics the scattering amplitude should be

$$(\bar{u}_2 \gamma_\mu u_1) J_\mu \frac{1}{q^2} \tag{1}$$

where q is the momentum transferred by the virtual photon, u_1 and u_2 are electron spinors in and out and J_μ is the matrix element of electric current between the nucleon states of four momentum P_1 and P_2. From relativistic invariance arguments J_μ must have the form

$$\gamma_\mu F_1(q^2) + \sigma_{\mu\nu} q_\nu F_2(q) \tag{2}$$

taken between proton spinors, where F_1 and F_2 are unknown functions of q^2. For a point particle $F_2 = 0$, $F_1 = 1$.

It is difficult to say what would happen if electrodynamics failed, as long as the exact manner of a supposed failure is not specified. A conventional way to assume the failure is to suppose that the propagator is altered from $1/q^2$ to

$$1/q^2 (1 - q^2/\Lambda^2) \tag{3}$$

and to tell how large Λ would have to be for such a modification to remain undetected in a given experiment.

In the proton scattering the effective $F_1(q^2)$ is found to fall, for smaller q^2, as $1 + q^2/(560 \text{ Mev})^2$ (q^2 is negative). If F_1 did not fall at all, but the altered propagator were responsible Λ would be 560 Mev. If Λ is much less than this the proton would look much softer and extended than it does. We can probably safely conclude from this experiment that Λ exceeds 500 Mev.

According to (1) the scattering for different angle and energies, *providing they correspond to the same q* should all be related via just two unknown numbers F_1, F_2. This will fail to be exactly so because of corrections, probably not large, due to the exchange of two or more photons. These corrections may be computed, although with some uncertainty due to proton structure. If a large deviation still persists it would mean that Q.E.D. fails in a very peculiar way — for example, that another virtual object of higher spin is exchanged, or that there is a new coupling of proton and electron so that they may combine to form a neutral heavy particle which disintegrates back again to proton and electron, etc.

So far most physicists believe that the general behavior of the functions F_1 and F_2 is understandable as a proton structure effect and therefore that Q.E.D. can be trusted to perhaps at least as high as 1 Gev.

Clearly these uncertainties of proton structure would not arise if electrons or positrons were scattered from electrons. For example, there are measurements to 5% accuracy of the cross section for annihilation of positrons in flight [5] up to laboratory energies of nearly 10 Gev. In the center of gravity system, however, the positron momentum is only 50 Mev and the virtual state momenta of importance are much lower still. The experiments agree with theory but this does not put a very great lower limit on Λ. The same comment applies to μ^-, e^- collisions measured [6] up to 8 Gev, also in agreement with theory.

On the other hand, these experiments must not be treated too lightly. They are only uninteresting if they agree with theory. Although they do not yet involve nearly as high virtual energies as the proton scattering experiment, they test different things, such as the electron propagator, or the muon structure.

The types of experiments at high energy which would more effectively test the predictions of Q.E.D. are discussed by J. Bjorken and S. Drell, *P. R.*, **114**, 1368 (1959).

Energy levels in hydrogen.

Turning now to precision low energy experiments, the classic experiment [7] is the direct measure of the $2s_{1/2}$ and $2p_{1/2}$ energy separation in hydrogen, deuterium and ionized helium. It was the analysis of this experiment by Weisskopf and by Bethe which led them to discover a way to circumvent the divergent self-energy which, up to then, had bedeviled any attempt to compute higher order effects from Q.E.D. The need to put their ideas into a relativistically invariant form led to the formulations of Schwinger and of the author. The Lamb effect still remains one of the most delicate tests of Q.E.D. A comparison [8] of theory and experiment is given in Table I (after Peterman [9]).

Contributions to the Lamb shift arise from several sources :

(*a*) *Virtual emission and readsorption of one virtual photon.*

Here in the initial state an electron is in a definite state ($2s_{1/2}$ or $2p_{1/2}$) in the nuclear Coulomb potential. In the intermediate state it is in some other exact state of the Coulomb potential. The wave function for these states should be found by the Dirac equation. The labor in doing this has been too great, so far, even for computing machines, because for each intermediate state n, matrix elements of the current times exp. $i\vec{K}\cdot\vec{X}$ must be found for every K and a double integral on $\vec{K}$ and n is involved. However for low energy photons the dipole approximation and Schrodinger wave functions can be used and the sums performed. This determines [10] the constants $K_o(2,0)$ and $K_o(2,1)$. For higher intermediate energy it is usual to make an expansion of the intermediate wave functions as plane waves, perturbed by the Coulomb potential (hence an expansion in orders of $Z\alpha$). Combining the first term with the low energy contribution gives the largest part of the Lamb shift, items 1 plus 2. Item 2 is separated here, for it is easy to understand as the correction to fine structure due to the apparent anomalous moment of the electron.

The correction to include two potential scatterings is included [11] in item 4. To include three scatterings is very difficult, but Layzer [12] has shown that it is a quadratic form in $ln(Z\alpha)$ and has computed everything but the constant term. This makes an uncertainty in the total effect from one virtual photon. It is unlikely that the part marked " ? " exceeds $\pm$ 10, so we may take this as a kind of limit of error. Terms of higher order in the potential are probably too small to be significant.

(b) *Emission and reabsorption of two virtual photons.*

This is of order one higher in α, and no large logarithm from low energy photons arises, so the effect is very small indeed. Although the magnetic moment part, item 7, has been worked out exactly [13], the potential spreading effect, item 6, has only been partially evaluated, in such a way that limits of error can be given [14]. This work should be completed because the uncertainty here is the largest contributor to the theoretical uncertainty in the Lamb shift.

TABLE I. — (After Petermann)[9].

Lamb shift : $2S_{1/2}$-$2P_{1/2}$ $(1/\alpha = 137.0389)$

Order		Formula, units $Z^4L(\mu/m)^3$	H	D	He^+
1. $\alpha(Z\alpha)^4$	Rad	$-2\ln Z\alpha + \frac{m}{M} + \frac{11}{24} - \ln \frac{K_0(2,0)}{K_0(2,1)}$	1009.85	1010.64	13168.4
2. $\alpha(Z\alpha)^4$	Mag Mom	$\frac{1}{2}(1 + \frac{1}{4}\frac{m}{M})$	67.71	67.77	1084.7
3. $\alpha(Z\alpha)^4$	Vac Pol	$-\frac{1}{5}$	—27.08	—27.11	—433.9
4. $\alpha(\alpha Z)^5$	2nd order V	$3\pi Z\alpha\,(1 + \frac{11}{128} - \frac{1}{2}\ln 2 + \frac{5}{192})$	7.14	7.13	228.4
5. $\alpha(\alpha Z)^6$	3rd order V	$(\alpha Z)^2[3\ln^2 Z\alpha + (4\ln 2 + 1 + \frac{7}{48})\ln Z\alpha + ?]$	—.25 ± .07	—.25 ± .07	—9.5 ± 4.
6. $\alpha^2(\alpha Z)^4$	4th order Rad	$\frac{3\alpha}{2\pi}(0.52 \pm .3)$	.24 ± .13	.24 ± .13	3.9 ± 2.
7. $\alpha^2(\alpha Z)^4$	Mag Mom	$-.328\ \alpha/\pi$	—.11	—.11	—1.7
8. $\alpha^2(\alpha Z)^4$	Vac Pol	$-\frac{41}{54}\frac{\alpha}{\pi}$	—.24	—.24	—3.8
9. $\alpha(\alpha Z)^4\frac{Zm}{M}$	Finite Mass	$\frac{Zm}{M}[4.862 - \frac{1}{2}\ln Z]$	.36	.18	2.5
10.	Finite size	$\alpha(m/\alpha)^2 \langle R^2 \rangle_{nuc.}$	.12 ± .02	.73 ± .02	7.1 ± 1.
		Total, Mc	1057.74 ± .22	1058.98 ± .22	14046.1 ± 7.
		Exp. Mc	1057.77 ± .10	1059.00 ± .10	14040.2 ± 4.5

$L = \alpha^3\ \mathrm{Ryd}_\infty\ c/3\pi = 135.6353Mc$, $\mu/m = (1 + m/M)^{-1}$

m = mass of electron, M = mass of nucleus.

(c) *Vacuum polarization.*

The effective potential from the nucleus is altered by the existence of virtual pairs of electrons and positrons created by the potential. This has the main effect given in item 3. A correction of relative order α has also been calculated [15], item 8. Corrections of order $Z\alpha$ to the vacuum polarization are included in item 4 (the term 5/192). In fact the vacuum polarization has been calculated [16] for arbitrarily strong fields (arbitrary $Z\alpha$), but additional corrections to the term first order in $Z\alpha$ are small even for Pb.

(d) *Finite nuclear mass.*

The biggest correction resulting from the finite nuclear mass is the correction to the probability of finding the electron at the nucleus due to use of the reduced mass μ rather than the electron mass m in the Schrodinger equation. Beside this the mass appears in the logarithms for item (1) and in the fine structure correction item (2) in a way that is readily evaluated. There are, however, additional corrections of order Zm/M from two photon exchanges between the electron and the recoiling nucleus. They are given by [17] [18]

$$Z\frac{m}{M}L\left(2ln\frac{2K_o(2,1)}{Z\alpha K_o(2,0)}+\frac{83}{24}+\frac{3}{2}\left[ln\,Z\alpha+\frac{7}{4}+\frac{4}{3}(1-ln2)\right]\right)$$

and are included in item 9. (My figures seem to differ slightly from those of Peterman.)

(e) *Nuclear structure.*

If the nucleus is not a point charge the potential near the nucleus is slightly altered, perturbing the s state but not the p state in first approximation. This is an effect calculated by elementary perturbation theory if the mean square radius of the nuclear charge density $<R^2>_{nuc.}$ is known. This can be got directly from scattering experiments and the effect evaluated. The error is just a reflection of experimental error.

(f) *Higher order terms.*

Terms in next order in α should probably contribute at most a few hundredths of a Mc to H and D and perhaps up to 1 Mc to He^+.

The last three columns give the shift calculated in megacycles from each of these terms for H, D, and He^+. They are calculated using $1/\alpha = 137.0389$. If $1/\alpha$ is larger than this by ε the correction to the theoretical value for H and D is -22ε Mc which is almost certainly less than $\pm .02$ Mc (the present uncertainty [8] in $1/\alpha$ is $\varepsilon = \pm .0006$).

The agreement between theory and experiment exhibited in the last line is excellent. The errors quoted should be considered more as limits of error than probable errors. The error in the theoretical estimate could probably be reduced by a factor nearly 10 by a more detailed calculation of item 6, and, what might prove even harder, an estimate within ± 1 unit of the constant term " ? " in the expression for item 5. [It might also be possible to compute the total for one virtual photon to all orders in $Z\alpha$ exactly, as described in part (a) above.] It may be very hard to reduce the experimental error, however, for the position of a line is being measured to about one-thousandth part of its width.

What is the significance of this agreement, let us say, to ± 0.1 Mc in the hydrogen Lamb shift ? Again, an evaluation depends on how you expect Q.E.D. to fail. If it is expected that the failure appears only at high momentum transfer, say in the photon propagator, very little is checked here that is not already involved in the electron-proton scattering experiments. In the hydrogen atom the electron is successively scattered by the proton and if this, at very short distances, is not the ideal point charge scattering it can be corrected by using the directly measured scattering, without regard for the reason for the difference from the ideal scattering. For example, in the very unlikely event that Q.E.D. and proton structure effects are compensating each other in the proton scattering experiments, they will compensate here too; the correct net effect being still given in item 9.

On the other hand, one might contemplate a failure involving a modification of the propagator extending out to very large distances (compared to 10^{-13} cm), but having a very small coefficient. For example, suppose it is suggested that Coulomb's law is altered so that the potential from a charge can be approximated by the form $1/r^{(1+\varepsilon)}$ in the range of r of order 10^{-8} cm. We can conclude from the Lamb experiment that ε is less than 10^{-10}. This is because

the close coincidence of the $2s_{1/2}$ and $2p_{1/2}$ levels, each of which has an energy of the order of 10^9 Mc, is a kind of accident involving the perfection of the Coulomb law. The Lamb experiment tells us that any modification that perturbs the $2s$ energy level (in a substantially different manner than it disturbs the $2p$ level) must disturb it by less than one part in 10^{10}.

But perhaps the most satisfying aspect of the agreement of theory and experiment here is that it checks the general theoretical viewpoint. There cannot be much argument that the effects which we ascribe to virtual photons or virtual pairs acting in various orders do exist (although the philosophical ideas used to describe them may someday be altered drastically, of course).

The analogous Lamb shift in other states, such as $3s_{1/2} - 3p_{1/2}$, etc. have also been measured [7] and agree in a satisfying way with theory, although the test here is not quite as stringent as for the $2s_{1/2} - 2p_{1/2}$ because of the somewhat larger experimental error.

The $2s_{1/2} - 2p_{3/2}$ separation is also measured so, as a byproduct, we have a measurement [20] of the fine structure separation $2p_{3/2} - 2p_{1/2}$ in deuterium. The formula for this separation is [20] [18] [12]

$$\Delta = \frac{1}{16}\alpha^2 \,\mathrm{Ryd}_\infty\, c \left(\frac{\mu}{m}\right)^3 \left[(g_{el}\frac{m}{\mu} - 1) + \frac{5}{8}\alpha^2 - 2\frac{\alpha^3}{\pi}\,(ln(1/\alpha) + ?)\right].$$

This formula can be derived by the usual complete Q.E.D. analysis, but its terms are easy to understand. The first is the energy of interaction of the electron's magnetic moment with the nucleus revolving about it, considering the electron at rest. It involves the anomalous magnetic moment of the electron, calculated [13] to be

$$g_{el} = 2\,[1 + \alpha/2\pi - 0.328\,(\alpha/\pi)^2] = 2\,(1.00115961) \qquad (4)$$

and a factor $m/\mu = 1 + m/\mathrm{M}$, where M is the deuteron mass, to correctly represent the velocity, relative to the electron, of the nucleus generating the magnetic field. The factor in front of the [ ] is obtained by averaging the interaction over the Schrodinger wave function for the p state. The next term, -1, is the Thomas precession correction. The relativistic correction to these two terms combined is to order α^2 just $\frac{5}{8}\alpha^2$, in accordance with the fine structure formula of the Dirac theory of hydrogen. Because the electron can emit and absorb virtual photons its effective location is smeared

over a range (the main source of the Lamb shift of the $2s$ state), so the fine structure interaction is corrected in order α^3. The biggest part of this is a term in $ln\ \alpha$ whose coefficient is easily understood, but a more complete evaluation of the α^3 term, up to the constant " ? ", has not been carried out. Since the $\alpha^3\ ln\ \alpha$ term only amounts to one ppm (part per million) and the experimental result

$$\Delta = 10971.58 \pm .20\ \mathrm{M}c$$

is available only to 20 ppm (limit of error) there is no great need to evaluate the α^3 term more completely than at present. Aside from the small terms $\alpha^3\ ln\ \alpha$, if an experimental value for g_{el} is used, this formula contains no subtle virtual state effects of Q.E.D. that cannot be understood from the Dirac equation and semi-classical arguments. It has been used to obtain a value for α, or as one of the equations in a general evaluation of the fundamental constants.

Anomalous magnetic moment of electron and muon.

From the emission of virtual photons the predicted value 2 for the gyromagnetic ratio of the electron is altered to the expression (4) valid to order α^2. The first term is from one virtual photon. The second term contains two effects : (*a*) the effect of two virtual photons, and (*b*) the vacuum polarization correction to the propagator of the first virtual photon. They have been calculated by Peterman. For the muon, supposing it to satisfy the Dirac equation but with a different mass, all the terms are, of course, the same except term (*b*). In the vacuum polarization for the muon there are terms for virtual electron pairs as well as muon pairs, and the predicted [21] g value for the muon is

$$g = 2\,[1 + \alpha/2\pi + 0.75\,(\alpha/\pi)^2] = 2\,(1.001165) \qquad (5)$$

A value for g_{el} has been obtained by Hardy and Purcell by combining a measurement of Gardner and Purcell (see reference [24]) of the cyclotron frequency of the electron to a magnetic moment measurement of Beringer and Heald [22] to get

$$g_{el} = 2\,(1.0011552 \pm 8)\,.$$

However, an independent measurement of the cyclotron frequency by Franken and Liebes [23] gave somewhat different results, and leads to a g value of

$$g_{el} = 2\,(1.001168 \pm 7)\,.$$

A direct measurement of Schupp, Pidd and Crane [24] gives

$$g_{el} = 2\,(1.0011609 \pm 24)\,.$$

These results are in fair agreement with the theoretical result (4). The result of Schupp *et al.* implies that the coefficient of the α^2/π^2 is -0.1 ± 0.4.

The theoretical result should probably be accurate to one part in about 10^{-8} guessing that the next term in the series is roughly $\pm(\alpha/\pi)^3$. If the photon propagator is modified as (3), the correction to $g/2$ is $-\frac{\alpha}{3\pi}\left(\frac{m}{\Lambda}\right)^2$ so an agreement to 1 ppm means only that Λ exceeds 15 Mev. More information comes from the measurement of $g_\mu - 2$ for the muon. The experimental result [25] for g_μ is $2\,(1.001145 \pm 22)$ agreeing with the theory within its error of 22 ppm. In this case a propagator like (3) would correct $g/2$ by $-\frac{\alpha}{3\pi}\left(\frac{m\mu}{\Lambda}\right)^2$ where m_μ is the meson mass of 105 Mev. If this is not to exceed 22 ppm, Λ must exceed 630 Mev. This is therefore at least as good a test as is provided by the proton-scattering experiments. It is remarkable in that it tests at the same time that the heretofore unfamiliar particle, muon, satisfies the Dirac equation with no appreciable structure comparable to its own Compton wave length.

Hyperfine interaction.

The hyperfine splitting in the ground state of hydrogen resulting from interaction of the nuclear moment and the electron has been measured [26] very accurately for the three isotopes of H, and for the He^3 ion. The theoretical formula [27] for this splitting is

$$\Delta\nu = \frac{16\,\alpha^2 c}{3}\,\mathrm{Ryd}_\infty\left(\frac{\mu_p\,\mu_{el}}{\mu_o{}^2}\right)\left(\frac{\mu}{m}\right)^3\left[1 + \frac{3}{2}(Z\alpha)^2 - (\frac{5}{2} - ln2)Z\alpha^2 - X\frac{\alpha m}{M}\right] \tag{6}$$

where μ_p, μ_{el} are the magnetic moments of proton and electron and μ_o is the Bohr magneton, $\mu/m = \left(1 + \frac{m}{M}\right)^{-1}$ where M is the mass of the nucleus. The terms in front of the bracket gives the value expect from a non-relativistic analysis, given by Fermi. In the bracket the $\frac{3}{2}(Z\alpha)^2$ is a Breit correction resulting from the use of the Dirac equation instead of the Schrodinger equation. The next term is a correction from virtual photons in Q.E.D.

The last term is a correction for recoil and finite size of the

nucleus. A direct calculation assuming a point charge and dipole, leads to a result which diverges logarithmically [28]. It is sensitive to the electromagnetic structure of the nucleus. It arises from the exchange of *two* virtual photons with the proton (in H). If the amplitude for this exchange at high energy were known, the term could be evaluated. Data could come from the forward spin flip Compton scattering from a proton. It cannot come directly from proton-electron scattering experiments for these only give the form factor for a one photon interaction with the proton. Assuming that each of the two interactions has the same form factor as does a single photon, C. Iddings and P. Platzmann, *P. R.*, **113**, 192 (1959) evaluated X as 8.7, corresponding to a correction of — 35 ppm. However, I think uncertainties in the assumption relating one and two photon structure factors, as well as uncertainties of the one photon structure factor itself for high energy may make the error in X as high as — 2 or + 10 ppm. [There is a relative insensitivity to these assumptions because a major part of X involves the large $ln(M/m)$.]

Unfortunately, therefore, we cannot use these high precision measurements directly to test Q.E.D. independent of our uncertainties in the electrical properties of the nucleus *. Nevertheless, there may be a discrepancy here for the measurement of Lambe and Dicke [19] corrected by 27.5 ppm for a diamagnetic correction gives, with $1/\alpha = 137.0389 \pm .0006$, a result for the X term of $+ 0.7 \pm 8.8$ ppm instead of — 35 ppm, but terms in (6) of order α^3 may be unusually large **.

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* The hyperfine splitting for deuterium is still more dependent upon the nuclear structure, this time of the deuteron. The ratio υ_D/υ_H divided by the ratio of the magnetic moment of D and H and by the reduced mass factors cubed is one minus 1.703×10^{-4} experimentally. An attempt to estimate this from nuclear theory by Low and Salpeter, *P. R.*, **83**, 478 (1951) gave $1.98 \pm .20 \times 10^{-4}$, but this calculation could probably be improved today.

** NOTE ADDED IN PROOF :

D.E. Zwanziger has just informed me of calculations of corrections of order $\alpha^3(ln\alpha)^2$ and $\alpha^3 ln\alpha$ to the hyperfine separation in hydrogen made by him and A.J. Layzer independently. They find a contribution of — 9 ppm so the total predicted term is — 44 ppm. Thus, a real discrepency to the " measured " + 0.7 ± 8.8 ppm seems to be developing here. However, the trouble might lie instead in the measurement of the fine structure separation in deuterium, for E. Richard Cohen has kindly informed me that if this fine-structure measurement is omitted from a least-square reduction of the fundamental constants the value of $1/\alpha$ is 137.0417 ± .0025. The " measured " value of the hyperfine separation term would then be — 40 ± 36 ppm (instead of + 0.7) which would be consonant with the theoretical value.

Zwanziger has noted that these uncertainties disappear if one compares, in the same atom, the hyperfine structure in the $2s$ state to that in the $1s$ state. Measurements of the hyperfine shift in the $2s$ state have been made by Heberle, Reich and Kusch, *P. R.*, **101**, 612 (1956), **104**, 1585 (1956). According to the Fermi formula the ratio should, in first approximation, be as the probability of finding the electron at the nucleus, (1/8), but the actual measurements lead to a result

$$8\,\nu_{2s}/\nu_{1s} = 1 + d \text{ where } d = 34.6 \pm 0.3 \text{ ppm for H}$$
$$= 34.2 \pm 0.6 \text{ ppm for D} \qquad (7)$$

The formula (6) is not adequate to calculate d to this accuracy, for the expression in brackets has not been carried to a high enough order. It might be expected that if divergences arise already in this order they should be still worse for higher order, but Zwanziger shows that this is not true if the ratio ν_{2s}/ν_{1s} is calculated. The term d has several contributions. The electron magnetic moment is spread by vacuum polarization, and by the form factor in the theory of this moment. In addtion the interaction of the electron with the nucleus is altered because the wave function of the electron is altered. This is because, just as in the usual Lamb shift, the electron sees a modified potential since it emits and readsorbs photons [giving a term in $ln(m/\text{Ryd})$] and further, the potential is actually modified by the vacuum polarization. This calculation is similar to the first order Lamb calculation. Terms of order $\alpha^2 m/M$ have been calculated by Schwartz (see reference [27]). The theoretical result for d is

$$d = 34.5 \pm 0.2 \text{ ppm}$$

which agrees excellently with the experiments (7). The Breit term alone gives $\frac{5}{8}\alpha^2$ or 33.3 ppm so that we do not here have any sharp test of Q.E.D. at short distances. But it does confirm our general ideas and checks again, but less accurately, that there are no small deviations at larger distances.

There is a measurement of the hyperfine structure of the metastable triplet state of He^3 by White *et al.*, *P.R.L.*, **3**, 428. If this is compared to the He^3 ion hyperfine separation, the dependence on nuclear structure cancels out. For the ratio of frequencies they get 6.2211384 ± 12. To calculate this it is necessary to know the wave function for the triplet state to find the probability (times 6)

that one electron is at the nucleus. The non-relativistic theory with the best known wave function gives 6.222030 for this. Relativistic corrections reduce this to the theoretical prediction 6.221199 ± 6 for the frequency ratio. The remaining deviation is likely to be a slight inadequacy (only 10 ppm) of the variational wave functions. There is no evidence that Q.E.D. is any less adequate in handling systems with two electrons than it is for one.

Positronium.

The " atom " formed from an electron and a positron presents none of the uncertainties of nucleon structure that the hydrogen atom does. It is therefore an interesting object for calculation, although the experiments are much more difficult because of its transient nature. A still better object would be muonium but the experiments here have not yet been performed.

Just after the newer methods of Q.E.D. were developed, since they were so readily applied to perturbation theory of free systems, it was supposed by many that bound state problems presented some special difficulty. That this was not so was noted by W.E. Lamb, *P. R.*, **85**, 259 (1952) and E.E. Salpeter, **87**, 328 (1952). Evidently one cannot analyze the bound state by starting a perturbation series from non-interacting particles. But one very effective way is to use, as a starting point, the system held together by instantaneous Coulomb potentials. This system can be analyzed by an ordinary differential equation in time, like the Schrodinger equation, because of the instantaneous nature of the interaction. The perturbation then consists of adding the effect of virtual transverse photons in various orders. Of course, any other unperturbed system, held together by some approximation to the true interaction, will serve as well; the perturbation being the difference between the true interaction of Q.E.D. and the approximate interaction assumed. The instantaneous Coulomb potential is a good starting point because its initial approximation is so good.

The most complete analysis of the hydrogen-like atom with arbitrary mass ratio of the two charges has been given by T. Fulton and P. Martin [18], where references to earlier work will be found. They have used their equations to compute the energies up to the first order Lamb effect (i.e., to order α^3 Ryd) of many states in

positronium. The most delicate test is the separation between the singlet and triplet $1s$ states of positronium measured by Weinstein, Deutsch and Brown, *P. R.*, **98**, 223 (1955) to be $2.0338 \pm 4 \times 10^5$ Mc.

The theoretical value [29] for this shift is

$$\alpha^2 \, \mathrm{Ryd}_\infty \, c \left[\frac{2}{3} + \frac{1}{2} - \left(\frac{16}{9} + ln2\right)\frac{\alpha}{\pi}\right] = 2.0337 \times 10^5 \, \mathrm{M}c$$

the first term represents the first order interaction of the spins and the second the amplitude for virtual annihilation into a photon with re-creation of the positron again. It is clear experimentally that such a term exists, once again confirming our general view of what virtual processes go on. The last term is the first order Lamb correction for this system; it amounts to 0.0100 Mc.

General conclusions.

There are many other calculations and experiments in which some aspect of Q.E.D. is involved (such as vacuum polarization effects in mu-mesic atoms, or relativistic corrections to the computed helium ground state energy, etc.). We shall not go on to describe them for, although confirming Q.E.D., they do not provide sharper tests than the examples already given.

All this may be summarized by saying that no error in the predictions of quantum electrodynamics has yet been found. The contributions expected from the various virtual processes envisaged have been found again and again, and there is very little doubt that in the low energy region, at least, our methods of calculation seem adequate today. The region of energy (of virtual states) that has not yet been explored even for gross errors exceeds 600 Mev (the Compton wave length corresponding to this is 2π times 3×10^{-14} cm). There are no experimental indications that the laws of Q.E.D. cannot be exact. Are there any theoretical reasons to expect a failure ? I will discuss such questions in the next few sections.

Before we do this I should like to make a remark on the character of these calculations. It seems that very little physical intuition has yet been developed in this subject. In nearly every case we are reduced to computing exactly the coefficient of some specific term. We have no way to get a general idea of the result to be expected.

To make my view clearer, consider, for example, the anomalous electron moment given in (4). We have no physical picture by which we can easily see that the correction is roughly $\alpha/2\pi$, in fact, we do not even know why the sign is positive (other than by computing it). In another field we would not be content with the calculation of the second order term to three significant figures without enough understanding to get a rational estimate of the order of magnitude of the third. We have been computing terms like a blind man exploring a new room, but soon we must develop some concept of this room as a whole, and to have some general idea of what is contained in it. As a specific challenge, is there any method of computing the anamalous moment of the electron which, on first rough approximation, gives a fair approximation to the α term and a crude one to α^2; and when improved, increases the accuracy of the α^2 term, yielding a rough estimate to α^3 and beyond ?

THEORETICAL QUESTIONS

Self-energy.

The first difficulty which arises if one assumes that each of the transverse electromagnetic modes in a box is a quantized oscillator is that each should have a zero point energy $\omega/2$. Since there are an infinite number of modes this zero point energy is infinite. It is easy to get around this difficulty, however, by supposing that absolute energy cannot be measured (leaving a question for the theory of gravitation) so that all the zero point energy is subtracted. But now suppose we put atoms or other objects in the box at a small density N per unit volume, so that the index of refraction is changed from 1 to $1 + 2\pi N f_K^\gamma/\omega^2$ where f_K^γ is the real part of the forward scattering amplitude of the object for light of mode K. The wave lengths which fit into the box are still the same, but the frequency of the modes is changed by $2\pi N f_K^\gamma/\omega$ so the total zero point energy is changed, per object, by

$$\Delta E = \frac{1}{2}\Sigma_K \frac{4\pi f_K^\gamma}{2\omega_K}.$$

This shift in energy we would associate with the object and would call it the self-energy of the object. There are higher terms from

the effect of scattering two photons at once, but we shall just go to the first order in α. In addition, we have the positron-electron Dirac field and will have to add a contribution for the shift in energy of the electrons in the negative energy sea. This will involve f_N^{pos} the amplitude for the object to scatter forward a positron in state N. It is better to deal with electrons and positrons symmetrically and we find the following formula for the self-energy of an object :

$$(4\pi)^{-1}\Delta E = \frac{1}{2}\Sigma_K \frac{f_K^\gamma}{2\omega_K} + \frac{1}{2}\Sigma_n \frac{f_n^{el}}{2E_n} - \frac{1}{2}\Sigma_N \frac{f_N^{pos}}{2E_N} \qquad (8)$$

where f_n^{el} is the real part of the amplitude for the object to scatter forward an electron in state n, f_N^{pos} that for scattering a positron in state N, and f_K^γ that for scattering a photon in state K (K specifying momentum and polarization), everything to first order in α *.

Applied to a free electron, however, (8) still gives a divergent result. It might be thought that this could also be subtracted away, and only differences taken for the electron in different states i, but these differences are also infinite. This is most easily seen if one compares the energy of two photons with the energy of the pair one expects to create from them. No completely satisfactory way has been found out of these difficulties.

Electron mass.

In making actual calculations, one way to handle the difficulty is this : temporarily stop the divergent integrals at some high energy and note that the self-energy effect, at least insofar as it depends (logarithmically) on the cut-off is equivalent to changing the mass of the electron from m_o to $m_o + \Delta m$ in every process (of energy well below the cut-off). If we write $m = m_o + \Delta m$, interpret it as the experimentally observed mass, and write all results in

* To be more explicit if the object is an H atom with an electron in state i this formula gives the correct level shift to order α if n and N are states in the nuclear potential Z, but the interaction of the electron in i and the electron n is calculated only to first order in α. That this is true can be easily demonstrated by writing out each amplitude by diagrams, adding the results, and comparing with the unusual diagram for the virtual photon level shift. Actually, only the exchange scattering in f^{el} and the annihilation scattering in f^{pos} need be taken. The rest cancels out. If the object is charged, like a free electron, there is no true forward scattering f^{el} as it goes, by the Rutherford law, as θ^{-2} but f^{pos} does likewise and, in (8), they cancel out to a finite limit as $\theta \to 0$.

terms of m, then the limit can be taken as the cut-off energy goes to infinity. Expressing everything in terms of m is called renormalizing the mass, and a theory for which results are then independent of the cut-off as it goes to infinity are called renormalizable. Q.E.D. is renormalizable if the electron mass, and the charge, are both renormalized (it is, I think, not necessary to renormalize the mass ratio of muon and electron).

What is the meaning of this ability to renormalize the mass ? From one point of view it is no problem at all. After all, only m is observable, not m_o so the m_o is a construct which should be got rid of anyhow. Only the renormalized theory should have been written in the first place. Two questions arise, however.

The first question is whether the renormalized theory is, in fact, a logically consistent theory. With any finite cut-off the theory is not consistent, slight deviations from unitarity (the principle that the sum of probabilities for all alternatives should be unity) occur. These get smaller as the cut-off energy Λ goes to infinity. But the mass m_o that may be needed to get a finite m for very large Λ may be negative. That is, the theory may contain hidden difficulties if we computed processes for energies E such that $\alpha\, ln(E/m) \gtrsim 1$. This is such an extreme energy that such matters are of no apparent concern to calculation of lower energy phenomena. However, from a strictly theoretical view it would be nice to know whether renormalized Q.E.D. is a consistent theory, or whether difficulties may not arise of relative magnitude e^{-137}. The great difficulty in answering such questions is our limited mathematical ability to deal with situations where some kind of perturbation theory does not suffice. I do not know if it has even ever been proved that Δm still diverges if all orders of perturbation theory are included. [The perturbation result for Δm is $m_o\,(3\alpha/2\pi) ln\, \Lambda/m_o$].

The second question concerning the renormalization idea is that renormalization of a quantity Λ gives up any possibility of calculating that quantity. Now it may be that the " electromagnetic part of the electron mass " is unobservable, but this is not true of other particles. The difference in mass of proton and neutron, or of π^+ and π^o, or of K^+ and K^o, etc. are almost certainly electromagnetic in origin. They cannot be computed with a renormalized theory, for in such a theory any constant can be added to the masses of

each particle. It should be objected that these baryons and mesons are complicated systems, in virtue of the strong interactions, and we therefore do not know the correct laws of coupling to calculate this mass difference. If we knew them, perhaps the mass differences would converge without any modification of Q.E.D. itself. This is indeed possible, in principle, but so far it has not been demonstrated to be true in fact. Any complete field theory yet written for strongly interacting particles and electromagnetic interactions, has always appeared to be unrenormalizable unless these mass differences are renormalized as well. It seems odd to try to put the complete burden of the divergences of Q.E.D. on special properties of the strong couplings, but on the other hand, that is where they may indeed lie. At any rate, close study of these mass differences would probably teach something; either of the breakdown of Q.E.D. or of the electromagnetic characteristics of the particles.

If it is assumed that the nucleon and meson structure is not solely responsible for the convergence of the mass differences, and that it is a failure of Q.E.D. instead, then the numerical values of these differences suggest that the failure of Q.E.D. should begin to show up strongly at virtual energies around 1 Gev.

Finally, we should remark on the possibility that all of the mass of the electron is electromagnetic in origin. First, of course, the correction seems to be too small to do that (yet how can something that is infinite appear to be too small ?). But disregarding that, there is the argument that, if the electron mass is zero, the change of the electron field operator from ψ to $\gamma_5\psi$ will not alter things. Put otherwise, a state of right helicity will never be converted to one of left helicity, no matter how often it interacts with real or virtual photons. But an electron with mass does not have this property, so it has been believed that mass cannot come from no mass. But recently several physicists (Heisenberg, Nambu, Schwinger, for example) have argued that this is, in fact, not true. In a ferromagnet the original system has the symmetry that all space directions are equal; but in fact the interaction can produce a polarized background state in which any excited state has an energy depending on its alignment to an axis. That is, if we go beyond perturbation theory such symmetry arguments may fail.

On the other hand, pure Q.E.D. with only zero mass electrons

and photons interacting with no other particles and having no cut-off energy probably cannot produce a finite electron mass. This is because the system is also invariant to a change of scale, there is no parameter to determine a length. Yet an electron with a mass involves such a length. I am not certain, but it appears to me impossible to generate a specific length from no scale whatsoever.

However, in fact, there are two places in Nature from which such a length could come. One is the theory of gravitation, the gravitational constant involves length dimensions and light interacts with gravitons. Further, Machs' principle in quantum mechanics is equivalent to the statement that the surrounding nebulae determine the atomic scale of length in a local vicinity. However, these theories are not developed far enough for us to compute the electron mass starting only with zero mass electron, photons and gravitons in interaction.

A more practical point to notice is that, in fact, photons do interact with nucleons and mesons, and to allow that there is, in that system, an independent scale of length, say the nucleon mass. This would serve as the small length-determining perturbation, which works its way back to determine the mass of the electron. It is always possible that the equation determining the electron mass has more than one solution, and that a second solution is the muon, but we are engaged in pure speculation here.

One way to find out about this is to study more seriously Q.E.D. with electron mass exactly zero. On the one hand, it may be useful for getting a better understanding of Q.E.D. when electron energies are high, but most particularly, it would be interesting to see if such a theory is consistent at all. For example, a charge entering a magnetic field presents problems; it seems to radiate at an infinite rate. Perhaps a careful study of this problem, and the effect of a small length-determining perturbation on the result, would lead us to an understanding of the ratio of electron mass to nucleon mass.

Electric charge.

Q.E.D. contains two constants which must be determined by experiment. One is the electron mass, which we have just discussed. The second is the electric charge, or the dimensionless combination $a = \hbar c/e^2 = 137.039$. For both of these the renormalization process must be applied, so we have foregone computing a also.

It is interesting that all the „ fundamental „ particles which are charged have the same charge, but we have no remarks to make on that point.

The way that the charge becomes renormalized is via the polarization of the vacuum. A virtual pair produced by a photon (also virtual) annihilates again to re-create the photon. In first approximation the correction to a from virtual electrons is $\Delta a = (2/3\pi)\, ln(\lambda/m)$. That is to say, it is divergent, so we have this time to cut-off the electron propagator at some energy λ (this is in addition and different than the photon propagator cut-off discussed above — but they may ultimately have the same origin). Virtual pairs of other particles such as muons simply add their contribution to Δa in first order.

At first it may be argued that here, at least, the philosophy of renormalization is unassailable. The " free charge " must be unobservable (although Gell-Mann has suggested that this may not be so, but high energy interactions may determine it). Only the total corrected a can be measured. Unlike the case of mass where we have charged and uncharged particles, like neutron and proton, to compare, here we have nothing but the single measured a.

This is true, but are you willing to give up, forever, the possibility of computing this remarkable constant a ? If in some ultimate future theory a is to be computed, will we find ourselves correcting some value a_o for virtual pairs ? One can hardly begin to speculate from our present position, but the question of how a can be computed at all has always been intriguing, and I should like to make a few remarks about it.

The quantum theory of electromagnetic interaction can be formulated roughly as follows. Let S_o be the S-matrix operator for the system of particles not interacting with the electromagnetic field, let j_μ be the current density operator of this matter omitting the charge factor, and A_μ be the electromagnetic potential operator times the charge. Then the S-matrix including the field is something like (I am merely outlining here, the precise definitions are assumed to be familiar)

$$S = S_o\, e^{i \int j_\mu A_\mu d\tau}\, e^{i L_o(A)} \tag{9}$$

where

$$L_o(A) = \frac{a_o}{8\pi} \int F_{\mu\nu}\, F_{\mu\nu}\, d\tau \tag{10}$$

$a_o = \hbar c/e_o^2$ is the unrenormalized a and $F_{\mu\nu} = \partial_\mu A_\nu - \partial_\nu A_\mu$. Note that we are using somewhat unfamiliar notation since neither j_μ nor A_μ require knowledge of the charge in their definition, it appears only in L_o.

Now if we take expectation values between states of the vacuum for matter (to get at the vacuum polarization and other effects of virtual pairs) we shall have to calculate the expectation

$$<S_o\, e^{i\int j_\mu A_\mu d\tau}> = e^{i\,L(A)} \tag{11}$$

it being sufficient to consider A_μ as a c-number function and L(A) defined here is simply a functional of A_μ. The completion of a problem requires that we integrate

$$\int e^{i\,(L(A) + L_o(A))}\, D\,A \tag{12}$$

over all potential functions A_μ satisfying the boundary conditions of the problems.

In a way of speaking, then, if we were not aware of the pairs we would say that electrodynamics has the effective Lagrangian $L(A) + L_o(A)$.

The L(A) defined in (11) can be expanded in powers of A_μ and in powers of its derivatives assuming in some situation that A is smoothly varying and small. A constant A_μ has no effect, assuming $\partial_\mu j_\mu = 0$, the conservation of charge, or guage invariance, so the expansion begins :

$$L(A) = c\int F_{\mu\nu}\,F_{\mu\nu}\,d\tau + c'\int (F_{\mu\nu,\,\sigma})^2\,d\tau + c''\int (F_{\mu\nu})^4\,d\tau + \ldots \tag{13}$$

where, except for the first term, only the type of term is meant to be indicated, two $F's$ and two derivatives in the second term, four $F's$ in the third, etc. The c, c', c'' ... are constants. The first term can be combined with L_o in (12) to form $\frac{a_1}{8\pi}\int F_{\mu\nu}\,F_{\mu\nu}\,d\tau$ with $a_1 = a_o + 8\pi c$ and this is the origin of part [34] of the charge renormalization. For electron pairs $c = (1/12\pi^2)\,ln(\lambda/m)$ (as we have said), which is very small (although infinite !) compared to a.

The other terms in the effective Lagrangian $L_o + L$ generate modifications of Coulomb's law, the scattering of light by light, etc. Had these phenomena been discovered before Q.E.D., they would have been representable in classical theory as just such

modifications of Maxwell's Lagrangian, so that the complete Lagrangian would be considered to be a complicated, non-linear and partly unknown affair. With the advent of Q.E.D. explaining the origin of the non-linearities, etc. the natural reaction would be *to try to explain the complete Lagrangian* in this way. The fact that c is infinite, although discouragingly small, would be exciting. It is, in fact, possible that c is not so small; perhaps the cut-off is controlled by gravitation, or, perhaps, some other modification of the laws of matter replaces the logarithm with a less convergent expression, or the cut-off may not be necessary if perturbation theory could be avoided. Finally, every charged particle makes a nearly equal contribution (although not to c' or c'' which vary inversely as the mass squared, or fourth power). So we must add contributions from muons, nucleons and mesons. We do not know how many nor how much each contributes and, although difficult, it is not impossible that a complete future calculation would give a value near 137 to $8\pi c$ so that no a_0 is required at all !

If that is the case, quantum electrodynamics takes a very simple form. In (9) $L_0(A)$ must be omitted. Then the functional integral on A_μ, which must be taken, gives, since $\int \exp.(i\int j_\mu A_\mu d\tau) DA_\mu = \delta[j_\mu]$, a functional delta function of j_μ. It says therefore simply that all amplitude and *expectations* of S_0 *must be taken subject to the condition that the total current density is everywhere and always zero.*

What is electromagnetic interaction ? If a real charge exists it must generate its own four-current (charge density, if at rest). Since total current vanishes this must be compensated by an opposite current in the vacuum sea of charged particles. Because of the dynamics of these particles the compensation cannot occur just locally, but another counter current is generated nearby, compensated by vacuum current again, etc. until this effect is propagated out to infinity. The energy associated with these compensating currents is the electromagnetic self-energy of the charge. Another charge placed in the vicinity adds its system of compensating currents so there is an energy of interaction between these charges. At any rate something like that is implied by these unsupported speculations.

Interaction of other particles.

Although Q.E.D. is very accurate, there is of course one way in which it *must* be wrong; it is incomplete. There are not only

photons, electrons, and muons in the world, but charged baryons and mesons as well. It will not do to say that Q.E.D. is exactly right as it stands in a limited situation where only electrons and muons are present, because virtual states of charged baryons must have an influence. Two sufficiently energetic photons, colliding, will not do just what Q.E.D. in that limited sense supposes; they also produce pions. Or, more subtly, a sufficiently accurate analysis of the energy levels of positronium would fail, for the vacuum polarization from mesons and nucleons would have been omitted.

On the other hand, we use that theory as best we can to discuss the Coulomb potential from the nucleus, the nuclear emission of γ-rays, the Bremstrahlung expected from pions, the chance that γ-rays will be found in the disintegration of the K^+, etc. How do we do it ?

We expect that the principles of Q.E.D. will extend to these particles too, and replace our incomplete knowledge of them by a set of constants (charge, magnetic moment, etc.) which will suffice for low energy analysis.

Yet we use the tool of electrodynamics for much more than that. We make some hypothesis about how the photons are " ultimately " coupled to the new particles. We suppose for example, that the anamolous magnetic moment of the proton has its ultimate origin, not in an extra Pauli term $\frac{\mu}{4M} \sigma_{\mu\nu} F_{\mu\nu}$ in the " original Lagrangian " but in the currents of virtual mesons which surround the nucleon. That is, we assume that the coupling is in some sense ultimately as simple as possible, and all apparent anamolous effects have their origin in complexities of the strongly interacting particles themselves. This hypothesis is universally used, permitting us to use electromagnetic interaction to learn something about the strange particles. Yet it has never been formulated in a completely precise manner. Its importance was first emphasized by Gell-Mann who called it the principle of minimal electromagnetic interaction, but following a suggestion of Telegdi, I shall call it Amperes-hypothesis (the assumption that all magnetism comes from currents).

There is one exception already known to this principle; photons interact with the gravitational field without a charged intermediary. But this interaction can be viewed the other way about, that gravity

interacts with all energy, photon energy included. We shall therefore disregard this counter-example for the present.

The simplest suggestion for defining Amperes-hypothesis would be to say that in the "fundamental Lagrangian" (the exact form of which, at present, unknown) all gradient operators ∂_μ on charged fields are to be replaced by $\partial_\mu - A_\mu$ and no other coupling to A_μ is to be assumed. There are two objections to this formulation. First, we do not know the form of the future theory; no Lagrangian may exist. Is there not some formulation closer to the observed properties of the particles? The second objection is possibly academic; if the Lagrangian were before us, we would probably know exactly what to do. Yet a term like $\sigma_{\mu\nu}\partial_\mu\partial_\nu$ which is evidently zero could be added, but when ∂_μ is changed to $\partial_\mu - A_\mu$ it is no longer zero but is a Pauli term $\sigma_{\mu\nu} F_{\mu\nu}$ instead. Such an ambiguity would arise, say, if the Lagrangian contained second derivatives, and it was not clear whether to write $(\gamma_\mu\partial_\mu)^2$ or $\partial_\mu\partial_\mu$ at some point.

One of the effects of terms like a Pauli moment is that certain processes in Q.E.D. become uncalculable. For example, if the muon carried a true Pauli moment the hyperfine split of muonium could not be computed, as the term "X" in (6) is divergent in that case. In other words, the theory would not be renormalizable. Perhaps then Amperes-hypothesis is equivalent to the assumption that the theory is renormalizable. On the other hand, the proton-neutron mass difference may not be one of those computable quantities. We must understand renormalizability, then, as the hypothesis that only a finite number of unknown quantities must be attached, before everything else can be computed.

Another effect of a term like a Pauli moment is to drastically alter the behavior of cross-sections at high energy. From the dispersion point of view, discussed below, constants such as the charge, anamalous moments, etc. appear in the form of subtraction constants required to ensure the convergence of the dispersion integrals involved. More constants are needed if the high energy cross-sections remain large, or increase, with rising energy. Thus Amperes-hypothesis in this viewpoint would take the form of a statement that a certain minimum number of subtraction constants are required.

What amounts to the same thing, but is more readily available to experiment is to try to replace Amperes-hypothesis by a statement

that the size of high energy photon cross-sections are limited in some specific way.

As a last remark, if the current j_μ comes from the Lagrangian so easily, might the structure of this current not tell us something about that Lagrangian (or whatever more fundamentally replaces it) ? This possibility is discussed at this conference by Gell-Mann.

Dispersion theory [31].

In the lowest order in which any process occurs, there are no integrals over virtual states (closed loops), and no divergences will arise. The Q.E.D. difficulties arise on integrating over invisible virtual processes. This generates a feeling that such virtual state integrations are unreal, or at least are not handled quite correctly, and that all of the formulas should be put in terms of directly measurable quantities. This can be done because of the analytic character of the functions involved. Their real parts can be expressed in terms of their imaginary parts. The imaginary parts can be expressed as the rate for real processes of lower order. Thus a diagram involving one virtual momentum integral can be expressed as a dispersion integral over a function determined from processes without a virtual integral at all. Put in this way, it sounds trivial, one integral is replaced by another, almost identical, and in fact from a practical calculational point of view there is often little to gain. But it is hoped that this viewpoint gives a clearer insight into the virtual state integrals and a closer relation to experiment. (The greatest utility of this method results, of course, in the analysis of strong coupling where the fundamental equations are unknown, for here one experimental result can be related to another.)

We can illustrate by the simplest example, the second order vacuum polarization effect of electrons. The amplitude that a virtual photon of momentum q^2 makes a pair and annihilates again is written $q^2 f(q^2)$, so that the entire dependence of L(A) on A expanded to second order is $\int f(q^2)\, F_{\mu\nu}(q)\, F_{\mu\nu}(q)\, d^4q$ written in momentum space.

Now the imaginary part of $q^2 f$ is the rate that a (virtual) photon makes real pairs. It only exists, writing $q^2 = 4m^2x$ for $x > 1$. Choose the time axis in the direction of q and the photon polariz-

ation in the z direction and we require the probability that a vector potential of frequency $2m\sqrt{x}$ constant in space, produces a pair of electrons each of energy $E = m\sqrt{x}$, of momentum $p = m\sqrt{x-1}$. This is one of the simplest elementary problems in Q.E.D. The amplitude for this is $(\bar{u}_2\gamma_z u_1)$, squared and summed on polarizations it is $-sp\,[(\bar{p}_2+m)\,\gamma_z(\bar{p}_1+m)\gamma_z] = -(p_2\cdot p_1 - m^2) - 2p_{2z}p_{1z}$ or averaging over directions, $E^2 + \frac{1}{3}p^2 + m^2 = \frac{2m^2}{3}(1+2x)$. The phase space factor is $pE/(2E)^2$ so, dividing by q^2, we find for the imaginary part of f (times $(4\pi\alpha)^{-1}$)

$$f_i = \frac{1+2x}{3x}\left(\frac{x-1}{x}\right)^{1/2}.$$

The real part is now given by a dispersion integral from Cauchy's theorem as

$$f_R(q^2) = \pi^{-1}\int f_i(q_1^2)\,dq_1^2/(q^2 - q_1^2)$$

or

$$f_R(x) = \frac{1}{\pi}\int_1^\infty \frac{1+2y}{3y}\left(\frac{y-1}{y}\right)^{1/2}\frac{dy}{x-y} \tag{14}$$

The integral, however, is divergent (so the Cauchy theorem is not strictly true, there is a contribution from the contour at infinity). This can be handled in the following way. We can assume f_R for some x is known experimentally or by definition. In this case $f_R(0)$ is the renormalization of the charge, so in the spirit of renormalization we can take it to be zero. Then we can use the Cauchy relation for the more convergent expression $[f(x) - f(0)]/x$. What it leads to is the same as if we subtract from (14) the same equation with $x = 0$:

$$f_R(x) = f_R(0) + x\int_1^\infty \frac{1+2y}{3y}\left(\frac{y-1}{y}\right)^{1/2}\frac{dy}{y(x-y)} \tag{15}$$

an integral which is now convergent and whose integrated value $(2/3x)\,[(2x+1)\,(1-\beta\,ctn\,\beta) - x/3]$, where $\sin^2\beta = x$, is exactly the finite part of the vacuum polarization effect obtained by the usual integral over a closed loop [30].

It is clear that Q.E.D. in its renormalized form may have its simplest expression in this mathematical scheme. We need merely say that $f_R(0)$ vanishes, for we wish to work with the constants

already renormalized. Several authors have discussed Q.E.D. from this point of view [32].

If the integral (15) were still divergent we could perform another subtraction, but generate another uncalculable constant (analogous, in a different problem, to a Pauli anamalous moment). Amperes-hypothesis is that this is not necessary.

It must be admitted, however, that no fundamental change in position on the renormalization question is really involved. If the integral (14) *did* converge we certainly *would* compute it, and call its value at $x = 0$ the change Δa in a_0 induced by pairs. In the usual loop integral method the integral is completed by subtracting the effect that the pairs would have if the mass of the electron were changed from m to λ. The same method here makes (14) convergent and gives the same value for $f_R(0) = \Delta a = (2/3\pi)\, ln(\lambda/m)$.

It might be hoped, therefore, that such dispersion relations involving the entire system of strange particles and Q.E.D. may be a satisfactory way of representing nature. It has much to recommend it; its close relation to experiment, the possibility of interdetermining coupling constants, the avoidance of the possibly meaningless question of which particles are fundamental and which compound, etc. These points have been emphasized by Chew in a remarkable speech at the conference in La Jolla, California this year. One serious question appears, however. Integrals over all energies are still required and at high energies the real processes involve all kinds of particles in considerable numbers. Thus the set of interconnected equations becomes enormously elaborate just when it becomes interesting. It is not clear how to get started grappling with this complexity.

On the other hand, the experimentally observed extreme energy phenomena suggest that they may have certain regularities. If this is so, a central theoretical problem is to formulate these regularities [33]. Only then may it be possible to close in an intelligent way the wide-open hierachy of dispersion relations.

It is in the spirit that all quantities should be reexpressed in terms of others, in principle, observable that the formula (8) for the Lamb shift self-energy was developed. Dispersion theory will probably also permit its being simplified still further. The real

part of the forward photon scattering cross-section $f_K\gamma$ can, of course, be immediately expressed as an integral on the imaginary part, and a similar reduction can likely be made for the other terms. If this can be done the calculation of the Lamb shift for H (to first order in α, all orders in $Z\alpha$) will only involve true rates of real photon absorption or of pair annihilation. Thus, for each state n a definite K value is associated (so $\omega_K = E_n - E_l$), so that the computation may be possible on machines. In fact, many of the matrix elements, useful in calculating internal conversion coefficients, etc. have been already calculated.

The formula (8) is not valid directly for calculating such things as the proton-neutron mass difference, because other virtual fields, like the meson field, must be included. The necessary generalizations of (8) can be written, but we shall have to see how useful they are and if the necessary experimental quantities are available.

Conclusion.

In writing this report on the present state of quantum electrodynamics, I have been converted from a long-held strong prejudice that it *must* fail significantly (other than by simply being incomplete) at around 1 Gev virtual energy. The origin of this feeling was the belief that the mass of the electron (relative to the nucleon, say) and its charge, must be ultimately computable and that Q.E.D. must play some part in this future analysis. I still hold this belief, and do not subscribe to the philosophy of renormalization. But I now realize that there is much to be said for considering theoretically the possibility that Q.E.D. is exact, although incomplete. This assumption may be wrong, but it is precise and definite, and suggests many things to study theoretically, while the other negative assumption, (that it fails somehow) is not enough to suggest definite theoretical research. This is Wheeler's principle of " radical conservatism ".

Things are, of course, quite the other way for experimental research. One should look very hard for an " expected " failure. I have probably been converted from my prejudice, that it must fail, just in time to be caught off base by an experiment next month showing that indeed it does.

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Discussion des rapports de Heitler et Feynman

W. Heisenberg. — From Heitler's lecture we have learned that Lorentz-invariance and local interaction seem not to be compatible in the conventional quantum-theory. On the other hand Feynman explained to us that quantum electrodynamics gives very accurate results on a very wide range of phenomena. These facts suggest, as has often been discussed, that in Qu. E. Dyn. we might meet a situation very similar to the Lee model with local interaction. Let me specify this assumption somewhat further : it would mean that by the process of infinite renormalization we have unconsciously introduced " ghost-states " of very high energy, i.e. an indefinite metric in Hilbert space. If this was true it would easily explain why for all low energy phenomena Qu. E. Dyn. gives excellent results and is a perfectly " closed theory ". At higher energies in realistic physics some modifications of Qu. E. Dyn. will occur since there will be the possibility of creating pairs of nucleons, π-mesons, etc. It may in fact be that the ghost states could be identified to some extend with the baryons, since the norm of baryon-states may have opposite sign to the norm of the electron-states. (That would not in itself interfere with the unitarity of the S-matrix, since we have baryon and lepton-conservation.) At the same time the indefinite metric would explain why it has not been possible to formulate Qu. E. Dyn. without these divergences and limiting processes. Because if, like in the Lee-model, the renormalized operators commute or anti-commute everywhere for a given time, then these operators at a given time are not sufficient to define the complete Hilbert space and we would need the operators in some arbitrarily small but finite time interval in order to define it. This would be equivalent to an infinite renormalization. Quite generally, and independently of Qu. E. Dyn. it seems natural to assume that a local interaction will have the tendency to eliminate the δ-functions on the light cone in the commutators and replace them by a minor singularity — which would be equivalent to an indefinite metric in Hilbert space. Certainly we have no general proof, that we can in

such a formalism always avoid the well known difficulties with the probability interpretation. On the other hand several cases are known in which an indefinite metric in Hilbert space is compatible with a unitary S-matrix. Therefore we should investigate this possibility for reconciling local interaction and Lorentz-invariance, unless some better solution can be suggested.

P.A.M. Dirac. — I would like to give briefly my point of view with regard to field theory. The foundation of atomic physics is the superposition principle, which says that states are of such a character that they can be added together to give other quantities of the same nature. The states must be pictured as embedded in space-time; so that if one is given a state, one can apply to it various operations of rotation and translation to get other states. These operations form a group, the inhomogeneous Lorentz group. It follows that the states provide a representation of the inhomogeneous Lorentz group. The problem of setting up a quantum theory thus becomes the problem of finding a certain representation of the inhomogeneous Lorentz group.

One could attack the problem by looking for all the representations of the inhomogeneous Lorentz group. This method was followed by Wigner in 1939. He expressed the representations in terms of the irreducible representations. The irreducible representations correspond to particles by themselves. Particles in interaction also correspond to representations, but reducible ones. All the work that has been done on quantum field theory may be looked upon as attempts to set up a suitable representation of the inhomogeneous Lorentz group corresponding to physical reality. The attempts fail because of the infinities and produce nothing of mathematical significance.

I think it would be worth while to work on the problem from a more general mathematical basis, in which one does not necessarily build up the representation in terms of field quantities suggested by existing physical theories. The task of primary importance is to get the mathematical relations right. One can then afterwards look for the physical interpretation of the various quantities that enter into the mathematical scheme.

W. Heitler. — I would have no objection to the use of indefinite metric provided it can be done without inconsistencies. When

Heisenberg suggests that quantum-electrodynamics is a "closed theory", this surely can apply to electrons only and implies that the self mass remains infinite. This would not permit a treatment of the electrodynamics of bosons and would not permit a calculation of the mass differences.

W. Heisenberg. — For π-mesons and nucleons it is more reasonable to start not from Qu. E. Dyn., but from an entirely different scheme.

A.S. Wightman. — I should like to comment on Heitler's non-invariant theory. Such theories are of interest from a point of view quite different from that which Heitler considered. They can be studied for the light which they may throw on the theory without cut-off. For this purpose one must examine the dependence on the cut-off of the various quantities occuring in theory. Normally, one considers two cut-offs in this connection, an ultra violet cut-off and a box. In Heitler's theory there is no box and the theory appears Euclidean invariant. This gives rise to phenomena which, I believe, could strongly affect Heitler's conclusions, whatever the purpose for which the theory is studied. What I have in mind is the Haag theorem which says that in a Euclidean invariant theory in which there are canonical variables, the no particle state is necessarily Euclidean invariant. Now the only Euclidean invariant state which admits a reasonable physical interpretation is the physical vacuum. Since in any theory where there is non-trivial pair creation (as in Heitler's) the no particle state is not stationary, there is no reasonable vacuum state. The only way out of this difficulty is to use one of the so-called strange representations of the commutation relations, but in that case the evaluation of the physical quantities of the theory will certainly require some better technique than perturbation theory.

G. Källén. — Perhaps I may be allowed to formulate what Wightman has just said in a slightly different way. In any ordinary field theory you have the interacting field $A(x)$ and the asymptotic incoming field $A^{(in)}(x)$. Further you have a Hamiltonian

$$H(A) = H_0(A) + H_{int}(A).$$

Especially in the old times one often introduced states, mathematical states, as eigenstates of $H_0(A)$

$$H_0(A) \mid n, \text{math}\rangle = E_n \mid n, \text{math}\rangle.$$

In contradistinction to this we have the physical states which are eigenstates of the total Hamiltonian or, which is practically the same thing, $H_0(A^{(n)}) \neq H_0(A)$

$$H_0(A^{(n)}) \mid n, \text{phys.}\rangle = E_n \mid n, \text{phys.}\rangle.$$

Many times one tries to write expansions of the form

$$\mid n, \text{phys.}\rangle = \sum_{n'} C_{nn'} \mid n', \text{math.}\rangle.$$

The so-called " Haag theorem " says that the transformation $C_{n,n'}$ is indeed, very singular from the mathematical point of view. I agree that that is certainly so also for the theory Heitler discussed yesterday. However, I also believe that this fact is not really very serious. If one computes more physical quantities like scattering amplitudes or even self-masses, they can very well exist even if $C_{nn'}$ is singular. Therefore, I believe that this particular argument against the Heitler model is not very relevant.

A.S. Wightman. — The problem stated by Professor Dirac can be regarded as half-solved. I believe that we know *up to unitary equivalence* the reducible unitary representation of the inhomogeneous Lorentz group which belongs to a physical theory with given stable particles. It may be displayed as the representation of the free field theory of particles of the same masses.

To go further one must specify the physical observables of the theory. For the theory of a scalar field, for example, one has

$$U(a, \Lambda)\, \Phi(x)\, U(a, \Lambda)^{-1} = \Phi(\Lambda x + a),$$

$$[\Phi(x), \Phi(y)] = 0 \qquad (x^2 - y^2) \text{ spacelike.}$$

Here $U(a, \Lambda)$ is the unitary representation of the Lorentz group. I believe that the problem of finding the Φ with these properties is a well posed one mathematically and the solutions would give a

natural expression for the basic ideas of field theory put forward thirty some years ago by Dirac, Heisenberg and Pauli. Unfortunately, it is as yet unsolved. Until we understand its solutions or lack of them I feel there will be no physical paradox in the foundations of field theory, just a muddle.

L. Van Hove. — I would like to ask a question to Heisenberg concerning his remarks at the opening of this discussion. If, as you suggest, the baryons regularize the leptons and vice versa, don't you expect that the vacuum polarization effects of the baryons which become important in high energy electrodynamics may come out to be different, for example in sign, from what conventional theory predicts ?

W. Heisenberg. — If the baryons have opposite norm to that of the leptons, this should certainly have some influence on vacuum polarization effects of the baryons. Whether it would change the sign of these effects could probably be answered only by a careful investigation; at least I don't know the answer.

G. Chew. — If none of the strongly interacting particles is elementary, there should be no divergences in calculating electromagnetic mass splittings of isotopic multiplets, even with existing rules of electrodynamics. (Think, for example, of the Coulomb splittings of He^3 and H^3.) The principle that none of the strongly interacting particles is elementary is a feature of Heisenberg's theory and can be incorporated into the S-matrix theory — even though the notion is awkward in conventional (Lagrangian) field theory. One may hope therefore, not to need cut-offs when such mass calculations are finally carried out.

W. Heisenberg. — With regard to Wightman's remark, I would like to emphasize, that in my opinion wellknown difficulties of divergences, etc. are not primarily mathematical problems. Quantum field theory is in two respects essentially different in its physical content from quantum mechanics :

(1) In field theory the interactions are local while in quantum mechanics they are non local.

(2) In field theory we have three boundary conditions while in quantum mechanics we have only two (at infinitely small and in-

finitely large distances of particles). The third boundary condition in field theory refers to an infinite number of particles.

It is a problem of physics and not only of mathematics to see how such a profound change as the replacement of non local interactions by local ones will affect the mathematical representation. We cannot expect that such a change could be represented without radical changes in the formalism of quantum theory.

With respect to the problem of the third boundary we should try to get some information from the experiments on multiple production of particles.

Concerning the views expressed by Chew, I think I can agree in principle with most of his points. It perhaps should be possible in principle to construct the S-matrix simply by considering the group structure of the system of elementary particles and adding the postulate of unitarity and analyticity (except at points representing physical states) in order to represent causality. All this could probably be done without the use of an indefinite metric. On the other hand I cannot see how from a practical point of view one could deal with the enormous complexity of the analytic behaviour of S-matrix elements, without deriving them from some kind of " local interaction ". One should also keep in mind that by the postulate of analyticity one goes already away from the energy-shell into the more " local " regions; and discussing these regions without indefinite metric may be just as complicated as for instance a discussion of the fundamental laws of algebra without introducing $\sqrt{-1}$. But aside from these practical points I would approve of the views expressed by Chew.

L. Van Hove. — At center of mass energies of the order of 1 Gev and higher the vacuum polarization effects of strongly interacting particles become a more important part of radiative corrections than at low energies. Could Feynman comment on the implications of this fact if quantum electrodynamics would be " exact, although incomplete " ?

R.P. Feynman. — I do not want to give a precise answer to this question. It might be possible to write electrodynamics with e and μ, which would be complete but incorrect.

R. Peierls. — Even if there exists a modified form of electrodynamics in which there are no infinities it may not be easy to discover this from the study of electrodynamics by itself, because the modifications are likely to relate to extremely high energies (small distances) and their discussion becomes very academic. The similar difficulties in the theory of strong interactions are very substantial in the region of practical interest. It therefore seems likely that we may first discover the remedy (if it exists) in the strong interactions, and then recognize how to remove the troubles of electrodynamics by similar means.

A. Salam. — I would like to come back to Heitler's formula connecting energy and momentum for an electron and the violation of Lorentz invariance. If so perhaps we could have a discussion of the experimental situation.

W. Heitler. — Concerning the variation of mass with velocity : the formula given in my report for the self mass $\delta m(p)$ refers to free electrons only (not to bound electrons). The coefficients depend very much on the choice of the form factor and it may even be that a form factor exists for which δm is independent of p. Personnally, of course, I do not believe that any departure of this sort from invariance exists, this was merely meant to show what kind of results arise when one insists on the finiteness of the theory, and to suggest that such fundamental relation as Einstein's mass-velocity relation, should be checked as accurately as possible by experiments.

R.P. Feynman. — If the relativity formula is wrong, there are two masses that can be defined : the rest mass or self energy, and the coefficient of $v^2/2$ in the energy for small velocities or the " kinetic mass ". If gravity acts on energy, or, therefore rest mass, and " kinetic mass " represents inertia then we know from the experiment of Eotvös that they differ by less than one part in 10^{-8}. Therefore the electromagnetic part of the proton mass, being of order 10^{-3} must have the right coefficient of v^2 to order 10^{-5}. I pointed out yesterday that if the electron energy had a velocity dependence of the form $m_0(1 + \frac{1}{2} v^2/c^2 + a v^4/c^4)$ the Lamb experiment shows that $a = 3/8$ to one part in 10^{-6} so that if a fractional part of the mass of order 0.1 % is electromagnetic and varies in a

new way with velocity, then it must have the right coefficient of v^4 correct to one part in 10^{-3}.

L. Van Hove. — Present mass determination of the particles at ultrarelativistic energies are, I think, of an order of accuracy of a few percent. Is this sufficient for the question you raised ?

W. Heitler. — I think that an accuracy of 1% would not be good enough.

H.A. Bethe. — I want to draw your attention to *two experiments*, the first of which may be legitimate, in Heitler's sense, and the second may not be. The first experiment compares directly the rest energy of an electron to the kinetic mass of the electron; the kinetic mass is known with absolute accuracy; the rest energy can be measured by determining the energy necessary to produce an electron and a positron from radioactivity; this is known in some cases to a few parts in ten thousand and therefore the equality of rest mass and kinetic mass is established to a few parts in ten thousand. An even more direct and accurate way to measure the rest energy is by means of the wave length of annihilation radiation which has been measured by Dumond at Cal. Tech., also to an accuracy of a few parts in 10,000. The second experiment, I want to mention, refers to high energy; one can measure the total energy of the particle and measure the difference between the velocity of the particle and the velocity of light. This difference for synchrotrons giving electrons of 1 Gev is something like one part in 10^7, and one can measure this difference very accurately by measuring the total intensity of the light emitted in synchrotron radiation. One can measure this difference to something like 1% by measuring the intensity of the light; this has been done at 300 Mev and there is a project for 1 Gev but I am sure it would give the right result. This would give the velocity of the particle to one part in 10^9. Thus it is established with phenomenal accuracy that the velocity of an electron actually approaches that of light. The rest mass of a fast electron is measured by the difference $1 - \beta$ and is therefore known, for 300 Mev electrons, to an accuracy of about 1%. It is of course equal to the familiar kinetic mass. All the experiments I mentioned refer to free electrons.

WEAK INTERACTIONS

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1. What does it mean when we say that an interaction is weak ? Consider this interesting problem [1, 2] for the example of μ-decay,

$$\mu^- \to e^- + \upsilon + \bar{\upsilon} \tag{1}$$

which we know to be well described by the following effective interaction [3]

$$H = \frac{G}{\sqrt{2}} j_\lambda^{(e)}(x)\, j_\lambda^{(\mu)*}(x) + \text{H. c.} \tag{2}$$

$$j_\lambda^{(l)} = i\bar{l}\gamma_\lambda(1+\gamma_5)\upsilon, j_\lambda^{(l)*} = i\bar{\upsilon}\gamma_\lambda(1+\gamma_5)l, \tag{3}$$

$$G \approx 10^{-5} M^{-2}, \tag{4}$$

where M is the nucleon mass. A difference in dimensionality makes it impossible to compare G directly with, say, the dimensionless electric charge e. Rather do we meet in this problem, as in related ones, with two dimensionless parameters

$$g^2 = G L^{-2}\ pL, \tag{5}$$

where L is some characteristic length and p some momentum characteristic for the specific problem. For β-decay p lies in the Mev-region, for μ- and K-decays (for example) p is larger by two orders of magnitude. In short, for all weak decay and capture processes studied so far experimentally, we can imagine an L so chosen that

$$pL \ll 1, \tag{6}$$

$$g^2 \ll e^2. \tag{7}$$

Thus at least for a limited domain (6) of frequencies, (7) enables us to say that the interactions in question are much weaker than the electromagnetic ones. The question is whether and in what sense this is also true in the high frequency domain.

Before discussing this, it is well to recall the following. Quite apart from the difficulties which arise if one considers (2) as a field theoretical interaction, even the present more phenomenological view where H is considered as an effective S-matrix element cannot possibly be rigorous. For example, according to (2) the cross section for the scattering process $e^- + \upsilon \to \mu^- + \upsilon$ would indefinitely increase with the square of the c. m. momentum p. There exists therefore a p for which the unitarity limit is reached. This p is given by $4G^2p^2/\pi = \pi/2p^2$ or

$$p = (\pi^2/8G^2)^{1/4} \approx 300\,\text{Gev}. \tag{8}$$

Let us look upon L^{-1} as the effective momentum cut off of the Fermi interaction. In a future theory such a cut off should of course have a definite physical meaning. It may or may not be the mass of some intermediate boson (see below). In view of this incompleteness of the theory, arguments concerning the relative strength of the interactions in the high momentum region involve an element of guess work. Such guesses have been made ever since four Fermi-interactions were first written down for β-decay. For example the idea that weak interactions become effectively strong at high frequencies was at the root of early and abortive attempts to understand nuclear forces as the effect of virtual lepton pairs.

Presently we still do not have at our disposal any direct experimental results which bear on this question. Actually, in this respect these days mark a time of transition; we are about to move into the domain of high energy lepton experiments. However, there are two indirect arguments concerning the role of virtual high frequency lepton pairs which tend to indicate that also at high frequencies the weak interactions continue to deserve their name. First there is the smallness of the $K_1 - K_2$ mass difference,[4] the only pure second order weak interaction effect presently known (see below). Secondly there is the absence to considerable accuracy [5] of any parity-nonconserving contributions to nuclear interactions. In a theoretical estimate of the contribution to these effects due to the high frequency virtual lepton pairs, the previously mentioned

cut off enters and almost per definition one considers momenta up to pL being of order unity. Unless we are badly fooled, the smallness of the virtual effects then tells us that (7) should still be true even where (6) is not. Combining (7) with (4) and (5) gives $L \lesssim 30\ \text{Gev}^{-1}$. This in conjunction with (8) indicates that deviations from the phenomenological four Fermi-interaction could well appear at energies much lower than the incontrovertible breakdown energy.

2. The phenomena for which (7) is true, and for which in fact g^2 does not vary much relatively from process to process, are conveniently subdivided as follows :

(I) Leptonic $\Delta S = 0$ process : π, β, μ decays, etc. Also such reactions as $\Sigma^+ \to \Lambda + e^+ + \upsilon$, as yet unobserved;

(II) Leptonic $\Delta S \neq 0$ process : K_{l2}, K_{l3}, Σ or Λ decays into nucleon + lepton pair, etc.;

(III) Non-leptonic $|\Delta S| = 1$ process : $K_{\pi 2}$, $K_{\pi 3}$, (Σ or Λ) $\to$ nucleon + π, $\Xi \to \Lambda + \pi$, etc.

By far the most detailed information exists for (I), and, moreover, we have a fairly detailed theoretical picture of these reactions. Next in available data comes (III) for which in the last few months important new results have been obtained; some interesting rules have been found to work well for these processes. However, there is much less of an understanding of which goes on here than there is for (I). Finally, even experimentally, (II) is largely uncharted. After some general remarks about the totality of these processes the three classes will be discussed in a little more detail.

3. a) *P and C violation.* It is just five years ago that the suggestion of nonconservation of parity in weak interactions was made [6] and it is now known that this is the case for all three classes. Likewise, the violation of C-invariance is a general feature of all weak interactions. Parity violation manifests itself as an interference phenomenon between states of opposite parity and a degree of P-violation can therefore generally be expressed in terms of the relative magnitude of such interference. For the processes (I) P-violation invariably turned out to be maximal.[7] Limited information on (II) indicates

that this is again the case there;[8] so far the only experiment done which bears on this point is a study of $K^+ \to \mu^+ + \upsilon$.

Maximal P-violation also persisted in the first experiments on (III), where $\Lambda \to p + \pi^-$ was studied[9], and where also clear cut evidence for C-violation was found.[10] According to the conventional definition of the " up-down asymmetry parameter " α, this quantity equals minus the helicity of the decay baryon in the rest-system of the (unpolarized) decay hyperon. For the above reaction it was found that α_Λ is large and, after some confusion, that its sign is negative.[11] On the other hand α^+, the parameter for $\Sigma^+ \to n + \pi^+$ is found to be essentially zero within the errors.[12] Thus maximal P-violation is not a general rule. I shall come back later to these and related aspects of non leptonic hyperon decays.

b) *CP-invariance and T-invariance.*[13] The attractive idea of invariance under the combined inversion CP came to the fore in prospect of P-non conservation.[14] It is interesting to recall, though, that already in 1952 it was remarked [15] that " the disturbing possibility remains that C and P are both only approximate and CP is the only exact symmetry law ".

Certainly the most appealing assumption is that CP-invariance if true applies equally well to all weak interactions. It must be admitted, however, that so far there is no logical objection to the view that CP may hold for some but not all of the weak interactions. This is so because we may in practice neglect higher order interference effects between different weak interactions, as has recently been emphasized especially in connection with certain class (II) reactions.[16] It may therefore be best to state briefly what the situation is for each of the three classes separately. As we have no information on antibaryon decays, all our present knowledge stems from μ, π and K-mesons.

Concerning (I), all we know on the relative properties of the $\pi\mu e$ chain for positive as compared to negative charge is in good agreement with CP-invariance.[17] As to (III), CP-invariance forbids both 2π-decay modes [18] of the long lived K_2^0 and indeed no such disintegration has been recorded. I shall come back later (see Section 8 below) to some specific points concerning the validity of CP for class (II). For the moment it may merely be stated that

there is no direct positive evidence for CP-invariance for this case.

Within the accuracy of the experiment, time reversal invariance has been verified within (I) by the study of the decay of polarized free neutrons.[19] For (III) the fact that the up-down asymmetry parameters for $\Sigma^+ \to p\pi^0$, $\Lambda \to p\pi^-$ are so large in absolute magnitude [20] also tends to indicate that T is good for these processes. Information on the so-called β-parameter [21] will make this statement more quantitative. There are several ways [22] to check T for class (II); no information is as yet available.

c) *CPT-invariance.* Sufficient conditions for this invariance to hold are that one has a local theory invariant under the proper Lorentz group.[23] Principal consequences of this symmetry are the equality of masses of stable particles and of lifetimes of unstable ones as compared to their corresponding anti-particles.[24] As far as weak interactions are concerned the mass criterion is perhaps not quite critical, as such couplings would probably give very small mass differences ($\sim 10^{-5}$ ev) under any circumstance. For lifetime comparisons we can so far again use K, π, μ only. Here a technical, but not basic, impediment is the difference in absorptive properties of negative versus positive particles. Thus neither the $K^{\pm}$ not the $\pi^{\pm}$ lifetime equality has been established with an accuracy much better than ~ 10 per cent, due to the relatively large uncertainities for the negative particles. An experiment in progress on μ^- interactions in H gives better than 3 per cent for the $\mu^{\pm}$-equality.[25]

Quite apart from these experimental questions, it should be observed that invariance under [C times anything] suffices to get the mentioned mass and lifetime equalities which therefore hold true, if for example, CP-invariance but no T-invariance were to exist.

Thus the best way to find out about CPT is to study CP and T separately. Hence from the previous discussion it follows that CPT-invariance for $\Delta S = 0$ processes has been verified to some precision, while for $\Delta S \neq 0$ more information is necessary. The situation can perhaps best be summarized as follows. Even though the experimental information on CPT-invariance could well be improved on, there is no evidence of any kind which points to CPT-violation. On the other hand, a departure from CPT-invariance

would bring with it such drastic changes in our concepts that no serious speculation in this direction has been entertained. We hold on to CPT until forced otherwise. In this spirit we believe until further notice that CP- and T-invariance are the same thing.

d) *Two component neutrino theory.* Shortly following the suggestion of possible non-conservation of parity this theory was proposed by various authors.[14, 26, 27] It postulates a zero bare mass neutrino with interactions satisfying the invariance under $\upsilon \to \varepsilon\gamma_5\upsilon$, $\varepsilon = \pm 1$ (the V-A law, see below, specifies ε to be $= +1$). This implies strictly zero self-mass and magnetic moment for the neutrino.[28] All the present evidence is in accordance with the two component coupling which brings into evidence the maximal character of P-violation in leptonic processes.

e) *Local action of lepton currents.* It is generally assumed that, whatever may be the detailed structure for any process involving lepton pairs, the lepton current effectively acts locally, apart from small electromagnetic corrections. This means that if for example a $\bar{\upsilon}\, e^-$ pair is involved in a reaction, the fields $\bar{\upsilon}$ and e always have a common space time point as their respective argument in the effective interaction density. This lack of smearing out reflects our present belief that there exist no other forces between (e, υ) or (μ, υ) than the weak couplings. This simple dynamical postulate has interesting practical consequences. This was first recognized in connection with $K \to \pi e \upsilon$ disintegrations, where it was shown [29] that in this way the structure of the decay angular distribution is largely determined and is in fact uniquely fixed if the lepton current is a pure (V, A) mixture.[30]

Thus from the local lepton current assumption one can obtain useful information without recourse to any details of intervening strong interactions. This line of reasoning has recently been applied to the theory of high energy neutrino induced reactions for which a number of results were obtained which are largely independent of the complexity of the final states.[31]

f) *Lepton conservation.* All known lepton phenomena satisfy the following rule. Assign a lepton number $+1$ to μ^-, e^-, υ; -1 to their anti-particles; 0 to all other particles. Then the lepton

number is additively conserved in all reactions.[32] In practice this rule correlates helicity properties of different reactions. Concerning (II) much remains to be learned, but the one experiment [8] on K^+ conforms to this law.

g) *μ-e-universality*. As things stand today, it seems that μ and *e* are identical apart from their mass. In order of interaction strength, this is first of all true for the electromagnetic properties. Here, within the present accuracies of theory and experiment, careful studies have failed to reveal [33] anything "anomalous" about the μ. Secondly the weak interactions of class (I) likewise possess this property which mathematically amounts to invariance for an operation which involves the interchange of μ and *e*. This is shown for example from the decay ratio of π into $\mu\upsilon$ versus the long elusive [34] mode $e\upsilon$ and also from a comparison of β- to μ-decay rates to which I shall come back.

Quantitatively the situation is less clear for (II). The K_{e2} mode has not even been observed as yet and we know very little indeed about the μ- and *e*-leptonic decays of hyperons.[35] Thus the only source for a μ/e comparison so far is K_{e3} and $K_{\mu 3}$, where one can compare polarizations, spectra and rates. There is qualitative agreement for the ratio of rates.[36]

This so-called μ-*e* universality is perhaps the most intriguing aspect of the current state of particle physics. We may not call it a paradox, yet we would like to know "where the μ gets its mass from", we would like to correlate the μ-*e* mass difference with some other dynamically distinguishable feature. We meet here a challenge to the view that mass differences are due to distinct self-energy effects generated by interactions. In this respect the resolution of this question may conceivably contain a lesson for the strong interactions as well, where often certain coupling schemes are discarded on the mere ground that they do not generate (at least in principle) certain mass differences.

To what extent is the μ-*e* universality valid for high momenta ? The present situation here is not unlike the one for the weakness of weak interactions discussed previously. On the one hand, some bounds on possible deviations from universality can be obtained from virtual effects.[37] For example, the *g*-2 experiment indicates[33]

a coupling $<.003$ of the μ to an unknown field of $\sim$ nucleonic mass. Similarly one may use the $e\upsilon/\mu\upsilon$ ratio for π-decays to estimate limits on wave function renormalization effects due to hypothetical μ-couplings.[38] On the other hand, so far we do not have any information from real processes at high energies and the study of the validity of the μ-e universality is in fact one of the incentives to the experimental pursuit of high energy neutrino reactions.

4. *Class (I). The V-A law and the conserved vector current idea.* The foregoing remarks all had bearing on several or all of the three classes of the weak phenomena. I shall now have to discuss in more detail certain specific points concerning each of the classes separately. Thereupon I shall return to the question to what extent these pieces of information can be unified to an overall view of the weak interactions. Let us first consider the $\Delta S = 0$ leptonic processes.

At a time when the experimental situation was still fairly confused, several authors [39, 40, 41] proposed, on several grounds of simplicity, that like the interaction (2), the β-decay and μ-capture coupling should be of the V-A type. This has meanwhile come to be firmly established. The V-A law for all class (I) processes implies CP- and T-invariance, two-component neutrino coupling and lepton conservation with the added specification that the neutrino emitted in neutron β-decay has negative helicity. The effective β-interaction is of the form

$$G_V J^*_\lambda j_\lambda^{(e)} + \text{H. c.} \tag{9}$$

where J_λ is the $\Delta S = 0$ current involving strongly interacting particles only. The detailed structure of J_λ is not too well known. At low frequencies

$$J_\lambda = \frac{i}{\sqrt{2}} \bar{p} \gamma_\lambda (1 + \lambda \gamma_5) n + \ldots; \quad \lambda = -G_A/G_V, \tag{10}$$

where G_V, G_A are the Fermi, Gamow-Teller constants respectively.

At this stage a new theoretical idea was injected in the discussion. From a comparison of the μ-decay rate with Fermi type β-transitions is was found [40] that

$$G_V \cong G \tag{11}$$

to a rather close approximation. This near equality raised the question why the meson cloud of the nucleon does not introduce

any appreciable renormalization effects on G_V which have no counterpart for G. In other words, while one might easily be led to speculate that the corresponding bare constants were to be equal, it was not at once obvious why this should lead to a near equality of the effective constants. The remark was made [40] that this could be understood, very much like the (exact) equality of the effective electric charge of proton and electron, in terms of a conservation law for the β-decay vector current. Such a law is indeed not hard to come by. Due to the isotopic spin conservation of the strong interactions there exists an isotopic spin vector current T_λ which is conserved to order *e*. It was then proposed to identify the charge carrying Fermi currents $V^{\pm}{}_\lambda$ with the $T^{\pm}{}_\lambda$ components of T. The absence of G_V-renormalization follows up to electromagnetic corrections. It is interesting to note that in 1955, before the clarification of the β-decay situation, this possibility was already noted [42] as " of no practical significance but only of theoretical interest ".

While the above identification $V^{\pm}{}_\lambda = T^{\pm}{}_\lambda$ does of course not give us the detailed structure of the vector current,[43] this does lead to a non-trivial connection between V and the isotopic vector part of the electromagnetic current. In fact by a simple scaling of G to *e* one can now establish a direct relation between " isotopically related " Fermi β-transitions and γ-transitions. This was in particular discussed [44] for the respective (β^-, γ, β^+) transitions from the isotopic triplet (B^{12}, C^{12*}, N^{12}) to the singlet C^{12}. I understand that a recent Cal Tech experiment has shed light on this situation. I did not have a chance to see the details but I hope that we can hear more about this in the discussion.

If the conserved vector current idea proves to be correct, we then have an instance where the weak interactions serve as a probe for a symmetry property of the strong interactions. In this case the symmetry, charge independence, is one long familiar. I hold it for possible that in the future other weak phenomena may serve to help unravel further details of the presently so complex situation in strong interaction physics.

It is an attractive idea to comprise now all couplings of type (I) in the compact current × current form

$$G\, \mathcal{J}_\lambda^{\,*}\, \mathcal{J}_\lambda, \tag{12}$$

$$\mathcal{J}_\lambda = J_\lambda + j_\lambda^{(e)} + j_\lambda^{(\mu)} \tag{13}$$

which in particular takes account of the μ-e universality. New effects are hereby implied such as electron-neutrino scattering and weak P-violating nuclear interactions. The detection of the latter may not be far from present accuracies.[5]

The conserved current idea can also be tested at frequencies higher than those involved in the conventional decay processes : one can compare the (distinct) cross sections for $\upsilon + n \to e^- + p$ and $\bar{\upsilon} + p \to e^+ + n$ with electron-nucleon scattering at the same momentum transfer.[2]

5) *Class (III).* $|\Delta S| = 1$ *and* $|\Delta \mathbf{T}| = 1/2$. All known non-leptonic decays satisfy $|\Delta S| = 1$. The question is whether we have here a weak interaction law in the sense that $|\Delta S| > 1$ can only happen to higher order in a fundamental $|\Delta S| = 1$ weak interaction. A main problem in this context is to find the decay ratio $\Xi \to \Lambda + \pi$ to nucleon $+ \pi$. Far too little is known about cascade particles to make any strong statements.[45] There is however an indirect argument which leads one to suspect that $|\Delta S| = 1$ is indeed a general law.

This concerns the mass difference Δ of K_1^0 and K_2^0. It has been pointed out [46] that it is reasonable to expect the relation $\Delta \sim \tau_1^{-1}$ to hold (τ_1 is the K_1 lifetime) if all weak non-leptonic interactions satisfy $|\Delta S| = 1$ because in this case Δ is a second order weak effect, like the decay rate of K_1 itself. (The K_2 rate can be neglected on quantitative grounds.) However, most $|\Delta S| = 2$ interactions would contribute to Δ while leaving τ_1 unaffected.[47] If such interactions are comparable in strength to $|\Delta S| = 1$, a relation $\Delta \sim 10^6 \tau_1^{-1}$ would result. Experiment shows that such a large factor is out of the question.[48]

This is the second instance where this precious little Δ-effect leads to interesting conclusions. In passing it may be noted that Δ provides also the best known upper limit on " anti-gravity " phenomena.[49]

All known non-leptonic decays satisfy [47] $|\Delta \mathbf{T}| = 1/2$ to a very good approximation. The question is whether we have here a weak interaction law in the sense that $|\Delta \mathbf{T}| > 1/2$ can only happen as an effect of interference between a fundamental $|\Delta \mathbf{T}| = 1/2$ inter-

action and electromagnetism. (The latter gives of course $|\Delta \mathbf{T}| = 1$ transitions to order e.) In my opinion this question is still somewhat open. The main practical argument here comes from $K^+ \to \pi^+ + \pi^0$. This reaction is only possible via an effective $|\Delta \mathbf{T}| \geq 3/2$. It is actually inhibited [51] by a factor ~ 100 compared to $K_1{}^0 \to \pi^+ + \pi^-$ which goes via $|\Delta \mathbf{T}| = 1/2$. If only electromagnetism is invoked to generate the effective $|\Delta \mathbf{T}| > 3/2$, then the $K^+{}_{\pi 2}$ rate certainly contains a factor $(137)^{-2}$ as compared to $K^0{}_{\pi 2}$. Thus one needs a partially compensating enhancement factor to reach the ratio just quoted. There have been several suggestions at an explanation,[52] but I do not think that the relative $K^+{}_{\pi 2}$ rate has been understood in a definitive way. However, this may be, the evidence is impressive for at least a quite preponderant fundamental $|\Delta \mathbf{T}| = 1/2$ weak interaction.[53]

This rule leads to intriguing consequences in regard to the decays $\Sigma^+ \to p + \pi^0$, $n + \pi^+$; $\Sigma^- \to n + \pi^-$. Call the asymmetry parameters α^0, α^+, α^- respectively. It has recently been established [12, 20] that $\alpha^0 \sim +1$, $\alpha^+ \sim 0$. Using information on the relative rates,[54] $|\Delta \mathbf{T}| = 1/2$ predicts that also $\alpha^- \sim 0$. More than that, if $\Sigma^+ \to n\pi^+$ is nearly pure s (or p) wave,[55] then $\Sigma^- \to n\pi^-$ is nearly pure p (or s) wave. This is remarkable for the following reason.

It is an easy matter to write down a weak interaction which reproduces all the known properties of Σ-decays. The problem is, why whould the strong interactions respect these regularities ? For example let the weak interactions give s-wave $n\pi^+$, p-wave $n\pi^-$. The strong interactions allow Σ^+ to be part of the time $\Sigma^- (+ 2\pi^+$ for example). Then why does this not lead to appreciable p-wave admixture to the (presumed) s-wave decay of Σ^+ ? These and related questions have recently been treated in some detail.[54] I shall not enter here in a full discussion, but I would like to state the main idea of the analysis which is that this peculiar (but not paradoxical) parity situation possibly indicates the existence of a symmetry stronger than charge independence which is shared by the strong and the non-leptonic weak interactions. Put differently, these weak phenomena may possibly serve as a probe of more detail of the strong interaction dynamics. The theory predicts [54] that $\alpha_0 \sim -\alpha_\Lambda$ and $|\alpha_\Xi| \sim |\alpha_\Lambda|$. Both these predictions have recently been verified. [20, 45]

Concerning the dynamical origin of $|\Delta T| = 1/2$, perhaps the most common view is that this rule is understood neither better nor worse than the rule $|\Delta T| = 0$ for strong interactions. Alternatively, it has been suggested that the latter rule yields the former as a dynamical by-product via a presumed preponderance of specific strong interaction matrix elements.[56] This is hard to disprove, though especially the Σ-decay situation does not lend any particular support to this view.

6) *Class (II). $\Delta S/\Delta Q$ and related rules.* Experiment has yet to teach us a good deal before we can firmly answer the question : Do $|\Delta S| = 1$ and $|\Delta T| = 1/2$ apply not only to (III) but to (II) as well ?

No e- and μ-decay of any kind has so far been observed for Ξ's, hence from this corner we have no information at all about the validity of $|\Delta S| = 1$. Furthermore the K_1-K_2 mass difference argument [46] is readily seen not to apply to virtual leptonic transitions. While I have no particular predilections for any alternatives, it must be stated that a leptonic $|\Delta S| = 1$ rule remains entirely to be verified.

In order to discuss the $|\Delta T|$ situation, it is convenient to subdivide (II) further. For this class let ΔQ, ΔT_3, ΔS denote the difference in charge, 3-component of isotopic spin and strangeness respectively between the strongly interacting particles in the initial and final state. Clearly $\Delta Q = \Delta T_3 + 1/2 \cdot \Delta S$. Consider the physically interesting case $|\Delta Q| = 1$ only ($|\Delta Q| \geq 2$ has never been observed). Then

$$\Delta S/\Delta Q = \quad 1 \to \Delta T_3 = \pm 1/2 \to |\Delta T| \geq 1/2, \tag{14}$$

$$\Delta S/\Delta Q = -1 \to \Delta T_3 = \pm 3/2 \to |\Delta T| \geq 3/2. \tag{15}$$

Hence $|\Delta T| = 1/2$ implies $\Delta S/\Delta Q = +1$ but not vice versa; while $\Delta S/\Delta Q = -1$ is incompatible with $|\Delta T| = 1/2$. Thus (II) divides as follows :

(α) $|\Delta T| = 1/2$ trivially. Examples : K_{l2}, $\Lambda \to p + e^- + \bar{\upsilon}$.

(β) $\Delta S/\Delta Q = +1$ but $|\Delta T| = 1/2$ or $3/2$ so that a check on $|\Delta T| = 1/2$ can be made. Example : a sufficient condition [57] for this rule is that the rate ratio $K_2^0 \to \pi e \upsilon$ (summed over

both charge possibilities) versus $K^+ \to \pi^0 e^+ \upsilon$ shall equal two. Recent experiments seem to confirm this.[58]

(γ) $\Delta S/\Delta Q = -1$. Examples : $K^0 \to \pi^+ e^- \upsilon$, $\Sigma^+ \to n e^+ \bar{\upsilon}$. In all instances there are also many examples pertinent to neutrino induced reactions.

The following relations for K_1 and K_2-decays should also be noted. Let $R_i(\pi^\pm)$, be the rate for $K_i \to \pi^\pm e^\mp \upsilon$, $i = 1, 2$. It is a consequence of CP-invariance [57] that

$$R_1(\pi^-) = R_1(\pi^+),\ R_2(\pi^-) = R_2(\pi^+). \tag{16}$$

The absence of $\Delta S/\Delta Q = -1$ is a sufficient (but not necessary [59]) condition for

$$R_1(\pi^-) = R_2(\pi^-). \tag{17}$$

$\Delta S/\Delta Q = 1$ transitions certainly happen, the examples under (α) have been observed. Do $\Delta S/\Delta Q = -1$ reactions take place as well ? This is currently a much debated issue which is of great importance for our over-all view of the weak couplings. In order to give some background for this, I shall defer the discussion of the relevant experiment just a while longer (Section 8) and first mention some attempts at a unified view of all weak interactions.

7) *Attempts toward a synthesis.* In pursuing further the reasoning which led to (12), it was a natural speculation [40] to assume that this structure represents all weak interactions by an extension of the definition of J_λ to

$$\mathcal{J}_\lambda = J_\lambda + S_\lambda + j_\lambda^{(e)} + j_\lambda^{(\mu)} \tag{18}$$

where S_λ is a strangeness changing charge carrying current. A prototype term for S_λ would be $\sim \bar{p}\,\gamma_\lambda\,(1 + \text{const.}\,\gamma_5)\Lambda$. In principle, the reactions (II) are generated by the cross terms $S^*(j^{(e)} + j^{(\mu)}) + \text{H. c.}$ — which implies full μ-e universality — and (III) by $S^*J + \text{H. c.}$ From the reasoning in Section 6 it follows that S must contain $\Delta S/\Delta Q = +1$ terms. As the same S intervenes in both (II) and (III) reactions, the arguments of Section 5 now tell us that S may not contain $\Delta S/\Delta Q = -1$ terms. For if the converse were true, unwanted [46] non-leptonic $\Delta S = 2$ couplings would be generated by S^*S. In turn, it follows from (12) and (18) that $\Delta S/\Delta Q = -1$ reactions of class (II) are thereby forbidden.

The absence of these last processes therefore now becomes crucial for the validity of the simple structure (12), (18).

Next a remark on hyperon β-decay. We can calculate the probability for Λ_β-decay, say, on the following naive picture. Let in (18) all relative constants referring to J and S be near unity. λ of Equ. (10) is one such constant,[43] the relative weight of J to S an (unknown) other. I call this picture naive for two reasons. First there may be large renormalizations of S relative to J and, in view of large momentum transfers, induced terms may be more important for Λ_β than for, say, neutron β-decay.[60] The second reason for calling it naive is that it is wrong. The observed rates seem to be smaller by an order of magnitude compared to the naively calculated ones.[61] If we give (18) the benefit of the doubt, S_λ must for whatever reason contain some cofactor not so near unity compared to J_λ.

It was soon recognized [62, 63] that, whatever the detailed structure of J and S may be, (12), (18) are incompatible with $|\Delta \mathbf{T}| = 1/2$ for (III), and it was proposed to remedy this by introducing neutral currents as follows.

$$\text{Weak interactions} = G\left[\mathcal{J}_\lambda^{*}\mathcal{J}_\lambda - \frac{1}{\sqrt{2}}\mathcal{J}_\lambda^{(0)*}\mathcal{J}_\lambda^{(0)}\right], \quad (?) \qquad (19)$$

$$\mathcal{J}_\lambda^{(0)} = J_\lambda^{(0)} + S_\lambda^{(0)} \qquad (20)$$

If we economize by postulating (J_λ, J_λ^{*}, $J_\lambda^{(0)}$) to form an isovector, then (S_λ, $S_\lambda^{(0)}$) form an isospinor [63] if (19) shall conform to $|\Delta \mathbf{T}| = 1/2$. A non-paradoxical but uncomfortable disparity exists between (18) and (20) in that $\mathcal{J}_\lambda^{(0)}$ may essentially not be given a neutral lepton part on the ground that this would lead to contradictions with observations on strangeness changing decays.[63, 64]

Neutral currents play no role for the reactions (II) (with $|\Delta Q| = 1$). However, as S_λ is an isospinor and the charged lepton current per definition an isoscalar, this implies that, according to (19), the reactions (II) should not only obey $\Delta S/\Delta Q = +1$ (as we have seen already) but more specifically $|\Delta \mathbf{T}| = 1/2$.

The question arises whether (19) yields further specific results for non-leptonic decays, in particular whether the intriguing relations $\alpha^0 \sim -\alpha_\Lambda$, $\alpha_+ \sim \alpha_- \sim 0$ can be explained. Neither a proof nor a disproof of this has been given so far.

It should be added that 1) other generalizations of (12) have been contemplated,[65] 2) in a recent attempt [54] to understand the parity situation for non-leptonic decays the structure (19) cannot be maintained as a complete description. In summary, the correctness of the generalization from (12) to (19) remains so far an open question. The experiment to be mentioned next could tip the balance.

8) *A crucial K^0-experiment.*[66] A study was made of the $e3$-decay modes of neutral K-particles which aroused some suspicion that (α) the relation $R_1(\pi^-) + R_1(\pi^+) = R_2(\pi^-) + R_2(\pi^+)$ which follows from (16) and (17) is not satisfied, (β) the reaction $K^0 \to \pi^+ e \bar{\upsilon}$ is perhaps not forbidden. I know it to be in the spirit of those involved in this investigation when I say that more experiments will be necessary before a definitive answer to either question can be given. For the present it suffices to state that if either or both of these statements turn out to be true, there will be evidence for $\Delta S/\Delta Q = -1$ transitions. It would follow that $|\Delta \mathbf{T}| = 1/2$ is not valid for class (II) and hence that the structure (19) cannot possibly be right (even the charged current terms alone would be incorrect).

Suppose then for a moment that both subclasses of (II) corresponding to $\Delta S/\Delta Q = +1$ and -1 are indeed present in comparable strength. Along with pure $\Delta S/\Delta Q = +1$ reactions (such as Λ_β-decay) there should exist pure $\Delta S/\Delta Q = -1$ processes (such as $\Sigma_\beta{}^+$-decay). In addition, however, there are the $e3$-modes of $K_1{}^0$ and $K_2{}^0$ where the special situation arises of interference between $\Delta S/\Delta Q$ interactions of both types. It has been stressed [16] that the study of this interference provides unique possibilities for the verification of the validity of CP- or rather T-invariance. Indeed, whenever interference between couplings takes place the question arises of the relative reality of coupling constants (or effective coherent partial amplitudes). Thus while almost always T-tests are internal to classes (I) or (II) or (III) with either $\Delta S/\Delta Q = +1$ or -1, the $K_1{}^0$ and $K_2{}^0$ offer the one known example to check the relative reality of couplings corresponding to distinct weak interactions.[67]

9) *Question of intermediate vector bosons.* Interactions of the structure (12) and generalizations of the kind discussed previously invite speculation [31] that the weak interactions are mediated by heavy bosons of spin 1. If such mesons (call them W) are coupled with a (dimensionless) strength g to the (V-A) current $\mathcal{J}_\lambda$ then (12) represents in the low frequency limit the short-range analog of an effective Møller-matrix element for electromagnetic interaction, where $G = g\mu^{-2}$, μ is the W-mass. A lower bound $\mu >$ K-mass is set by the K-particle stability. An added interest in such speculations comes from the fact (Section 1) that here we have at least one possible way out of the inadmissibility of the local four-Fermi interaction at high frequencies.

If only (12) were so treated, two W's (electric charge $\pm e$) would suffice. Some simple properties of these, such as an upper bound on their lifetime ($\lesssim 10^{-17}$ sec), their influence on the μ-spectrum, etc. are readily estimated.[31] If (19) were true, two additional neutral W's would need to be introduced. The resulting 4W-scheme has been discussed in detail.[31] As we have seen there may possibly be something amiss with (19) and, if vector mesons are involved at all, one might have to contemplate more elaborate W-systems.[68] For the moment it seems best, therefore, to confine the attention to some points which may qualitatively if not quantitatively survive if there is any truth at all to the general idea of vector bosons.

One such qualitative point is the very relation $g = \mu\sqrt{G}$ which implies that W's are coupled with a strength the " square root of weakness ". This implies [69] that, if μ is not too large there may be a considerable enhancement in the cross section for high energy neutrino-nuclear interaction because of the occurrence of

$$\upsilon + Z \to W^+ + l^- + \begin{cases} Z & \text{(coherent)} \\ \\ \text{star (incoherent).} \end{cases}$$

Far from threshold the respective cross sections are $\sim Z^2 \cdot 10^{-37}$ and $Z \cdot 10^{-37}\,\text{cm}^2$ respectively. In this energy region closed expressions for the cross sections have been given. Near threshold, numerical evaluations have been made [69] using electromagnetic form factor information, and for several values of μ. Enhancement factors ~ 10 to $10Z$ for neutrino capture may be anticipated if W exists and $\mu \sim$ nucleon mass.

Another problem which has received special attention in connection with the possible existence of W's is that of the electromagnetic decays of μ-mesons.

10) *The two neutrino question.* The process $\mu \to e\gamma$ has never been seen, its occurrence is $\lesssim 10^{-6}$ times the normal mode.[70] This low (if not zero) relative rate is hard to understand [71] if $W^{\pm}$ exist, essentially because the W-cloud around the μ can shake off a photon and so lead to $e\gamma$. The corresponding branching ratio $e\gamma/e\upsilon\bar{\upsilon}$ is evidently independent of the weak coupling constant and reasonable estimates [72] put it at $\sim 10^{-4}$. Thus regardless of whether $e\gamma$ is absolutely forbidden, the observed low rate poses a problem. The same is true [73] in regard to the somewhat different problem of the absence of $\mu^- + N \to e^- + N$. While historically the "$e\gamma$-problem" arose in connection with the $W^{\pm}$-postulate, it was soon realized that essentially any effective non-locality of the four Fermi interaction poses the same question on the grounds of gauge invariance.[74]

Several speculations have been made about a further conservation law in lepton physics [75] which forbids the unwanted modes. The only effective assumption seems to be that there exist two neutrinos.[71] Indeed, if in (2) the neutrino accompanying the μ (call it υ_1) is not identical with the one that goes with $e(\upsilon_2)$, normal μ-decay is not changed, yet at the same time the mechanism of virtual neutrino emission and reabsorption which leads to the $e\gamma$ problem for $\upsilon_1 = \upsilon_2$ is now eliminated.

How does $\upsilon_1 \neq \upsilon_2$ affect previous results ? Class (I) reactions require them to have the same helicity. Lepton conservation demands them to have the same lepton number. One may argue [76] about the mass of υ_1, still zero mass seems most natural for any and all neutrinos.

Suppose all this to be true. Then we meet with a situation very strongly reminiscent of the mysterious μ-e universality — only more so. We face the possibility of two particles which are identical in all known quantum numbers; they are only different in their pairing to μ and e. This may seem hard to swallow, but so is the low $e\gamma$ rate.

However this may be, if $\upsilon_1 \neq \upsilon_2$, it would be somewhat of a

relief if υ_1 and υ_2 would do something else than just forbid some unwanted processes. This thought has led some to entertain the possibility that in K-decays υ_1 and υ_2 change roles.[59, 77] If true, the occurrence of two neutrinos would then conceivably be connected with some analog of strangeness for leptons. Just as (strongly) π is equivalent to $\overline{N}N$ or $\overline{Y}Y$ ($Y = \Sigma$ or Λ), while K is equivalent to $\overline{N}Y$ (or $\overline{Y}N$), so (weakly) π would be equivalent to $\overline{e}\upsilon_2$ or $\overline{\mu}\upsilon_1$ and K to $\overline{e}\upsilon_1$ or $\overline{\mu}\upsilon_2$. If this " neutrino flip " occurs, the $e\gamma$-process would no longer be absolutely forbidden, but it could only occur to third order in weak interactions.[59]

While in 1961 the question of one or two neutrinos is still a favorite topic for placing bets, the issue may rather soon be settled by high energy neutrino experiments. One will let a υ_1 beam (prepared from $\pi \to \mu + \upsilon_1$) hit matter and one will look for the reactions $\upsilon_1 + Z \to e^-$ (or μ^-) + (star). Comparable rates for the e and μ-reaction will indicate $\upsilon_1 = \upsilon_2$. Absence of the e reaction will constitute evidence for $\upsilon_1 \neq \upsilon_2$. If the latter is found true, one will then check the " neutrino flip " idea with a K-beam, for example.

The two neutrino experiment constitutes the first target for high energy lepton physics. The next one, to find evidence concerning the possible existence of W's is already much harder with the present means.

11) *Conclusion.* I have tried to describe how for the " classical " weak interactions ($\Delta S = 0$) a rather solid theoretical structure has been arrived at. For $\Delta S \neq 0$ the situation is less clear. One wonders whether the low rates for leptonic hyperon decays have a rather trivial explanation or whether something new is going on here. One wonders whether the parity situation for the non-leptonic decays is more or less a dynamical accident or whether it is a subtler clue. Things do not seem to click as yet when, impelled by the common order of strength of all weak phenomena, one tries to attain a unified view of all weak interactions.

I have tried to indicate why and where lepton physics moves into new territory of higher energies, and why only few years after the discovery of " the " neutrino,[78] one wishes to find out if there

possibly are even two kinds of them. Concurrent with this interest in the higher frequencies are studies on the limitations of the four Fermi interaction, also in relation to higher order effects of weak interactions.[79]

To conclude I would like to make three not unrelated remarks of a more general nature :

a) It is the ideal of particle physics to arrive at a unified description not only of all weak interactions but of all interactions. The attempts at classification schemes of strongly interacting particles and their couplings represent another partial approach in this direction. It is significant that the place of leptons in such schemes is most often not obvious, to say the least. The one or two neutrino question is of great importance also in this respect. I would even venture further and say that the incorporation of the leptons may well lead the way to understanding the strong interactions themselves. Indeed, as has been said earlier several times, weak interactions may be important probes of strong ones. In this context the greatest challenge is to find a rational way of incorporating the baryon conservation law.

b) There is evidently a profound connection between the strength of interactions and their symmetry. In particular, for weak interactions we have become familiar with P-nonconservation. But we have not understood it. It may be too early yet to tackle the famous question : if CP were to be the universal law, then why do strong and electromagnetic interactions respect P as they seem to have every intent of doing ? Yet this or some such question is there, and it seems reasonable to ask of a future dynamics to answer it.

c) I have tried to review how, by staying close to the phenomena, certain rules such as $|\Delta S| = 1$ or $|\Delta \mathbf{T}| = 1/2$ have come to be considered. The quest for such and related rules has been very useful. But of course such rules do not explain anything. They only simplify the task to find what has to be explained.

What seems particularly dark to me is the notion of approximate conservation laws with which the rules just mentioned are intimately connected. To my mind we are here at the most important clues of all for further progress. I say this in the conviction that there will be no approximate laws at all once we learn to look at things

in the right way. Today we say that this or that law becomes rigorous in the approximation where we switch off this or that coupling. This is manifestly unphysical. I rather believe that we should look for dynamical ways of " transforming away " certain interactions. The following may serve as a (possibly inapropos) analogy. In the framework of general relativity we may say : Lorentz invariance is not a rigorous law. But it becomes rigorous in the limit where we switch off the gravitational coupling. This is mathematically not wrong. But it largely ignores of course the physical content of the theory which only becomes clear after we realize that, even in the presence of gravitation, Lorentz invariance is a locally true law because we can locally transform gravitation away. Maybe this is the kind of thing we should in some ways learn to do for other interactions. Maybe also that differential geometry does not provide the appropriate tools for this.

But now I am getting too foggy, and I had better stop.

FOOTNOTES AND REFERENCES

(1) B. Ioffe, *JETP*, **38**, 1608, 1960 [translation : *Soviet Phys. JETP*, **11**, 1158, 1960].

(2) T.D. Lee and C.N. Yang, *Phys. Rev. Letters*, **4**, 307, 1960; T.D. Lee, Particle Physics Seminar, CERN, 1961.

(3) $\hbar = c = 1$. $l = e$ or μ. H.c. = Hermitian conjugate. In (3) and in what follows the particle symbol is often used to denote the corresponding field.

(4) This is relevant to the extent that strangeness changing weak interactions are also described by four-Fermi interactions.

(5) Experimental : see e.g. R. Haas, L. Leipuner and R. Adair, *Phys. Rev.*, **116**, 1221, 1959, where references to earlier work are also found.
Theoretical : see e.g. R. Blin-Stoyle, *Phys. Rev.*, **118**, 1605 (1960); **120**, 181 (1960).

(6) T.D. Lee and C.N. Yang, *Phys. Rev.*, **104**, 254, 1956. See also some interesting remarks by E. Purcell and N. Ramsey, *Phys. Rev.*, **78**, 807, 1950.

(7) For surveys of the stormy developments in relation to processes of this kind, see E.J. Konopinski, *Ann. Rev. of Nucl. Science*, **9**, 99, 1959, and the article by Miss C.S. Wu in the Pauli Memorial Volume " Theoretical Physics in the Twentieth Century ", Interscience Publ., 1960, p. 249. A paper by L. Grodzins, *Proc. Nat. Ac. Sciences*, **45**, 399, 1959, is of historical interest.

(8) C. Coombes *et al.*, *Phys. Rev.*, **108**, 1348 (1957).

(9) F. Crawford *et al.*, *Phys. Rev.*, **108**, 1102, 1957; F. Eisler *et al.*, *Phys. Rev.*, **108**, 1353, 1957.

(10) R. Gatto, *Phys. Rev.*, **108**, 1103, 1957.

(11) R. Birge and W. Fowler, *Phys. Rev. Letters*, **5**, 254, 1960; J. Leitner *et al.*, *ibid.*, **7**, 264, 1961; E.F. Beall *et al.*, *ibid.*, 7, 285, 1961.

(12) B. Cork *et al.*, *Phys. Rev.*, **120**, 1000, 1960.

(13) This and the subsequent discussion on CPT-invariance is treated here in a little more detail than in the talk given at the Conference in order to reply to questions raised by C. Møller and E. Wigner. I want to thank G. Feinberg for a discussion on these points.

(14) C.N. Yang, Proc. of the Seattle Congress, September 1956, *Rev. Mod. Phys.*, **29**, 231, 1957; L. Landau, *Nuclear Physics*, **3**, 127, 1957.

(15) G. Wick, A. Wightman and E. Wigner, *Phys. Rev.*, **88**, 101, 1952, footnote 9.

(16) R. Sachs and S. Treiman, " Is CP conserved in neutral K-meson decay ? " To appear in *Phys. Rev. Letters*.

(17) See reference 7 and especially a more recent paper by M. Bardon *et al.*, *Phys. Rev. Letters*, **7**, 23, 1961.

(18) An upper limit of 0.3 per cent for the relative probability of $K_2 \rightarrow \pi^+ + \pi^-$ is set by D. Neagu *et al.*, *Phys. Rev. Letters*, **6**, 552, 1960. For the theory see L. Landau (ref. 14) and also R. Gatto, *Phys. Rev.*, **106**, 168, 1957; A. Pais and S. Treiman, *ibid.*, **106**, 1106, 1957. It has been stated (T.D. Lee, R. Oehme and C.N. Yang, *Phys. Rev.*, **106**, 340, 1957) that the experimental situation could also be unterstood by simply using the fact that the $\pi^+\pi^-$-decay mode is so preponderant and by not necessarily insisting on strict CP-invariance. However, S. Weinberg, *Phys. Rev.*, **110**, 782, 1958, observed that the absence of both $\pi^+ + \pi^-$ and of $2\pi^0$-decay forces one essentially back to CP-invariance. But in turn, this last argument loses its strength to the extent that the relative reality of the two decay amplitudes in question is dictated by $|\Delta \mathbf{T}| = 1/2$.

(19) M. Clark *et al.*, *Phys. Rev. Letters*, **1**, 100, 1958; M. Burgy *et al.*, *ibid.*, **1**, 234, 1958. Quantitatively, a T-experiment determines the extent to which a certain phase difference conforms to its value prescribed by T-invariance. In the present case the agreement is within $\pm 8^{0}$.

(20) E.F. Beall *et al.*, *Phys. Rev. Letters*, **7**, 285, 1961.

(21) $\beta = 2\,\mathrm{Im}\,S^{*}P/(|S|^{2} + |P|^{2})$ where S, P are the amplitudes of the $l = 0$, 1 (and $j = 1/2$) final states respectively.

(22) See for example J. Sakurai, *Phys. Rev.*, **109**, 980, 1958; S. Okubo, *ibid.*, **109**, 984, 1958; A. Pais, *Phys. Rev. Letters*, **3**, 242, 1959.

(23) W. Pauli in " Niels Bohr and the Development of Physics ", London, Pergamon Press 1955, p. 30. This article contains critical reference to other independent contributions. The relation between the CPT theorem and the postulates of quantum field theory has been clarified by R. Jost, see the Pauli Memorial Volume (cf. ref. 7) pp 116 ff. He derives the theorem by working with the complex extension of the Lorentz group in which PT can be continuously connected with the identity.

(24) T.D. Lee, R. Oehme and C.N. Yang, *Phys. Rev.*, **106**, 340, 1957; G. Lüders and B. Zumino, *Phys. Rev.*, **106**, 385, 1957.

(25) According to P. Norden, *Phys. Rev.*, **123**, 2168, 1961, the weighted average for K− is $(1.24 \pm .07) \cdot 10^{-8}$ sec. For K+ see W. Barkas and A. Rosenfeld's Table, Proc. Rochester 1960, p. 878. For π+ see H. Anderson *et al.*, *Phys. Rev.*, **119**, 2050, 1960; for π−, L. Lederman *et al.*, *Phys. Rev.*, **83**, 684, 1957. I was informed of the preliminary results of the μ−-experiment in a private communication from J. Rosen, E. Blesser, J. Rothberg and L. Lederman.

(26) A. Salam, *Nuovo Cim.*, **5**, 299, 1957.

(27) T.D. Lee and C.N. Yang, *Phys. Rev.*, **106**, 340, 1957.

(28) In early discussions on P-violation the two component postulate was sometimes phrased by saying that the free neutrino obeys the two-component Weyl equation (H. Weyl, *Z.Phys.*, **56**, 330, 1929) and that in this way one " understands " P-violation in leptonic phenomena. It was soon realized however that this formulation (which seemed to put leptonic and non-leptonic weak phenomena on a somewhat different footing) does not put the proper emphasis on the fact that P-violation is a property of interaction rather than of free fields. See e.g., K.M. Case, *Phys. Rev.*, **107**, 307, 1957.

(29) A. Pais and S. Treiman, *Phys. Rev.*, **105**, 1616, 1957. See also N.N. Bogolubov *et al.*, *Nucl. Phys.*, **5**, 383, 1958.

(30) Or, for that matter, if it were pure S or pure T. For experimental results on this point see D. Luers *et al.*, *Phys. Rev. Letters*, **7**, 255, 361, 1961; J. Brown *et al.*, *ibid.*, **7**, 423, 1961.

(31) See reference 2 and T.D. Lee and C.N. Yang, *Phys. Rev.*, **119**, 1410, 1960, especially eqs. (60) - (63).

(32) T.D. Lee and C.N. Yang, reference 27.

(33) G. Charpak *et al.*, *Phys. Rev. Letters*, **6**, 128, 1961.

(34) First observed by T. Fazzini *et al.*, *Phys. Rev. Letters*, **1**, 247, 1958. For recent data on the μ/e branching ratio see T. Kinoshita, *Phys. Rev. Letters*, **2**, 477 (1959), theory : H. Anderson *et al.*, *Phys. Rev.*, **119**, 2050, 1960, experimental.

(35) The first μ-decay event has only very recently been observed : F. Eisler *et al.*, *Phys. Rev. Letters*, **7**, 136, 1961.

(36) Theory : F. Zachariasen, *Phys. Rev.*, **110**, 1481, 1958; R. Gatto, *Phys. Rev.*, **111**, 1426, 1958. Most recent experiments : B. Roe *et al.*, *Phys. Rev. Letters*, **7**, 346, 1961.

(37) V. Berestetskii *et al.*, *JETP*, **30**, 783, 1956 [translation : *Soviet Physics JETP*, **3**, 761, 1956]; B. de Tollis, *Nuovo Cim.*, **16**, 203, 1960.

(38) G. Feinberg, private communication.

(39) R. Marshak and G. Sudarshan, Padua-Venice Conference on Elementary Particles, 1957; *Phys. Rev.*, **109**, 1860, 1958.

(40) R. Feynman and M. Gell-Mann, *Phys. Rev.*, **109**, 193, 1958.

(41) J. Sakurai, *Nuovo Cim.*, **7**, 649, 1958.

(42) S. Gershtein and J. Zeldovich, *JETP*, **29**, 698, 1955 [translation : *Soviet Physics JETP*, **2**, 576, 1956].

(43) As to the axial vector current, no satisfactory "explanation" exists for the actual value $\simeq 1$, 2 of λ.

(44) M. Gell-Mann, *Phys. Rev.*, **111**, 362, 1958.

(45) The most recent information on Ξ-decay is found in W. Fowler *et al.*, *Phys. Rev. Letters*, **6**, 134, 1961.

(46) L. Okun and B. Pontecorvo, *JETP*, **32**, 1587, 1957 [translation : *Soviet Physics JETP*, **5**, 1297, 1957].

(47) There are special situations where this is not true; namely for a $\Delta S = 2$ weak interaction which transforms like a pseudoscalar and hence cannot contribute to mass which is scalar. Such interactions would generate P-conserving decays $\Xi \rightarrow$ nucleon $+ \pi$. See S. Glashow, *Phys. Rev. Letters*, **6**, 196, 1961.

(48) F. Müller *et al.*, *Phys. Rev. Letters*, **4**, 418, 1960; R. Birge *et al.*, Proc. Rochester 1960, p. 601 (Interscience Pub.).

(49) M.L. Good, *Phys. Rev.*, **121**, 311, 1961.

(50) M. Gell-Mann and A. Pais, Proc. Glasgow Conference 1954, p. 342 (Pergamon Press).

(51) See W. Barkas and A. Rosenfeld's Table, Proc. Rochester 1960, p. 878 (Interscience Pub.).

(52) M.L. Good and W. Holladay, *Phys. Rev. Letters*, **4**, 138, 1960, discuss enhancement due to a strong low energy $T = 2$, $J = 0$ $\pi\pi$ interaction. A. Pais, *Rev. Mod. Phys.*, **33**, 493, 1961 (esp. p. 497) discusses a possible non-electromagnetic deviation from $|\Delta T| = 1/2$ in connection with strong interaction symmetries.

(53) For a survey, see L. Okun, Proc. Rochester 1960, p. 743 (Interscience Pub.).

(54) For a recent theoretical summary see A. Pais, ref. 52.

(55) Which can be (but has not yet been) decided by a polarization experiment, see e.g. A. Pais, *Nuovo Cim.*, **18**, 1003, 1960, Eq. (15).

(56) This possibility is surveyed by R. Dalitz, *Rev. Mod. Phys.*, **31**, 823, 1959.

(57) S. Okubo *et al.*, *Phys. Rev.*, **112**, 665, 1958. That this is not a necessary condition is discussed e.g. by R. Behrends and A. Sirlin, *Phys. Rev. Letters*, **5**, 476, 1960.

(58) D. Neagu *et al.*, see ref. 18. No error is quoted.

(59) G. Feinberg, F. Gürsey and A. Pais, *Phys. Rev. Letters*, **7**, 208, 1961.

(60) Cf. C. Albright, *Phys. Rev.*, **115**, 750, 1959.

(61) W. Humphrey *et al.*, *Phys. Rev. Letters*, **6**, 478, 1961.

(62) R. Sawyer, *Phys. Rev.*, **112**, 2135, 1958.

(63) S. Treiman, *Nuovo Cim.*, **15**, 916, 1960.

(64) For numerical estimates see ref. 31, Section VIII. S. Bludman, *Phys. Rev.*, **115**, 468, 1959, notes that neutral leptons currents are not necessarily ruled out for the separate class of strangeness conserving lepton reactions.

(65) For example, B. d'Espagnat, *Nuovo Cim.*, **18**, 287, 1960, who considers the absence of neutral lepton currents as a main reason for an alternative approach.

(66) Camerini, Fry, Natali, Birge, Ely, Powell, Baldo-Ceolin, Ciampolillo, Fabri and Husita, to be published. At the Aix-en-Provence Conference, Sept. 1961, preliminary results were presented by Mrs. Baldo-Ceolin and by J. Fry.

(67) As is noted in ref. 16 the usual CP-invariant definition of K_1^0 and K_2^0 should still be valid to a high accuracy in any circumstance.

(68) It is obvious that to any coupling $\sum_i J_\lambda^{(i)} J_\lambda^{(i)}$, $i = 1, 2, \ldots$ there corresponds a conceivable W-set.

(69) See T.D. Lee, Proc. CERN Conference on Very high Energy Phenomena, 1961, p. 44. Also T.D. Lee, Proc. Rochester Conference 1960, p. 567. I understand that B .Pontecorvo and R. Ryndin reported on similar work at the Kiev Conference, 1959.

(70) D. Berley *et al.*, *Phys. Rev. Letters*, **2**, 357, 1959; S. Frankel *et al.*, *Phys. Rev.*, **118**, 589, 1960.

(71) B. Pontecorvo, *JETP*, **37**, 1751, 1959 [translation : *Soviet Phys. JETP*, **10**, 1236, 1960]. See also S. Bludman, *Bull. Am. Phys. Soc.*, **4**, 80, 1959.

(72) G. Feinberg, *Phys. Rev.*, **110**, 1482, 1958. The ratio is ambiguous due to divergence difficulties, the order 10^{-4} is arrived at by a reasonable cut off.

(73) Theory : S. Weinberg and G. Feinberg, *Phys. Rev. Letters*, **3**, 111, 244, 1959. Experimental : see M. Conversi *et al.*, " Search for conversion of muons into electrons ", CERN, preprint.

(74) See for example reference *1*, where this problem is discussed in connection with higher order weak interactions. This great generality of the argument was emphasized at the Solvay meeting by J.R. Oppenheimer.

(75) Either an additive or a multiplicative law can be conceived. These two possibilities are distinguishable by the fact that the multiplicative law allows, the additive law forbids the conversion muonium $\rightleftharpoons$ anti-muonium. See G. Feinberg and S. Weinberg, *Phys. Rev. Letters*, **6**, 381, 1961; N. Cabibbo and R. Gatto, *Nuovo Cim.*, **19**, 612, 1961.

(76) J. Bahcall and R. Curtis, *Nuovo Cim.*, **21**, 422, 1961.

(77) S. Bludman, *Phys. Rev.*, **124**, 947, 1961.

(78) For a survey of neutrino experiments see F. Reines, *Ann. Review Nucl. Sci.*, **10**, 1, 1960.

(79) See refs. 1 and 2. Also A. Brodskii, *JETP*, **30**, 322, 1960; Y. Zel'dovich and A. Perelomov, *ibid.*, **39**, 1115, 1960. [Translations : *Soviet Physics JETP*, **12**, 229 and 777, 1960, respectively.] Also contributions by S. Berman and by J. Nilsson and R. Marshak to the Proc. of the CERN Conference on very high energy phenomena, June 1961.

Discussion du rapport de Pais

J.R. Oppenheimer. — I should like to make some comment on behalf of Lee. I think that he would be as pleased as all of us with Pais' report; but on one point his view is a little broader. He does not regard the absence of $\mu \to e + \gamma$ and the need for two neutrinos as implied only by a charged intermediate vector boson, but by the very fact that the 4-fermion coupling cannot be strictly local. This implies extended currents and these will radiate. I cannot make this a quantitative statement but if $\mu \to e + \gamma$ is really absent one would have a problem with or without the charged boson.

A. Pais. — I have had no opportunity at this short notice to get acquainted with the recent Cal Tech experiment concerning the conserved current. Would Feynman or Gell-Mann give us some details ? How sensitively does this experiment test the hypothesis ?

M. Gell-Mann. — According to the conserved vector current theory, the MI matrix element for the γ-decay is just proportional to the forbidden Fermi matrix element in the β transitions. Thus the latter can be calculated. The dominant term in the β-decays is, however, the allowed Gamov-Teller matrix element that comes from the axial vector current. The rate of β-decay measures this allowed matrix element. The interference between the two produces a small spectrum anomaly in the form of a factor $1 + \alpha E$ in B^{12} decay and $1 - \alpha E$ in N^{12}. The other forbidden corrections are the same in B^{12} and N^{12}. Thus we can test the C.V.C. theory by measuring the ratio of spectrum anomalies in the two decays and comparing the result with the predicted value $1 + 2\alpha E$.

The experiment was tried for a couple of years at Cal Tech with inconclusive results. During the last year Meyer-Kuckuck and Michel, using the same β-ray spectrometer, have repeated the experiment more carefully. They have tried to measure the two decays under the same conditions, as nearly as possible. The result for the ratio is in agreement with the theory. I believe the experimental

error is about 20%; the theoretical uncertainty is something like 15%.

In order to test the hypothesis, we must try to say what would happen if it failed. Suppose the pion were not coupled to β-decay through the vector interaction. Now the quantity that enters into the γ-decay matrix element is essentially the difference $\mu_p - \mu_n$ of the proton and neutron magnetic moments which is nearly 5 nuclear magnetons. If the pion contribution were removed, we would get the quantity proportional to the β-decay matrix element with *its* pion contribution removed.

We don't know exactly what $\mu_p - \mu_n$ would be without the pion contribution, but we might guess that it would be of the order of 1 nuclear magneton. Thus we might expect the constant "a" to be reduced by a factor of 5 if the β-decay current failed somehow to include the pion.

R.P. Feynman. — The comparison of the μ-e decay rate to the β-decay rate (determined from $O^{14} \rightarrow N^{14}$ decay) still is in some difficulty. The result disagrees with theory by $4 \pm 2\%$; of which 1% is experimental and 1% theoretical uncertainty. It assumes that the O^{14} state is a pure I-spin = 0 state like N^{14}. If the overlap integral were less than 1 by 2 or 3% we would explain the discrepancy. Estimates of the impurity produced by the Coulomb potential show it must be certainly less than 1/2%. Blin-Stoyle suggested that the small differences of nuclear force between *p-p*, *p-n* and *n-n* may make a larger impurity. I do not know if this is substantiated by calculation.

E.P. Wigner. — On light nuclei, and as far as the normal states are concerned, the isotopic spin is a very good quantum number. This was pointed out by Radicati and more precisely by McDonald. In particular, the increase of the radius as a result of the decreased binding energy of the proton-rich component of the isotopic spin multiplet is perhaps a less serious effect than one might be inclined to believe.

As a matter of fact, if one plots the average potential for proton and neutron in the inside and near the surface of the nucleus, they are quite alike. The decrease of the binding energy of the proton is the result of the decrease of the potential because of the $1/r$ decrease of the Coulomb field outside the nucleus. As a result, it takes less energy to move the proton to a point far from the nucleus, than

it takes to move the neutron to the same point. However, the nuclear wave function does not extend too much into the region in which the drop in the Coulomb field has become appreciable.

It is in conformity with this that in the case in which we have good reason to believe that the β-decay matrix element is constant, it remains constant within 1%. This has been experimentally demonstrated for the β-decay via the Fermi matrix element from the proton-rich member of an isotopic spin multiplet to the next member of this multiplet.

On the other hand, I would be worried to make statements about the isotopic spin impurity of highly excited states, such as the $T = 1$ state of B^{12}. Many $T = 0$ states may be close to this state and impart it considerable impurity.

Riazuddin, I believe, has investigated the difference between the nuclear part of the interactions of proton-neutron and proton-proton. It seems that this difference is smaller than the Coulomb interaction.

R.E. Peierls. — We seem to base the hypothesis of the conserved weak vector current on the equality of two rates, and at the same time to worry about a difference between these two rates. There is a precedent for such a situation in physics, in the history of charge independence of nuclear forces, and in that case the attitude justified itself. But we should still keep an open mind whether we are really justified in regarding the 20% difference in the axial-vector case as a substantial effect reflecting a renormalization due to the strong interactions, and the 1% or so in the vector coupling term as a correction to be explained.

R.P. Feynman. — There was an independent argument, forever it is worth, which lead me to believe in the vector coupling. This was at a time when there was (because I had only older β-decay data) a 9% deviation one which therefore worried me very much. It went like this.

One wanted to use two component wave equations for the electron and proton. It is easy for a particle in a field with a vector coupling like electrodynamics but not for the pion-nucleon scattering via γ_5 for $\gamma_5 \not{\nabla}$. So to sidestep this I took a model that nucleons have a strong vector coupling and pions were not elementary, but Fermi-Yang bound states of nucleon and anti-nucleon. On this model,

it is easy to deduce that there is no renormalization of the vector current and that the pion amplitude for leptic decay should be exactly $\sqrt{2}$.

Why should we believe this result from such a special model ? Because I believed that nature is so constituted that you cannot tell if pion is elementary or not. If the rate were not $\sqrt{2}$ you would be able to conclude it was not a system derived from nucleon-anti-nucleon. So to avoid that such a decision could ever be made I supposed that it was $\sqrt{2}$, and it is easy to see that the general rule must be that the coupling is via the isotopic spin current.

Admittedly that is not a very strong argument but at the time I believed it and was very concerned about this 9% error. I was very happy next day to discover that my data was bad and the agreement was more like to 2% and within the experimental error (before electromagnetic corrections were worked out).

H.A. Bethe. — Schumacher at Cornell has recently considered a point which may bring the constants of β-decay and μ-decay into closer agreement. The radiative corrections of μ-decay are perfectly definite, as Feynman pointed out in the discussion to-day. Those of β-decay need to be cut off at high energy. In β-decay $N \rightarrow N + e + \upsilon$. Usually the Stanford " size " of the nucleon is used for the cut-off. However, the e-υ system may have a much smaller " size ". According to Schumaker the existence of two different cut-offs may reduce the 4% discrepancy between β- and μ-decay to about 2%. For this purpose the size of the e-υ system is taken to be about equal to the wave length λ_{un} at which the " weak " interaction between e and υ would violate unitarity.

R.P. Feynman. — I believe that the μ-e decay rate correction is not divergent and can be calculated without uncertainty. The neutron decay rate is divergent and depends on a logarithm like *ln*(cut-off/electron energy) so it is not very sensitive to the value of the cut-off if that is near the proton mass. I doubt the calculation can be off by more than about one percent.

E.P. Wigner. — Could we hear a few words about the doubts raised lately in the CP invariance, particularly by Sachs and Treiman ?

A. Pais. — These doubts were brought up as a consequence of experimental results obtained in the $K^0 \to \pi e \nu$ type decays. These results are preliminary and as yet unpublished and it seems only right to me to wait for definite word on the results before we come to further conclusions.

M. Gell-Mann. — In connection with the Fry-Camerini experiment the result of which seems to challenge the $\frac{\Delta S}{\Delta Q}$ rule and CP invariance, some of the crucial decay events have turned out to be doubtful. We should wait for the analysis at Berkeley of 25,000 associated productions of K^0 and Λ^0, 10,000 of which have already been measured. The Berkeley bubble chamber experiment is a much cleaner one and should give statistics at least as good.

A. Salam. — The Fry experiment gives $\frac{\Delta S}{\Delta Q} = \pm 1$. To get a statement from it about CP invariance, Sachs and Treiman have to make additional assumptions about the behavior of the amplitude. Thus the statement that Fry experiment shows CP violation is true only subject to a number of additional assumptions which one can easily relax.

SYMMETRY PROPERTIES OF FIELDS

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1. FIELD THEORY

So far all our serious attempts to describe elementary particle phenomena in mathematical terms really constitute a single body of theory, all of which I shall describe as " field theory ". We assume relativistic invariance, microcausality, positive energies, and positive probabilities totaling one. Then we try to construct a consistent theory that will explain as much as possible of the available information.

Now there are various approaches to field theory, each of which has advantages in certain situations. There has been some controversy among proponents of the various methods, but in my view they all have something to teach us and should be considered together, so that ideas originating from one approach can be applied to the others.

We may distinguish three main divisions :

(1) Lagrangian field theory. Here the Lagrangian density L is specified as a function of certain " bare " fields ψ and their gradients. We have one ψ for each " elementary " particle. The parameters in the Lagrangian are " bare " masses and coupling constants which do not appear explicitly in the final results. The equations of motion (together with the commutation relations) are solved by perturbation methods or some modification thereof. The S-matrix is calculated and expressed in terms of physical masses and coupling constants; the theory is usually considered acceptable only if at this stage there is no dependence on a cut-off (i.e., the theory is " renormalizable "). The method of Feynman diagrams expresses the results very neatly.

Quantum electrodynamics was developed in this way and has been successful in describing the properties of electrons, muons, and photons to the high order of accuracy now attainable by experiment.

(2) A second approach is that of LSZ [1], Wightman [2], and others. For any particle to be discussed, elementary or not, free "fields" ψ_{in} and ψ_{out} are constructed [3]. For at least some of these, there are supposed to be local interpolating *renormalized* fields which reduce to ψ_{in} and ψ_{out} at $t = \mp \infty$ respectively. The vacuum expectation values of products of the interpolating fields are discussed, with the conditions of relativistic invariance, microcausality, and conservation of probability imposed. The S-matrix, which relates the ψ_{out} fields to the ψ_{in} fields, is then calculated in terms of certain boundary conditions, the specification of which replaces the choice of elementary particles and of Lagrangian in the first approach. In (2) as in (1), we calculate scattering amplitudes not only for physical particles on the mass-shell, but also for virtual particles off the shell.

(3) The third approach is that of dispersion theory [4]. As in (2), we impose directly the causality, relativity, and unitarity conditions; however, we work only with amplitudes on the mass-shell. Of course, we must pay the price of considering the never-never land in which $|\cos\theta| > 1$, certain momenta are imaginary, etc. The choice of theory corresponds in this case to the specification of the number and character of subtractions in the dispersion relations and the locations and strengths of "CDD poles" [5]. The concept of "field" is not made use of explicitly. Instead, one works directly with the analytic properties of the various amplitudes.

Now it is conceivable that the third approach is more general than the second, and the second more general than the first. If that is so and if a correct theory is found to be describable, say, by the third method and not by the other two, then of course it will have been worthwhile making a great fuss over the distinctions among the methods. For example, considering the strong interactions only, it is now being suggested [6] that perhaps there are no "elementary" particles, all baryons and mesons being bound states of one another. In practical terms, such a suggestion would mean that all the masses and coupling strengths of these particles could

be calculated, given one mass, that there would be no CDD poles, and that the S-matrix would have certain characteristic properties at high energies and momentum transfers. Such a situation might be hard to describe by the first method.

In none of the kinds of field theory do we have any assurance that solutions of the equations actually exist or describe nature accurately if they do. Thus some physicists believe that to find a theory of the particles we must violate the postulates of field theory, for example by introducing a cut-off at high energies. I shall not discuss this question further, but clearly it is important.

In my remarks on symmetry, I shall try to stick to statements that can be rendered into any of the three languages we have discussed. However, I shall not shrink in some cases from using terminology peculiar to the first approach, even though it is less fashionable now than the other two.

II. EXACT SYMMETRIES

Any field theory (in our sense of the term) has the following symmetry properties :

(*a*) Invariance under Lorentz rotations, corresponding to conservation of total angular momentum.

(*b*) Invariance under Lorentz translations, corresponding to conservation of total energy and momentum.

(*c*) CPT invariance, the basic symmetry between matter and antimatter that permits us to regard antiparticles as particles moving backwards in space-time [7]. Closely connected with CPT invariance is crossing symmetry [8], which plays a crucial role in the dispersion formulation of field theory.

(*d*) The connection between spin and statistics. With no method does it appear possible to describe a particle with half-integral spin consistently as a boson or a particle of integral spin as a fermion.

Evidently there is a selection rule prohibiting the transformation of a system containing an odd number of fermions into one containing an even number and vice versa [9]. That is our first example of what WWW [10] call a " superselection rule ". The conservation

laws of energy, momentum, and angular momentum are not superselection rules. The point is essentially the following : we are familiar with states that are linear combinations of various eigenstates of, say, total momentum, with definite phases (for example, localized states). If one such state can be prepared, others can too, using a measuring apparatus in a localized state. WWW assert, however, that no state has ever been prepared that is a linear combination of states with even and odd numbers of fermions, with definite phase relations between the states. Then, because of the absolute selection rule and the absence of any measuring apparatus in such a state, no such state will ever be prepared. In the next section, we mention conservation of charge, of baryon number, and of lepton number, all of which are presumably superselection rules, in the absence of evidence for any states which mix the various eigenstates of those operators with definite phases.

Invariance under Lorentz translations, giving conservation of energy and momentum, is an example of gauge invariance of the first kind. In the language of Lagrangian field theory, we perform an infinitesimal translation by the four-vector Δ_α on the argument x_α of each of the local fields ψ in the Lagrangian; for a gauge transformation of the first kind, Δ_α is independent of x_α. Under such a transformation, the action $\int \mathrm{L}\, d^4x$ remains invariant; hence the conservation of total energy and momentum. Now we may also consider an infinitesimal gauge transformation of the second kind, in which Δ_α is a function of x_α. When performing a general infinitesimal coordinate transformation, we must be careful not only to alter the arguments of the fields ψ, but also to perform the concomitant reshuffling of their spinor or vector indices, if there are any. Along with the translation, an additional operation is performed on the symmetric tensor field $h_{\alpha\beta}$ of the graviton, namely the addition of a term proportional to $\partial_\alpha\Delta_\beta + \partial_\beta\Delta_\alpha$. Under the complete gauge transformation of the second kind, the action is again invariant. This stronger invariance is connected with the vanishing rest mass of the graviton; furthermore, it gives the conservation of the local quantity $\theta_{\alpha\beta}$ called the stress-energy-momentum tensor, which is the source of the field $h_{\alpha\beta}$. We are dealing, in fact, with precisely the invariance called " general relativity " by Einstein; the gauge-invariant non-linear quantum theory of the graviton reduces to Einstein's theory of gravitation in the classical limit.

Although we discuss gravitation here in flat space, following Gupta [11], Feynman [12], and others, it can be shown that curved space may be introduced merely by re-interpretation of quantities[13].

Our discussion of exact geometrical symmetries may be concluded by mentioning two symmetries that are not built in to the structure of relativistic field theory like the ones we have discussed so far, but which may well be exact symmetries nevertheless. In saying they are not built in, we mean that it is perfectly possible to write down theories apparently consistent with special and general relativity and the other principles of field theory, but which violate the symmetry rules we are referring to. Thus the rules stand, at present, purely as empirical laws, though each of them is aesthetically appealing and has a geometrical interpretation.

The first is invariance under CP or T separately. The operation CP, which interchanges left and right, particle and antiparticle, is unitary and its conservation gives selection rules, of which the most striking examples occur in the decay of the mesons K_1^o and K_2^o. Time reversal T, however, is anti-unitary and invariance under T gives conditions on the phases of matrix elements instead of giving selection rules. In particular, by a proper choice of phases for the initial and final free-particle states, the S-matrix can be made symmetrical as a result of T invariance.

The second rule pertains to a single particle, the neutrino. Considering all present information on the neutrino and its interactions (which we suppose are just the weak and gravitational couplings), we may say that the laws of physics are invariant under the transformation $\nu \to \gamma_5 \nu$, so that the rest mass of the neutrino is zero and its helicity conserved. In fact, so far as we know, the neutrino exists only in a left-handed condition and the antineutrino only in a right-handed condition. Of course, it may be that the right-handed neutrino exists as well, uncoupled to the weak interactions, and can be made only by gravitational pair production; it is not expected that experimental evidence on this question will be available in the near future.

The conservation of electric charge is the most familiar example of an apparently non-geometrical symmetry. The electric charge operator Q has eigenvalues that are integral multiples of a basic unit. The conservation of Q is expressed in the language of

Lagrangian field theory by a gauge invariance of the first kind. As in the case of the energy-momentum four-vector P_α, there is also a stronger gauge invariance of the second kind; here the gauge is taken up by the massless field A_α of the photon instead of the field $h_{\alpha\beta}$ of the graviton. Analogous to $\theta_{\alpha\beta}$ is the local electric current j_α, which is the source of the field A_α and which is divergenceless. For the vector field A_α, the gauge-invariant theory is not infinitely non-linear like that of the tensor field $h_{\alpha\beta}$.

The remaining exact laws of symmetry are the conservation of baryon number n_b and lepton number n_l. These quantities also have integral eigenvalues and in each case the conservation law corresponds to gauge invariance of the first kind. A gauge transformation of the second kind can be used to define a local current for the baryon number or lepton number, but in neither case do we know of a field (like A_α) that takes up the gauge to give *invariance* under the transformation of the second kind. Presumably such a vector field would be massless and would produce a long-range universal force between particles or macroscopic objects, proportional to the product of their baryon numbers (or lepton numbers, as the case may be). If such forces exist, they must be very weak [14]; anyway, they have not been detected.

It is now being suggested [15], in the case of baryons, that perhaps if the vector field is very strongly coupled, a gauge-invariant theory might lead to a non-zero mass for the vector particle. Whether or not this is so, there could be a massive elementary vector meson coupled to the baryon current (provided the word " elementary " really has meaning). If such a meson exists, it could be the " gluon " that binds baryons and antibaryons together to make all the other mesons.

III. APPROXIMATE SYMMETRIES

We have listed all the known exact symmetries of the microscopic laws of physics. There are many mysteries associated with them, but even more mysterious are the approximate conservation laws.

The most prominent of the approximate symmetries are those that hold for the strongly interacting particles if the weak interactions (or else both the weak and electromagnetic interactions)

are " turned off ". There is the conservation of the x and y components of the isotopic spin **I**, broken by electromagnetism and the weak interactions; the conservation of I_z or of strangeness, broken by the weak interactions; and the conservation of C or P separately, also broken by the weak interactions.

These rules are all easy to notice because they are violated only by small perturbations. If we are willing, however, to consider rules that are valid if the strong interactions or the baryon masses are " turned off " also, then we may find a host of other approximate rules, such as conservation of helicity, conservation of axial vector or of strangeness-changing currents, etc.

For the leptons, certainly, there seem to be conservation laws that hold if we abolish all the lepton rest masses. If we go further and turn off the electromagnetic and weak interactions as well, then the resulting system (while rather empty physically) is symmetric under a huge continuous group of transformations. These symmetries are obviously less useful than isotopic spin or strangeness or parity conservation, but are they really very different in principle ? What, in fact, is the meaning of a quantity that is approximately conserved ?

We can best investigate the matter in the language of Lagrangian field theory, later translating our results into dispersion theory if we want to. Consider, for example, the isotopic spin **I** in the presence of the symmetry-breaking interactions. Although **I** is not constant in time, the commutation rules of the components of **I** at any given time are exactly those of an angular momentum. Moreover, in the Lagrangian approach, the field ψ have definite transformation properties under equal-time commutation with **I**. For example, for the nucleon field N, we have exactly

$$[\mathrm{N}, \mathbf{I}] = \tau\, \mathrm{N}/2 .$$

Now one may argue that the bare nucleon field is not a physically interesting quantity; however, in the Lagrangian picture the physically interesting quantities (like $\theta_{\alpha\beta}$, the operator that interacts with a weak external gravitational field, or j_α, the operator that interacts with a weak external electromagnetic field) are expressed simply in terms of the bare fields. Thus the transformation properties of measurable operators like $\theta_{\alpha\beta}$ and j_α under equal-time commutation

with **I** are specified (and come out very simple) in a given Lagrangian field theory. In particular, the transformation properties of the Hamiltonian density $H = \theta_{44}$ are specified, and it is H that determines the time dependence of all operators. The weak current J_α that interacts with lepton pairs is certainly a physically interesting quantity, too, although not precisely measurable in the technical sense.

Now another property of the isotopic spin emerges. We construct its current $\mathcal{J}_\alpha$ (by the gauge method, in the Lagrangian field theory) and we remark that the isovector part of the electric current j_α is just proportional to $\mathcal{J}_{z\alpha}$. Furthermore, according to the conserved vector current theory [16], the strangeness-conserving vector part of $\mathcal{J}_\alpha$ is just proportional to $\mathcal{J}_{x\alpha} + \mathcal{J}_{y\alpha}$. Thus the isotopic spin and its current have physical significance in themselves. The various equal-time commutation relations among **I**, $\theta_{\alpha\beta}$, j_α, and so forth, are thus relations among physical quantities and they can be taken over directly into dispersion theory.

Approximate conservation of **I** means that we can treat the part of H that fails to commute with **I** as small. But even if the violation of charge independence were very serious, the equal-time commutation rules we have discussed would still be valid and isotopic spin would still be a useful symmetry operator.

We are thus led to ask about the other parts of the weak current J_α for baryons and mesons : are they also the currents of symmetry operators with interesting intrinsic properties at a given time, but varying rapidly with time because their conservation is violated by *large* terms in the Hamiltonian ? This question is discussed at length elsewhere [17] and answered in the affirmative. I shall restrict myself here to a brief summary of the conclusions.

Let us construct the charge operator (space integral of the fourth component of the current) for each piece of j_α and of J_α. Commuting these charge operators at equal times, we construct other operators, and continue until the algebraic system is closed. The mathematical character of the algebra which the charge operators generate is a definite property of nature. So are the representations of the algebra obtained by commuting these charge operators with $\theta_{\alpha\beta}$ at equal times. Although the non-conserved charge operators vary

with time, the commutation algebra and the representations generated from $\theta_{\alpha\beta}$ remain the same at all times.

Now what is the algebra ? It could, of course, be very complicated and it could be of infinite dimension. I would like to suggest, however, that it may be the simplest possible one that has enough structure to include the known currents. We consider the currents in the Sakata-Okun model, according to which all baryons and mesons are constructed from three basic spinor fields transforming like proton, neutron, and Λ fields and denoted by p, n, and Λ. There may also be a neutral vector " gluon " field B_α.

All the known properties of the strongly interacting particles are consistent with such a simple model and with the assignments

$$j_\alpha = ie\bar{p}\gamma_\alpha p$$

$$J_\alpha = i\bar{p}\gamma_\alpha(1+\gamma_5)n + i\bar{p}\gamma_\alpha(1+\gamma_5)\Lambda$$

for the electromagnetic and weak currents.

The algebra generated by the charge operators in this theory is just the algebra of the infinitesimal generators of the group $U(3) \times SU(3)$. (We would have $U(3) \times U(3)$ if we included the baryon helicity current.) The eigenvalues of the hermitian charge operators (suitably selected) are simply proportional to the integers.

In the model, stronger algebraic conditions can be obtained as well, for example the equal-time commutation relations of the fourth components of the currents (i.e., the densities of the charges). Also, the divergences of each current is proportional to the commutator of the charge with the Hamiltonian density. The part of the Hamiltonian density that is not invariant under the algebra of the charges generates a non-trivial representation of the algebra. A particular form of the Sakata-Okun model tells us which representation appears. Naturally, the conserved charges are those that leave the whole Hamiltonian invariant.

If there are just three leptons (ν, e, and μ^-) and their antiparticles, then the algebraic structure of the currents in the lepton system can be essentially the same, except for the fact that here two basic fields are charged instead of one [18].

Having extracted from a model as many relations as possible among physical quantities, we may drop the Lagrangian picture and utilize the relations in dispersion theory. Imagine that the S-matrix of the strong interactions has been calculated by the dispersion method. The matrix elements of the weak and electromagnetic currents, treated in first order, then obey known linear homogeneous dispersion relations. But in order to fix the scale of the matrix elements (for instance, to calculate the renormalization factor for the axial vector β-decay coupling) we need the nonlinear conditions supplied by the commutation rules of the algebra.

We have seen that there is, underlying the structure of the elementary particles, a large continuous group of symmetry operations, which appear in the Lagrangian field theory as reshufflings of the fields ψ. These transformations are not closely connected with the geometry of space-time, but there are two apparently geometric symmetries that should probably be considered in the same category. One is the neutrino transformation $\nu \to \gamma_5 \nu$; the other is charge conjugation C. These symmetries are either finite elements of a very large continuous group or else discrete reflections that must be adjoined to it. Now if the basic symmetry of nature between left and right is expressed by the exact conservation of CP, then parity conservation is equivalent to invariance under C. The experiments performed in 1956-1957 at the suggestion of Yang and Lee [19] have shown us that C is an approximate symmetry, and it fits, along with the other approximate symmetries, into the group of reshuffling operations.

It is an important task to make sure what the group is and how $\theta_{\alpha\beta}$ transforms under it. Still more challenging is the problem of understanding the curious combination of symmetry and asymmetry that characterizes the laws of motion of the particles. If we think of the group as a rotation group in a certain space, then the system of elementary particles seems to have bumps and bulges in that space. Why ?

In this connection, Heisenberg, Nambu, and others are trying to see whether these asymmetrical laws can somehow be obtained as a special case of the solution of a basically symmetrical problem. Thus a single ferromagnetic domain, although it points in a particular direction, exists as a result of laws that are symmetrical under

rotations. Presumably, the system of elementary particles is a member of a set of conceivable systems such that the *set* is completely invariant. But can we show in some dynamical way that this set of bulging systems constitutes a unique self-consistent situation ? And besides, what is special about the particular group that nature has chosen ?

The next few years may answer these questions or else, more likely, teach us that they were the wrong questions.

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Discussion du rapport de Gell-Mann

A. Pais. — You have stated that you wish to determine the group in such a manner that it applies both to baryons and to leptons. I agree that this is desirable. My question is, if there exists two distinct neutrinos, do you think that then SU(3) is still an acceptable choice ?

M. Gell-Mann. — The problem certainly is different if there are two neutrinos.

A. Pais. — This morning the mysterious absence of the neutral lepton currents was mentioned.

In my opinion this effective absence should in some way be a consequence of the algebra of interactions. Do you have any comments ?

M. Gell-Mann. — The notion of neutral currents for baryons and not for leptons is a mysterious one.

It is conceivable to get along without neutral currents. It may well be that $|\Delta I| = 1/2$ dominates 3/2 for non-leptonic decays for dynamical reasons. As Nishijima has suggested, a dispersion relation for the matrix element (without subtractions) may be dominated by the K particle and a scalar analog of it in the case of $I = 1/2$, while no comparable effect exists for $I = 3/2$.

G. Chew. — The development of physics has been characterized thus far by the feature that no theory has ever been exact.

Perhaps we are destined indefinitely to this fate; theory will gradually become more and more comprehensive and accurate — perhaps more and more beautiful — but a final stage may never be reached. If one accepts this view, it is perhaps important at any given time not to try to make too large a step before the situation is ripe. Think of the development of atomic theory; might that

not have been hindered if one had insisted from the beginning on generating a comprehensive framework that included nuclear structure ?

Perhaps we have an analogous situation with regard to strong and weak interactions. What do you think of the argument that it is only a fluke we know anything about weak interactions at the present time — that historically an understanding of them may have to wait until a theory of strong interactions alone has been constructed ?

M. Gell-Mann. — Assuming we want to work on a part of the theory, we are faced with a choice. The strong interactions present some advantages, but so do the weak and electromagnetic ones. Because these couplings are not strong, we know how the photon and the leptons behave and we can use them as tools to measure the currents in the baryon-meson system. Furthermore, the weak interactions seem to have a number of symmetry properties; in fact, they may have a deep connection with symmetry. Thus one might argue that everyone should concentrate on studying the weak interactions and not the very complicated strong ones. But I do not say that at all; instead, I would say that it is a good thing to have people working on many different approaches to particles physics.

S. Mandelstam. — With regard to the gauge-invariance of particles with mass, one can always add a field to a given field and then restrict the physically significant variables by a gauge principle. In particular this has been done for a massive vector field by Stückelberg. This does not really tell us anything, since one can eliminate the added field. In the case of a vector field, however, the elimination would lead to infinities. This has been shown rigorously by Johnson in the sense that, if the Stückelberg theory is finite, as one would expect from perturbation theory or by analogy with electrodynamics, the theory without the extra field shows some very unphysical features and is probably not consistent. This reinforces the arguments based on perturbation theory or dispersion relations. One may therefore make a case for a kind of gauge invariance with massive particles.

If a vector field interacts with a non-conserved current, nothing has been proved rigorously. In that case one cannot construct a gauge-invariant theory so that, if one assumes that quantities which

are infinite when the current is conserved do not suddenly become finite when conservation is destroyed, one arrives at the conclusion that a massive vector particle, like a massless vector particle, must interact with a conserved current.

In this connection one may suggest an attractive model for the elementary particles which is probably wrong, where the only elementary particles are the proton, neutron, lambda and ω_0. If we take the masses of the proton and neutron to be approximately equal for some unknown reason, and if we assume that the ω_0 must interact with an exactly conserved current, even in the presence of weak interactions, its coupling constants to the proton and neutron will be equal and isotopic-spin invariance follows without further assumption.

L. Van Hove. — A very essential point in your approach is the fact that gravitational, weak and electromagnetic interactions are treated to first order so as to provide the matrix elements of the corresponding currents. Can higher order effects in these interactions be accommodated in your scheme ?

M. Gell-Mann. — For electromagnetism and gravity, we know how to put in the higher order effects, but not for the weak interactions.

L. Van Hove. — If you include the gravitational interaction to higher order, how can you determine the Hamiltonian density from experiment ?

M. Gell-Mann. — In principle, by the interaction with a weak internal classical gravitational field varying in space and time.

A. Pais. — Many of us have been looking for some tight structure which would enable us to say that just those particles observed fit into it and no others. Many of these attempts have been in the nature of finding an underlying group for particle physics. The trouble has always been that symmetries implied by such groups must be broken. Therefore one may wonder whether it is true that the " tight structure " is actually to be seen as the manifestation of a group structure. Now isotopic spin for example is indeed connected with a group. One can ask if there are structures which

are no groups but which contain groups. Such structures exist (quasi groups). An example, is provided by the octonion algebra and I have recently shown (*Phys. Rev. Letters*, Oct. 1) that there are remarkable coincidences between this algebra and the structure of interactions. It is interesting also from the point of view of gauge invariances to consider more general hypercomplex number systems which satisfy " composition ". That is if Ψ' is an element of the algebra, $N(\Psi')$ its norm, S another element with $N(S) = 1$ then $N(S\Psi') = N(\Psi')$ defines a composition algebra. This defining relation may be looked upon as a gauge transformation. Apart from the complex numbers only quaternions and octonions satisfy this composition law.

SOME ASPECTS OF THE FORMALISM OF FIELD THEORY

by **GUNNAR KÄLLÉN**

In classical physics we can say that we have e.g. an electromagnetic field $(\bar{E}, \bar{H})$ at a given space-time point x. By this we mean that if we put a test body with known values of its mass, charge and velocity at the point x we can compute its acceleration, i.e. the force which is acting on it, from elementary electrodynamics with the aid of the formula $\bar{F} = e(\bar{E} + \bar{v} \times \bar{H})$. If we want to check this by an experiment we can, in principle, measure the acceleration of the test body and compute the fields $\bar{E}$ and $\bar{H}$ from the result. In practice, a test body of this kind always occupies a small but finite volume in space. To measure its acceleration we have to observe it during a small but finite time interval. Therefore, the thing we really measure is not the electromagnetic field at a given point in space and time but rather its average over a small but finite space-time volume. In classical physics one always assumes that this averaging procedure is not very essential but that one is allowed to make the idealization that one can measure the fields at a given point. This averaging procedure is more essential in quantum mechanics. It is true that one still permits oneself to speak of " fields at a certain point ". However, the fields are no longer given numbers but rather " operators " with somewhat intricate mathematical properties. In particular, one usually assumes that these operators fulfil certain commutation relations where the right hand sides contain the Dirac delta function [1]. If one wants to interpret these commutation relations as limits on the simultaneous measurability of various components of the fields, these relations make no sense at all as they stand. However, if one considers space-time averages of the fields in the way indicated

above, the delta functions appear under the integral sign in the averaging procedure and the commutation relations give very reasonable statements about the uncertainty in the field values (2). To be quite consistent one should always keep in mind that a field in quantum mechanics really should be averaged in this way. There is a very powerful mathematical technique developed which can be used to handle this situation. I am thinking of the " theory of distributions " as developed by L. Schwartz and others (3). If we always pedantically insist on the use of the theory of distributions we shall be burdened with a somewhat heavy mathematical machinery which does not really seem to add very much to our understanding of the subject. It is true that the theory of distributions allows us to develope a theory of quantized fields in a way which is mathematically rigorous. If one is not mainly interested in rigour it is easier to use the somewhat sloppier way of expression which has always been used in physics.

Let us now suppose that we have a classical field theory which we want to quantize. We assume that our system is described by fields $A_\alpha(x)$. Ideally, our classical system should be described by a certain Lagrangian $\mathscr{L}(A_\alpha)$ and the dynamic behaviour is obtained from a variational principle $\delta \int \mathscr{L}(A_\alpha)dx = 0$. Such a formalism leads to definite equations of motion for the fields and also to a canonical formalism with explicit expressions for the Hamiltonian $H(A_\alpha)$. This formulation of classical field theory is most convenient for a transition to quantum mechanics and one is able to copy the formal rules of the canonical quantization which have always been used in non-relativistic quantum mechanics. Historically, this is the way the theory has been developed (1). The technique also works wonderfully in certain simple cases, in particular if the fields A_α have no interaction. In this way one gets a unified description of the concepts of particles and fields, a formalism which allows one to describe the annihilation and creation of particles and a simple explanation of why particles with integral spin should obey Bose statistics while particles with half integral spin obey Fermi-Dirac statistics. All these subjects are so well-known that it should not be necessary for me to enter into them here.

A physically much more interesting situation appears when we have interacting fields. In many cases it is still possible to write

down a formal canonical formalism. The most important example of this is electrodynamics where the classical Lagrangian is fairly well-known. However, it is not possible to find exact mathematical solutions to the complicated equations one writes down in this way, but one always has to take resource to various approximation methods to extract physical information from the equations. In one case, viz. electrodynamics, there is an expansion technique available which is nowadays able to predict the outcome of various experiments with an amazing accuracy (4). Also in this case there exists, however, a difficulty in principle which I should like to emphasize. Let us look at one of the basic equations of electrodynamics, viz. the Dirac equation

$$(\gamma \frac{\partial}{\partial x} + m)\, \psi(x) = i e \gamma\, \mathrm{A}(x)\, \psi(x)\,, \qquad (1)$$

and concentrate our attention on the right hand side. We there find the product of the Dirac field $\psi(x)$ and the electromagnetic field $\mathrm{A}(x)$. According to what has been said above this is really a product of two distributions and such a product does not always make sense. (For an illustration, one might think of the Dirac delta function which cannot be squared !) Off hand, one should therefore like to say that the Dirac equation as written down above in terms of quantized fields does not really make any sense at all and it is somewhat amazing that in spite of this one can get information from it which can be checked with experience. As is well-known this information is not obtained in a perfectly straightforward way but one encounters various infinite quantities when one tries to work out in practice any quantity to sufficiently high order. It is also well-known that these infinite quantities can be removed with the aid of a renormalization technique. For the particular equation written down above this renormalization amounts among other things to our adding a counter term $\delta m\, \psi(x)$ to the right hand side. The " electromagnetic self-mass " δm of the electron is an infinite quantity which is adjusted in such a way that the whole right hand side, or $\gamma\, \mathrm{A}(x)\, \psi(x) + \delta m\, \psi(x)$, makes more or less sense. That this is possible is not at all trivial but has to be verified by lengthy and very technical computations for every order in the expansion parameter. (We are here oversimplifying the situation to a considerable extent as there are many more renormal-

izations necessary to make the right hand side well defined.) The point we want to emphasize here is that even if the field $\psi(x)$ itself and the field $A(x)$ itself are reasonable physical fields, i.e. can be described as distributions in the mathematical language, the *product of two such fields in the same space time point is not necessarily a well defined concept* but has to be interpreted with the aid of various tricks. On the other hand, such a product of two fields automatically enters into any equation of motion based on a classical field theory. This is somewhat sad as the counter term itself, especially the self mass δm, is not necessarily an unobservable quantity. We can think of the difference in mass between the neutral and the charged π-meson. It is quite possible and even probable that this mass difference is mainly the electromagnetic self mass ([5]). The fact that this self mass turns out to be infinite in the theory is a serious drawback and is an indication that there is something basically incomplete in our formalism.

So far we have mainly spoken about electrodynamics. Another important subject where essentially the same mathematical formalism enters is the interaction between e.g. π-mesons and nucleons. Nowadays there exists quite an appreciable amount of experimental information about this interaction but the theoretical attempts to explain the data are still at a very primitive stage. We know experimentally that the π-meson is pseudoscalar and it is reasonable to try to describe π-mesons with the aid of a quantized pseudoscalar field. Already at this stage one does something which is not very well justified. The very concept of a field is based on electromagnetism (and to some extent also on gravitation) where one can measure the field strength with the aid of charged test bodies. However, no one so far has ever measured the pseudoscalar π-meson field with the aid of a test body with " mesic charge " and no one knows if such a test body exists even in principle. It is certainly a very bold extrapolation to use the same formalism that has been developed for the measurable electromagnetic field to describe mesons, but it is about the best thing we can do today. Perhaps, this is one point where some radical change has to be made in the future but it is not very fruitful to try to speculate about that today.

Even if we accept the idea that π-mesons should be described by a pseudoscalar field the very fact that we cannot measure this

field classically means that we have no classical Lagrangian which we can start quantizing. The best one can do is to try various simple "Ansätze" for the interacting Lagrangian, work out the consequences and see if possibly one of our attempts checks reasonably well with experiments. This has also been tried for very many years but so far with very little success. The first thing one tries here is to use the same expansion technique (including renormalization) which worked so well for electrodynamics, but the numbers one computes in this way seem to have very little to do with nature ([6]). Another thing here is that the first few terms in the expansion do not seem to form a rapidly decreasing sequence of numbers. Therefore, one does not know if the disagreement between theory and experiment is due to our not having guessed the correct interaction or if it is only the mathematical method we have to solve the equations which is at fault. In view of this one has tried various other approximation schemes but not with much greater success. We mention here in particular the Tamm-Dancoff method ([7]). Some ten years ago this method was very much in fashion and even today it has some advocates. The basic idea is rather similar to perturbation theory. One starts with the Hamiltonian for a non-interacting field as zero approximation and sets up a complete set of state vectors where each state is an eigenstate for the free particle Hamiltonian. One then formally writes the eigenstates of the Hamiltonian including the interaction as infinite sums over the eigenstates of the free particle Hamiltonian and assumes that the infinite sum can be approximated by a small number of terms. As we have already said most attempts at practical computations done in this way have been rather disappointing. Personally, I feel that the reason for this can be understood from the work by van Hove and Friedrich in 1952 ([8]). They showed for one particular model which can be solved exactly that this expansion of the physical states in terms of free particle states is indeed very singular. The model considered consisted of a scalar field in interaction with a given *c*-number source. It can be shown that if the source has a finite radius *a*, the number of states which is important in the expansion becomes very large as *a* goes to zero. In the limit it even turned out that the coefficient of each state goes to zero while the sum of the squares of the coefficients is still equal to one. Clearly, this is a very singular kind of expansion and one cannot hope to get a reasonable approximation of the exact states by using

only a small number of terms. Afterwards, similar investigations have been made by other authors (Haag, Wightman, Schweber, Greenberg) [9] and it is nowadays clear that this feature is not critically dependent on the model but appears in practically every realistic theory. For some reason or other, this phenomenon is known in the literature as the " Haag theorem ".

Because of the disappointing situation for perturbation theory and perhaps also because of some " mathematical curiosity " there have been a few attemps during the last ten years or so to try to discuss the structure of quantized field theory without any resource to perturbation theory or other approximation schemes. Ironically enough, these methods were first developed for electrodynamics [10] but have afterwards found applications in meson theory [11]. The general philosophy here is that one tries to extract as much information as possible from general symmetry properties and other physical assumptions about the theory but refrains from any attempt to solve explicit equations of motion. The motivation behind these efforts varies somewhat from author to author. Many people, among them myself, always have an uneasy feeling here that we are really only scratching the surface of things and are very far from anything which deserves the name of a physical theory. Other people seem to like to make a virtue out of a vice, and there has been much talk recently about " the axiomatic approach to field theory " [12]. In its most extreme form an axiomatic paper starts by three or four " axioms " which are supposed to give essentially the whole physical content of a theory. Then one tries to work out as many consequences of these assumptions as possible. It cannot be denied that it is possible to get quite amusing and useful results in this way but I think one must the whole time keep in mind that there are many questions which have a reasonable answer, say, in the perturbation theory approach to electrodynamics but which one simply cannot discuss in the most extreme versions of the axiomatic approach. However, even if the motivation varies, many people are doing the same things nowadays and I should like to try to give a short sketch of what is going on. I do not intend to use the most high-brow mathematical language possible but shall, on the other hand, also try to avoid some of the pitfalls which exist in a too sloppy presentation.

One of the basic assumptions or axioms in this connection is

the assertion that any theory existing in nature must be invariant under Lorentz transformations. Of course, this assumption is an extrapolation of our experience for macroscopic distances. Some of its most immediate consequences, viz. the existence of certain conservation laws for energy, momentum and angular momentum seem to be well fulfilled also in elementary particle physics. It is true that these conservation laws themselves are not characteristic for Lorentz transformations but really follow from invariance under space and time translations and three dimensional rotations. However, Lorentz transformations imply the well-known four vector character of energy and momentum and this also seems to be well-founded experimentally in elementary particle physics. Logically, it is quite possible that one in the end has to relax on Lorentz invariance and some recent attempts in this direction can be mentioned ([13]). I hope it is not too unfair to say that as of today we do not really have any compelling reason to abandon Lorentz invariance and we put down as our assumption I.

I. *LORENTZ INVARIANCE*

We have already mentioned that invariance under Lorentz transformations implies the existence of energy and momentum and also that these quantities form a four-vector. It is part of everyday experience that this four-vector is always time-like (or possibly light-like) for any physical system that exists in nature and also that the time component of this vector (the energy) is positive (except, of course, for the vacuum, where this vector is identically zero). There is no mathematical reason from Lorentz invariance alone why the energy should always be a positive quantity or why the corresponding four-vector should be time-like. Historically, this is e.g. illustrated by the development of the Dirac theory where the solution to the Dirac equation without second quantization showed states with positive and negative energy. One had to add as an extra postulate that the negative energy states should be interpreted with hole theory thereby making the total energy of any state positive. The most elegant mathematical way to handle this situation is, of course, to use second quantization and replace the commutators in the canonical formalism by anticommutators. Therefore, we put down as our second assumption that the energy momentum vector

of every physical state lies inside (or on) the positive light cone. For shortness, we describe this by saying that our theory should have a " reasonable mass spectrum ".

II. *REASONABLE MASS SPECTRUM*

These two assumptions alone would not permit us to go very far. To be able to draw any conclusion at all we have to make a third assumption the justification of which is not as evident as for the others. We have already mentioned that one way to handle a theory of quantized fields is to impose canonical commutation relations for space-like separations. The physical interpretation of this is quite similar to the well-known discussion of measurability in elementary quantum mechanics and says in a somewhat over-simplified way of speaking that the measurement of a field at one point does not influence the measurement of the field in another point if the distance between the two points is space-like. In mathematical language this means that the commutator between the two field quantities is zero for space-like separations. We denote this by " local commutativity " and write it down as our assumption III.

III. *LOCAL COMMUTATIVITY*

Let us admit once and for all that this assumption is again an extrapolation from our experience of macroscopic distances. Logically, we can have no objection if this " causality principle " is violated for distances of the order of magnitude 10^{-13} cm or smaller. However, it must be remarked that the assumption of Lorentz invariance itself is based on the idea that no signal can be transmitted with a velocity greater than the velocity of light and this is roughly what our assumption about local commutativity also says. Therefore, one gets a somewhat uneasy feeling if one tries to relax on local commutativity but wants to keep assumption I. about Lorentz invariance. Roughly speaking one should here like to have either both these assumptions or none of them. It is quite possible to make a mathematical formalism where Lorentz invariance is present in the usual sense but where local commut-

ativity is not fulfilled — at least not generally. Therefore, we cannot claim that III. really follows from I. by mathematical arguments but from the physical point of view one definitely has the feeling that there is some relation. As a curiosity we want to mention that one particular case of III. really follows from I. and II. For completeness, let me sketch a derivation of this. I apologize for having to enter into some mathematical technicalities at this point, but I shall try to make the mathematics as short as possible.

Consider a scalar field $A(x)$. (We make the field scalar to avoid some technical complications with transformation properties under Lorentz transformations. This assumption is not too essential for the argument.) Consider the vacuum expectation value of the product $A(x_1)\,A(x_2)$

$$F^{A}(x_1, x_2) = <0|A(x_1)\,A(x_2)\,|0> = \sum_{|z>} <0|A(x_1)|z><z|A(x_2)|0>. \tag{2}$$

The last step here implies that we insert a complete set of " intermediate states " in the product of the two field operators. Let us further assume that all these intermediate states have definite values of energy and momentum. Now, it is an immediate consequence of the invariance of the theory under displacements that matrix elements of the kind $<0|A(x)|z>$ have a simple exponential dependence on the coordinate x. The proof goes the following way. If P_μ are the four-generators of infinitesimal translations (the energy-momentum operators) the invariance of the theory is expressed by

$$[P_\mu, A(x)] = i\frac{\partial A(x)}{\partial x_\mu}. \tag{3}$$

If we take a matrix element of this relation between the vacuum $|0>$ and the state $|z>$ we are interested in, we find using the relations

$$P_\mu\,|0> = 0\,; \quad P_\mu\,|z> = p_\mu^{(z)}\,|z>\,, \tag{4}$$

($p_\mu^{(z)}$ is the total energy-momentum vector of the state $|z>$) the following result

$$<0|\,[P_\mu, A(x)]|z> = -<0|A(x)z>\,p_\mu^{(z)} = i\frac{\partial <0|\,A(x)|\,z>}{\partial x_\mu}. \tag{5}$$

The last two versions of this relation can be understood as a differ-

ential equation for the matrix element of the field $A(x)$. The solution of this differential equation is trivial and we find the desired result

$$<0|A(x)|z> = e^{i p^{(z)} x} <0|A|z> . \tag{6}$$

The last factor $<0|A|z>$ is a constant of integration which depends on the field $A(x)$ and the state $|z>$ but which is independent of the coordinate x. Using Eq. (6) in Eq. (2) we find

$$F^A(x_1, x_2) = F^A(x_1 - x_2) = \sum_{|z>} e^{i p^{(z)} (x_1 - x_2)} <0|A|z><z|A|0> . \tag{7}$$

We note that the function F^A does not depend on the two points x_1 and x_2 separately but only on their difference. It is now convenient to split the sum over all intermediate states in two parts and first sum over all states for which the energy-momentum vector $p^{(z)}$ has a given value. In this way we find

$$F^A = \frac{1}{V} \sum_p e^{i p (x_1 - x_2)} G^A(p) = \frac{1}{(2\pi)^3} \int dp\, e^{i p (x_1 - x_2)} G^A(p) , \tag{8}$$

$$G^A(p) = V \sum_{p^{(z)} = p} <0|A|z><z|A|0> . \tag{8a}$$

We now invoke Lorentz invariance and say that the function G^A must be invariant under Lorentz transformations if the field A is scalar. This is rather trivial from the point of view of physics and can be proved by mathematical arguments which we skip. From the mathematical point of view, G^A is the Fourier transform of the function F^A. We now have the result that every vector p which enters into the Fourier transform of F^A must be the energy-momentum vector of a physical state. According to our assumption II. this means that the Fourier transform of F^A vanishes unless p lies in or on the forward light cone. From this and Lorentz invariance we can write

$$G^A(p) = G(p^2)\, \theta(-p^2)\, \theta(p_o) , \tag{9}$$

$$\theta(a) = \begin{cases} 1 \text{ for } a > 0 \\ 0 \text{ for } a < 0 . \end{cases} \tag{9a}$$

We now make the following formal rearrangement

$$<0|A(x_1)\,A(x_2)|0> = \frac{1}{(2\pi)^3}\int dp\, e^{ip(x_1-x_2)}\, G(p^2)\,\theta(-p^2)\,\theta(p_o)$$

$$= i\int_o^\infty da\, G(-a)\,\Delta^{(+)}(x_1-x_2,a)\,, \tag{10}$$

$$\Delta^{(+)}(x,a) = \frac{-i}{(2\pi)^3}\int dp\, e^{ipx}\,\delta(p^2+a)\,\theta(p_o)\,. \tag{10a}$$

The function $\Delta^{(+)}$ in Eq. (10a) is one of the well-known singular functions extensively treated in the literature. It can be explicitly computed and expressed in terms of a Hankel function. Here, we shall use the following representation of it

$$\Delta^{(+)}(x,a) = \frac{1}{i\pi}\lim_{\varepsilon\to o}\int_o^\infty \frac{db\;\bar{\Delta}(b,a)}{b+\bar{x}^2-(x_o-i\varepsilon)^2}\,. \tag{10b}$$

The weight $\bar{\Delta}(b,a)$ is a certain Bessel function but its details are unimportant for our discussion.

As we are interested in local commutativity for the moment we use these formulae to compute the vacuum expectation value of the scalar field $A(x)$ with itself for general points x_1 and x_2

$$<0|[A(x_1),A(x_2)]|0> = <0|A(x_1)\,A(x_2)|0> - <0|A(x_2)\,A(x_1)|0>$$

$$= -i\int_o^\infty G(-a)\,da\,[\Delta^{(+)}(x_2-x_1,a)-\Delta^{(+)}(x_1-x_2,a)]$$

$$= -i\int_o^\infty da\, G(-a)\,\Delta(x_2-x_1,a)\,, \tag{11}$$

$$\Delta(x,a) = \frac{-i}{(2\pi)^3}\int dp\, e^{ipx}\,\delta(p^2+a)\,\frac{p_o}{|p_o|}\,. \tag{11a}$$

The function $\Delta(x,a)$ which has appeared here is another well-known singular function. We here only need that it vanishes for space-like values of the vector x. This follows rather trivially from symmetry arguments. (Suppose x to be space-like. We can then introduce a coordinate system where the time component x_o vanishes. In that coordinate system the integrand is anti-symmetric

in p_0 and the result of the integration over p_0 must be zero. As the function is invariant, this must be true for every coordinate system.)

We emphasize that the result (11) has been obtained using only our assumptions I. and II. but that we have made no use of local commutativity to obtain it. On the other hand, we see from the explicit form of the result that this vacuum expectation value of the commutator between the field $A(x)$ with itself is zero for space-like separations. This rather amusing fact shows that assumption III. is not quite independent of the other assumptions.

To avoid misunderstandings we must emphasize that III. is in no way a consequence of I. and II. The only thing we have been able to prove is that III. must be fulfilled for one particular matrix element (the vacuum expectation value) of the commutator we have investigated. This is much less than local commutativity but it shows that we are not able to modify III. quite arbitrarily if we want to have a theory which is Lorentz invariant and which has a reasonable mass spectrum. This result somewhat supports the intuitive feeling that Lorentz invariance and causality are not completely independent assumptions.

There is another moral which can be drawn from this computation. We have seen that the commutator between a scalar field with itself automatically vanishes for space-like separations. If we try to use canonical anticommutators instead of the commutators for the scalar field we would have the result that both the commutator and the anticommutator were zero for space-like separations. Clearly, this is a very heavy restriction on the field and intuitively it is reasonable to suppose that no such field exists at all. As a matter of fact this is also true mathematically but I do not want to enter into details here ([14]). The main conclusion to be drawn is that it is impossible to quantize a scalar field according to the exclusion principle and have particles with spin zero obey Fermi-Dirac statistics. This argument has the advantage over the original Pauli proof ([15]) that we make no assumption about the interaction involved while the original Pauli papers mainly deal with non-interacting fields.

Let us return to Eq. (10b). It expresses the function $\Delta^{(+)}$ as the boundary value of a function with a certain complex denominator. The fundamental variable in this denominator is the four dimen-

sional square of the vector x but with a small imaginary part for the time component. Eq. (10b) can be interpreted to mean that the function $\Delta^{(+)}$ can be extended to an analytic function depending on the " complex Lorentz square of the vector x ". This analytic function has a certain domain of regularity and we see from (10b) that this domain is the whole complex plane except the positive real axis. At the first moment this appears to be a highly mathematical construction. We should like to show that this extension of the function $\Delta^{(+)}$ out in the complex plane can be interpreted to be a substitute for the averaging in space and time over suitable test functions which we have mentioned many times before. Consider the following rather special test function

$$f(x) = \varphi(\bar{x}) \frac{1}{\pi} \frac{\alpha}{\alpha^2 + (x_o - t)^2}. \tag{12}$$

We here average in time around the point $x_o = t$ and over a time interval of the order of magnitude α. The function $\varphi(\bar{x})$ describes the averaging over three dimensional space. We are not going to be concerned about that for the moment. We now apply this particular test function to the expression given by Eq. (10) and find

$$\begin{aligned} &<0|\mathrm{A}(f_1)\,\mathrm{A}(f_2)|0> \\ &= \int\int dx_1\, dx_2\, \varphi_1(\bar{x}_1)\, \varphi_2(\bar{x}_2) \frac{1}{\pi^2} \frac{\alpha_1 \alpha_2}{[\alpha_1^2 + (x_{1_o} - t_1)^2]\,[\alpha_2^2 + (x_{2_o} - t_2)^2]} \\ &\times \frac{1}{\pi} \int_o^\infty da\, \mathrm{G}(-a) \lim_{\varepsilon \to o} \int_o^\infty \frac{db\ \bar{\Delta}(b, a)}{b + (\bar{x}_2 - \bar{x}_1)^2 - (x_{2_o} - x_{1_o} + i\varepsilon)^2}. \end{aligned} \tag{13}$$

The two time integrals can be performed in an elementary way and one finds after a few rearrangements that it is possible to write

$$\begin{aligned} <0|\mathrm{A}(f_1)\,\mathrm{A}(f_2)|0> &= \int\int d^3x_1\, d^3x_2 \varphi_1(\bar{x}_1)\, \varphi_2(\bar{x}_2) \\ &\times \int_o^\infty \frac{db\, g(b)}{b + (\bar{x}_2 - \bar{x}_1)^2 - [t_2 - t_1 + i(\alpha_1 + \alpha_2)]^2}, \end{aligned} \tag{14}$$

$$g(b) = \frac{1}{\pi} \int_o^\infty da\, \mathrm{G}(-a)\, \bar{\Delta}(b, a). \tag{14a}$$

This formula shows explicitly how the average over the particular test function given in Eq. (12) automatically leads to the appearance of the complex denominator one has in Eq. (10b) *before* the limit $\varepsilon \to 0$ is taken. Analytic functions are very convenient to use for computations as they are ordinary complex numbers. Their boundary values are distributions and much more complicated to handle. Further, the analyticity properties of these functions are very useful tools for many purposes as e.g. for a discussion of local commutativity.

As an illustration of this remark we can consider a slight generalization of the calculation above where we have two different fields $A(x)$ and $B(x)$. In this case we get two different vacuum expectation values corresponding to the two products $A(x_1)B(x_2)$ and $B(x_2)A(x_1)$. If local commutativity is not valid the two corresponding analytic functions defined with the aid of slight generalizations of Eq. (10) have nothing to do with each other. However, if local commutativity is invoked we find that the two corresponding analytic functions are equal on the negative real axis of the variable $-\bar{x}^2 + (x_0 - i\varepsilon)^2$. Consequently, these two functions are equal everywhere. We have already mentioned that local commutativity certainly should hold for macroscopic distances but that the assumption that it holds also for microscopic distances is an extrapolation for which there is no real logical foundation. From the result above we see that if we request local commutativity to hold *exactly* for space like distances larger than a certain length l, this means that we require the equality of our two analytic functions on the real axis to the left of a certain point on the negative real axis. This weaker assumption is enough to guarantee that the two analytic functions are equal everywhere and consequently that local commutativity holds also microscopically. Therefore, it is impossible to make a theory where one requires exact causality for macroscopic distances, has assumptions I and II fulfilled but tries to introduce a non-causal feature for small distances. Again, this shows that assumption III is not quite independent of I and II.

The computations sketched above can also serve as an illustration of the general techniques one uses in this approach to the theory of quantized fields. The idea to break down products of field operators into sums over intermediate states, to explore the x-dependence of matrix elements as illustrated by Eq. (5) and to rearrange

the summation over these intermediate states so as to obtain the Fourier transform of the vacuum expectation values is something which works not only for a product of two fields but for a general product of n fields. In this way one is able to isolate the consequences of our general assumptions in terms of singular functions similar to the $\Delta^{(+)}$, etc. in Eq. (9a) and to have the physics of the problem expressed by weight functions similar to the G above. However, when one investigates more than two fields in this way one finds that assumption III. about local commutativity implies restrictions also on the weights. This illustrates what has been mentioned above that the complete assumption III. is not equivalent to I. and II. but has to be added as an extra postulate. The mathematical technique convenient for the handling of this problem is borrowed from the theory of analytic functions of several complex variables. This is a highly specialized subject and not very familiar to physicists. I do not think we have the courage to enter into more details of this here today. Let me only mention that it is possible to get a proof of the famous TCP-theorem by exploiting the analyticity properties of the vacuum expectation values ([16]).

So far, we have spoken only of the fields themselves in our general approach but have not mentioned anything about particles. A typical experimental situation which our formalism is supposed to handle is a scattering experiment where a certain number of incoming particles collide and a new set of outgoing particles emerge at the end of the collision process. The basic feature here is that one observes the particles before and after the scattering but that one does not have to have a detailed description in terms of particles in between. With a slight idealization we can say that we are here only interested in particles at minus infinity and at plus infinity. We further know from our experience with perturbation theory that the most convenient formalism to describe particles and fields together is obtained from the theory of free fields. We therefore add as assumption IV. that our fields should approach free fields in the distant past and in the distant future ([17]). We call this

IV. *AN ASYMPTOTIC CONDITION*

The exact way in which this asymptotic limit should be introduced is a question of taste. The most fashionable way nowadays is due to Lehmann, Symanzik and Zimmermann ([11]). These authors build

up normalizable wave packets of free particle states and request the matrix elements between two such states of any field to approach the corresponding matrix element of a free field for $|x_0| \to \infty$. Before this technique had been developed one used instead the so called " adiabatic switching off of the interaction " for large absolute values of the time [17]. This means essentially that one removes the interaction between the fields by brute force in the distant past and in the distant future, thereby changing everything to free fields. The intuitive idea behind this is clearly that when the particles are very well separated before and after the scattering they have no interaction anyhow, so you can as well remove it without changing anything of the physics. The wave packet formalism is very elegant and sometimes yields results which had previously been obtained with the adiabatic technique but with considerably more work. This is particularly so for scalar fields. However, for fields with more complicated transformation properties, as e.g. a vector field, a naive application of the wave packet formalism yields results which are in sharp contradiction with the canonical commutators. As a vector field is a fundamental quantity in electrodynamics this means that at least for this theory we had better use the more old-fashioned method with the adiabatic switching technique. The only alternative is to make serious modifications of the canonical formalism [18] [19]. In many practical cases the two methods lead to the same or very similar results. Among the results we want especially to mention the so called " reduction formulae ". To illustrate how this technique works I will give one simple example. Consider an external electromagnetic field described by a potential $A_\mu^{ext}(x)$ and compute the vacuum expectation value of the current operator. This quantity is not zero because the vacuum is polarized by electron positron pairs and other particles (virtual or real). After some computations one finds to the first order in the external field the following formula [10] :

$$<0|j_\mu(x)|0> = -i \int_{-\infty}^{x} dx' <0|[j_\mu(x), j_\nu(x')]|0> A_\nu^{ext}(x') + \ldots \quad (15)$$

The dots at the end of Eq. (15) indicate higher order terms in the external field and also some technical complications due to renormalization which I do not want to describe here. The operators $j_\mu(x)$ and $j_\nu(x')$ are the current operators with no external field

present. The interesting feature of this formula is that it allows us to express the change of the vacuum expectation value of the current operator in terms of the vacuum expectation value of two currents with no external field. Therefore, some knowledge of the vacuum expectation value of a product of two operators allows us to make statements about the integral kernel appearing above. In a loose way of talking we can say that this integral kernel corresponds to a " dielectric constant of the vacuum ".

Using a technique of the same kind as in our discussion leading to Eq. (10) above we find that we can write the commutator between the two currents in Eq. (15) in the form

$$<0|[j_\mu(x), j_\nu(x')]|0>$$

$$= -i \int_0^\infty da\, \Pi(-a) \left[\Box \delta_{\mu\nu} - \frac{\partial^2}{\partial x_\mu \partial x_\nu}\right] \Delta(x' - x, a), \qquad (16)$$

$$\Pi(p^2) = -\frac{V}{3p^2} \sum_{p^{(z)} = p} <0|j_\mu|z><z|j_\mu|0>. \qquad (16a)$$

Actually, the expression that enters into Eq. (15) is not the commutator (16) itself but rather " the retarded commutator " obtained from (16) by a multiplication by the step function

$$\theta(x - x') = \frac{1}{2}\left[1 + \frac{x_0 - x'_0}{|x_0 - x'_0|}\right].$$

Apart from certain technical details this essentially means that the function $\Delta(x' - x, a)$ in Eq. (16) has to be replaced by its retarded counter part $\Delta_R(x - x', a) = \theta(x - x')\,\Delta(x' - x, a)$. Writing all this in momentum space we find that the Fourier transform of the dielectric constant of the vacuum contains as an essential factor the following expression

$$\int_0^\infty \frac{da\, \Pi(-a)}{a + \bar{p}^2 - (p_0 + i\varepsilon)} = \mathrm{P} \int_0^\infty \frac{da\, \Pi(-a)}{a + p^2} + i\pi\, \varepsilon(p)\, \Pi(p^2). \qquad (17)$$

This formula shows that the dielectric constant is a complex number and that its real part can be computed from its imaginary part with the aid of an integral transform. It corresponds roughly

to the relation between the real and imaginary parts of an analytic function. This integral relation is of exactly the same form as the dispersion relation which was written down by Kramers and Kronig in 1927 for a classical dielectric medium ([20]). This is one very simple example on how one is able to find relations which in principle can be checked experimentally without knowing very much about the detailed properties of electrodynamics. As we shall hear in other lectures during this conference one can do similar arguments also for other and perhaps more interesting physical systems and obtain " dispersion relations " for various scattering processes without really knowing anything about the interaction responsible for the scattering. Every relation one obtains in this way gives one measurable function expressed as an integral transform of another measurable function. If both functions are measured one can check whether or not the integral relation is fulfilled. As such a statement is independent of perturbation theory or, even, of any assumption about a classical Lagrangian, it is of rather general validity. In the very few cases where one has been able to make a check on these relations they seem to be fulfilled. However, it must be emphasized that these relations even in their most sophisticated forms are not equivalent to a detailed theory of interacting fields. They are at most consistency conditions.

In the whole of the previous discussion we have without comments accepted the conventional formalism of quantum mechanics. This means among many other things that the probability concept is carried by certain state vectors in a " Hilbert space " and that the metric of this space is positive definite. There has been some recent attempts to relax upon this latter condition and to work with a more general space of state vectors where one formally has negative probabilities. The main trouble here is, of course, to arrange everything in such a way that these negative probabilities are unobservable ([21]). I think it is fair to say that these attempts are still at a rather preliminary stage and I do not feel competent even to try to describe them here.

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Discussion du rapport de Källén

P.A.M. Dirac. — I would like to add some remarks to what I said yesterday about quantum field theory to make it more precise. Some of my more mathematically-minded colleagues have told me that all the representations of the inhomogeneous Lorentz group are known. This would imply that they know the representation needed for quantum field theory.

However, the mathematicians assume that two representations are equivalent if they are connected by a unitary transformation, which means counting a unitary transformation as trivial. To a physicist, a unitary transformation may be very far from trivial. A good deal of atomic physics consists in trying to find the S-matrix, which is just a unitary transformation and is certainly far from being a trivial one.

We must take the physical point of view that two representations connected by a unitary transformation should not necessarily be regarded as equivalent. The number of different representations is then much greater, and we have the problem of picking out the right representation from among this greater number. In this search we should use any mathematical methods that we can think of; for example, we need not restrict ourselves to working in terms of tensors of finite rank, but may bring in tensors of infinite rank, corresponding to all possible representations of the homogeneous Lorentz group.

E.P. Wigner. — May I say that the significance of unitary transformation is very greatly appreciated by us, the representations themselves consist of unitary transformations and we are surely interested in them. In fact, there is no real difficulty in specifying the most general representation of the inhomogeneous Lorentz group in an arbitrary coordinate system. The difficulty is, rather, that if we specify the form of the representation, we also specify the coordinate system and we specify it in such a way which does not tell us what the operators for the other physical quantities are. All

that is given are the operators for energy, momentum, angular momentum, etc.

There is one exception to these statements. If the representation is irreducible, or if we consider the part of the Hilbert space which is spanned by the axes which belong to the discrete spectrum of the restmass. The restmass is one of the two characteristics of the irreducible representations (the other being the intrinsic spin). In this case, or in the aforementioned part of Hilbert space in the general case, all the operators which appear significant to me can be obtained. The principal ones are the position operators which can be defined by their relations to the operators of the inhomogeneous Lorentz group. Thus the operator of the X coordinate is changed into this operator plus a constant if the operator is transformed by a displacement in the X direction by this constant.

This, however, is an exceptional case and in the really interesting case of a continuous restmass spectrum, that is for collision systems, only the various momentum operators can be obtained by any known consideration. In fact, if I am quite sincere, I cannot say that it is clear to me what the physical quantities are for which one wants the operators to be defined. These may be the various fields or they may not be. This is a very difficult question and it is possible that an essential physical idea will be needed before it will be answered definitively.

It may be worth while to point out that the equation derived by Källén

$$<o \mid \mathrm{A}(x)\,\mathrm{A}(x') \mid o> \;=\; <o \mid \mathrm{A}(x')\,\mathrm{A}(x) \mid o>$$

for space like $x - x'$ does not depend on "a reasonable mass spectrum" but follows already from simple Lorentz invariance. If $x - x'$ is space-like, it is always possible to find a Lorentz transformation which carries x into x' and x' into x. In fact, this can be done by a rotation in the coordinate system in which x and x' are simultaneous. Let us denote the Lorentz transformation in question by $\mathscr{L}$. Since the vacuum is invariant under all Lorentz transformations $\mathscr{L} o = o$ and $\mathscr{L}^{+} = \mathscr{L}^{-1}$

$$<o \mid \mathrm{A}(x)\,\mathrm{A}(x') \mid o> \;=\; <o \mid \mathscr{L}^{+}\,\mathrm{A}(x)\,\mathrm{A}(x')\,\mathscr{L} \mid o>$$

$$= <o \mid \mathscr{L}^{-1}\,\mathrm{A}(x)\,\mathscr{L}\mathscr{L}^{-1}\,\mathrm{A}(x')\,\mathscr{L} \mid o> = <o \mid \mathrm{A}(x')\,\mathrm{A}(x) \mid o>$$

This derivation does not give, of course, *all the results of Källén*, not in particular those which depend on the positive definite nature of quantities. On the other hand, one may generalize the argument to some degree. Thus if the Minkowski distance of x_o from x and x' is the same, one can find an $\mathscr{L}$ which leaves x_o unchanged but transforms x and x' into each other. One can then conclude in the same way as before that

$$<B(x_o)\, o \mid A(x)\, A(x') \mid B(x_o)\, o> = <B(x_o)\, o \mid A(x')\, A(x) \mid B(x_o)\, o>$$

where B is either the same field as A or is another field or even product of fields. This shows that if, in the theory discussed $A(x)$ and $A(x')$ (for space like $x - x'$) do not commute, their commutator must at least have many o matrix elements.

It may be of some interest also that the original form of the consideration, that leading to Källén's equation, can be used for fields which are not scalars but have a spin. One then obtains the result that the vacuum expectation values of $A_\mu(x)\, A_\mu(x')$ and $A_\mu(x')\, A_\mu(x)$ differ by a factor $(-)^{2\mu}$ which is 1 for vector, tensor, etc. components, — 1 for spinor, etc. components. Thus if one assumes that either the commutator or the anticommutator of two fields is a C-number, the connection between spin and "statistics" follows... This connection originally proved by Pauli, was obtained, of course, by Wightman and his students under less stringent assumptions as far as the nature of the commutator or anticommutator is concerned, but assuming a " reasonable mass spectrum ".

G. Källén. — I thank Professor Wigner very much for showing us this simple and elegant derivation. If I may defend the somewhat clumsier method I used myself to show the same result, I should like to say that I really wanted the explicit representation in terms of the analytic function (the $\Delta^{(+)}$ function) for the later argument. Therefore, I should have had to do all I did anyhow at a later stage and a simpler derivation for this particular result should have increased the over all length of the discussion.

W. Heisenberg. — In connection with the so-called " theorem of Haag " I would like to point out, that its content should be well known already from conventional quantum mechanics. If one compares e.g. the state of a ferromagnet where the total magnetic

moment has the direction of the Z-axis and another state with a slightly different direction of the total magnetic moment, these two states will always be completely orthogonal to each other, if we have to do with an infinite ferromagnet. If we excite an electron from one of these states, again the resulting state will be exactly orthogonal to the other. Therefore, in writing down matrix equations for such systems one must be careful not to write down relations between matrix elements in which both sides of the equation are trivially zero. Such an error might occur in perturbation theory or in the old Tamm-Dancoff method, where one starts with the " bare " vacuum; it ought however not to occur in the new Tamm-Dancoff method, where one starts from the real vacuum. Taking the theory of superconductivity (Bardeen-Bogoliubov) as an example, the new Tamm-Dancoff method gives correct results (the energy gap), while the theory of perturbation does not. This whole problem therefore has nothing to do with the real difficulties of quantum field theory, nor will it give rise to any criticism concerning the use of the new Tamm-Dancoff method in field-theory.

G. Källén. — I agree very much that the theorem discovered by Van Hove and Friedrichs and usually referred to as the " Haag theorem " is really of a very trivial nature and it does not mean that the eigenvalues of a Hamiltonian never exist or anything that fundamental. Your analogy with ferromagnetism is also very interesting. However, I do think that this theorem does show that the old fashioned Tamm-Dancoff method is essentially not better than perturbation theory and I do not believe that the new Tamm-Dancoff method is so much better. The fundamental difficulty is that a finite amount of probability (one) has to be divided between so many states that each state gets essentially zero probability. This problem remains also in the new Tamm-Dancoff method.

W. Heitler. — I find it very difficult to understand from a physical point of view that local commutativity should not always follow from Lorentz invariance. Once a signal can travel faster than light even in a microscopic domain Lorentz-invariance is violated. One more example is the non-local theory I mentioned at the end of my report. Here local commutativity of the Hamiltonian density was violated in a microscopic domain and the consequence was that Lorentz-invariance was violated in the results. Is there

a more physical way to understand that cases exist where local commutativity is not but Lorentz-invariance is fulfilled ?

G. Källén. — It is possible to make formal mathematical models where mathematical Lorentz-invariance holds but where local commutativity is violated. However I do not really understand what that means and know of no simple description of the physics involved.

A.S. Wightman. — I should like to explain the " some reason or another " why the Haag theorem is so-called. What Haag found was that the phenomena of the strange representations of the commutation relations, discovered by Friedrichs and Van Hove in special models, is a general feature of any translation invariant theory in which non trivial pair production occurs.

The significance which one attaches to Haag's theorem depends on one's attitude towards model such as Heitler's. On the one hand, one can regard this model as a short hand for the investigation of the numerical effect of cut-offs in the perturbation series of a relativistic theory. Then mathematical questions about the exact spectrum of the model are quite irrelevant. On the other hand, one can take the Hamiltonian of the model really seriously, and try to find out what spectrum it predicts and what properties its exact eigenfunctions have. In this case, it seems to me that Haag's theorem is distinctly non-trivial. It says that to make physical sense of the Hamiltonian one must insert not the familiar representation of the annihilation and creation operators but one of the strange representations.

L. Van Hove. — I would like to make a few remarks on the question of the expansion

$$| n \text{ phys.}> \; = \; \sum_{n'} C_{nn'} \, | n' \text{ math.}> \qquad (1)$$

mentionned in Källén's talk and in various of our discussions. The formal difficulties connected with this expansion originate from the fact that all $C_{n,\,n'}$ become zero in a realistic situation. This can be due to two completely different causes which should be sharply distinguished.

In the case of an interaction modifying the physical system over the whole of space (examples are field theories with pair creation and practically all many-particle systems) $C_{n,n'}$ is zero because of the infinite extension of space : this is seen by enclosing the system in a finite volume V, calculating $C_{n,n'}$ for V finite and noticing that for $V \to \infty$, $C_{n,n'}$ goes to zero, usually with an exponential dependence on V. This situation holds even in a field theory with cut off, we know how to handle it and it is not connected with the real difficulties of field theory (nevertheless Haag's theorem, if I understand it correctly, refers to this situation and is therefore, I think, of little direct relevance to the basic difficulties of field theory).

The second case where one knows that all $C_{n,n'} \to o$ is the case of point particles interacting with a quantized field, the interaction giving rise to ultra-violet divergences. In this case, the interaction acts in a limited region of space only. One introduces an ultra-violet cut-off K. All $C_{n,n'}$ go to zero as $K \to \infty$. This situation, which is absent in many particle problems, is connected with the divergence problem of field theory. It has been demonstrated on the simple example of a scalar field in scalar interaction with a static source (see K.O. Friedrichs, *Comm. Pure and Applied Math.*, **5**, 349 (1952) and L. Van Hove, *Physica*, **18**, 145 (1952)). Similar but less explicit conclusions have been obtained for more realistic cases in L. Van Hove, *Acad. R. de Belg.*, Bull. Cl. des Sc. 5e S, **39**, 1055 (1951).

I would like to mention another point in connection with the expansion (1). It is natural to try to avoid the difficulty $C_{n,n'} = 0$ by attempting to replace in the righthand side $| n', \text{math.}>$ by other states $| n'>$ which, although being simpler than $| n', \text{phys.}>$, would be better approximations to the latter and thereby give rise to a meaningful expansion with $C_{n,n'} \neq 0$. A choice of $| n'>$ which has been considered is to take states of several dressed particles neglecting their mutual interaction; in terms of diagrams the definition of such states is quite easy (see W. Frazer and L. Van Hove, *Physica*, **24**, 137 (1958)). Such states are then linearly independent of but non orthogonal to each other. In simple non relativistic models one has been able to show that they have the following interesting properties :

(1) the metric tensor elements $<n | n'>$ and the matrix elements

$<n \mid H \mid n'>$ of the Hamiltonian H have simple, finite expressions involving only the renormalized coupling constant.

(2) iterative solution of the Schrödinger equation in the $\mid n'>$ representation leads to convergent expressions (see Th.W. Ruygrok, *Physica*, **24**, 205 (1958). The difficulty however, is to carry out this program covariantly, although possible recent applications of dispersion techniques seem to embody the main idea of the method in a modified, manifestly covariant form.

Y. Nambu. — Regarding Professor Van Hove's remark on Haag's theorem I would like to emphasize the distinction between two different origins of the effect. One is related to the spatial volume or the size of the box which we consider and the other shares the common origin with the ultraviolet divergences. The latter is due to the fact that even in a finite volume there are an infinite number of field oscillators. In some models the divergence difficulties may be overcome. But perhaps we should keep in mind the possibility that Haag's phenomenon can arise from two different physical reasons, namely the continuous nature of space time and the practically infinite volume of the universe.

G. Källén. — I should like to remark that when we write e.g. the function $G(p^2)$ above as $G(p^2) = V \sum_{p^{(z)}=p} |<o \mid A \mid z>|^2$ the states $\mid z>$ that enter in this formula are the physical states including all of the interaction. When these states are classified as two-particle states, three-particle states and so on this classification is made in terms of asymptotic (e.g. incoming) particles. In principle, these states are not the same as the states just indicated by Professor Van Hove. However, in many practical applications one makes approximations, sometimes to the effect that the interactions between certain particles are neglected at one stage or another. In that case, one may not be so very far from the situation described by Professor Van Hove.

G. Chew. — There is a historical question that I have never before had the chance to ask of the people involved. In the forties — at the time when I could not yet call myself a physicist — it is my impression that most of the difficulties of quantum field theory were already recognized. Discouraged by the situation, Heisenberg

proposed that the S-matrix, defined a few years earlier by Wheeler, should be used as the fundamental basis for a theory. Lorentz invariance and unitarity were recognized as essential properties, as was analytic continuation in the energy, and for several years there was great enthusiasm for the S-matrix. The enthusiasm died down, I suppose, because people were not bold enough, then, to assume analyticity in all momentum variables and so found the theory lacking in dynamic content. Also the principles of renormalization were discovered and raised new hopes for field theory. During the fifties however, as Källén has told us, the difficulties of field theory have been confirmed and nothing here seems to have budged for a number of years. Ironically, the studies of field theory have suggested far broader analyticity properties of the S-matrix than were contemplated in the forties, and many of these properties by now have experimental support. As we shall hear tomorrow, it now seems likely that the S-matrix, with full analytic continuation, is dynamically as complete as field theory ever expected to be. The question then is two-fold :

(1) is my impression correct of the early history of S-matrix development ?

(2) how do those people who shared the early enthusiasm feel about it now as a substitute for field theory ?

W. Heisenberg. — I would like to give at least a partial answer to the questions of Chew. When I had worked on the S-matrix for a while in the years 1943 to 1948 I came away from the attempt of construction of a pure S-matrix theory for the following reason : when one constructs a unitary S-matrix from simple assumptions (like a hermitian η-matrix by assuming $S = e^{i\eta}$), such S-matrices always become non analytical at places where they ought to be analytical. But I found it very difficult to construct analytical S-matrices. The only simple way of getting (or guessing) the correct analytical behaviour seemed to be a deduction from a Hamiltonian in the old-fashioned manner. One also could argue that by allowing for an analytical continuation of the S-matrix elements, one actually went away from the energy-shell into a more " local " region. Therefore finally I had the impression that a *simple* definition of a field theory could only be found by stating something about a genuine " local " interaction.

In principle however I agree entirely with Chew's program. It

should be possible to define the S-matrix by postulating some underlying groups as basis of the theory, adding the postulates of unitarity and analyticity and calculating the masses, etc. from some condition of consistency, without any use of an indefinite metric in Hilbert space. My criticism comes only from the practical point of view. I cannot see how one could overcome the enormous complications of such a program. The indefinite metric may just be a practical tool to bring these S-matrix relations (concerning their analytical behaviour) back into the form of a local field theory. In such a theory one can find simple devices for estimating mass-eigenvalues, etc. In the end this theory might just lead to that unitary S-matrix you are looking for.

G. Källén. — Not taking the historical point of view but looking at the situation today, it appears to me that the important difference between an S-matrix theory and a field theory in a broad sense is that the S-matrix theory speaks only of quantities on the energy shell, while a field theory considers also quantities off the shell. Another way of describing this situation is to say that the S-matrix considers everything as happening between $t = -\infty$ and $t = +\infty$. In many purposes this is, of course, a very good approximation but I wonder if it is always so. This would mean that one could completely eliminate time from physical theory and that appears to be a very radical idea.

Another point I should like to ask Professor Heisenberg concerns the indefinite metric. If we have an indefinite metric, the function $G(p^2) = \sum_{p^{(z)}=p} <o \mid A \mid z><z \mid A \mid o>$ is not a sum of positive terms any more. Therefore, it could be negative somewhere. This means that one, with the aid of a suitable test function, could get a negative value for a vacuum fluctuation like

$$<o \mid A(f)\, A(f) \mid o> \sim \int dp \mid f(p) \mid^2 G(p^2).$$

If $A(x)$ is a component of the electromagnetic field this seems to be a statement with physical meaning. What is your interpretation of this ?

W. Heisenberg. — I certainly agree with Källén that paradoxes of this kind might occur occasionaly in an indefinite metric.

But there I would like to remind you of similar paradoxes in

ordinary quantum mechanics, and here I am referring to a paper by Sudershan and some papers by Bopp. One can — as Wigner has shown long time ago — put quantum mechanics into a mathematical form so that it ressembles classical statistical mechanics. One may introduce a density function $f(p, q)$ depending upon the coordinates and momenta of the particles and may write down a kind of Boltzmann equation constructing $\frac{d}{dt} f(p, q)$ by an integral operator acting upon $f(p, q)$. In some very simple cases like the harmonic oscillator the quantum equation is even identical with the classical one. But there is one essential difference between the classical and the quantum theoretical $f(p, q)$. In classical theory the density (or probability) $f(p, q)$ must by definition always be positive, in quantum theory it is not.

This paradox of course can be understood finally by the uncertainty relations. In a similar way I would expect that there will never be measurements by which you could find negative values of $| A(f) |^2$, even if formally such values could appear.

R.P. Feynman. — I would like to give my interpretation of history (for Chew's sake). I think that someone said once that the problem in theoretical physics is to prove yourself wrong as quickly as possible. The difficulty we have had for 27 years is that we haven't been able to prove Yukawa was wrong. I would like to discuss the history of attempts. The central problem at the beginning was to solve equations, figure out the consequences (that is what we used to do in physics once), make experiments and then think of another idea. The best progress is made when this can be done.

In the case of the field theories, other than electrodynamics where there was essentially no difficulty in making the calculations other than infinities, no one has figured out how to make the calculations. So there was an original history of Tamm-Dancoff method, various damping approximations, Salpeter equations and other tricks... One tried to solve these things and people became discouraged. A group of mathematically minded people who were not able to solve the equations tried to prove they had no solution and made no sense. This has not succeeded and absolutely demonstrate that this is essentially or nearly a blind out.

The other way to side track was to try to formulate things in another way. That is where the S-matrix and your attempts to

understand the π-meson without actually using field theory but getting clues from it came in. During all this time, no complete solution either of the S-matrix or of the field equations hasn't really been produced. You sit there and say : why isn't everybody doing S-matrix; another guy says : why isn't anybody doing field theory ? The real problem is : *why is nobody solving anything* ?

One of the reasons why you don't solve the problems is that you don't work hard enough. One of the reasons it is and has always been difficult to work hard on these problems is that nature keeps telling us that it has the quality of being much more elaborate than we thought and that any minute another resonance may come in and give another clue. There has always been a feeling that something is incomplete. But that is a side point.

I see I got some applause for the main point I tried to make. I think it would be a good idea if some people could keep trying.

M. Gell-Mann. — A good feature of the dispersion theory approach is that one works with quantities that are observable or nearly so. While the S-matrix theory is being built, we are learning to understand a great deal about the experiments. We could mention as examples the use of forward scattering and form factor dispersion relations, pology and the current study of high energy diffraction. These applications have not only helped to interpret data, but have stimulated a great deal of experimental work.

F. Dyson. — In reply to the historical question posed by Chew, I would like to state my personal interpretation of the history of field theory during the last 27 years. I believe that the central problem of field theory is to define a precise notion of convergence which makes the solution of an infinite set of equations a meaningful and feasible mathematical operation. We have had four infinite sets of equations, each of which has a good claim to represent the physical content of field theory. These are : ordinary perturbation theory, the Tamm-Dancoff equations, the Lehmann-Zimmermann-Glaser equations, and the Chew-Mandelstam equations. Each in turn has occupied the attention of physicists for 5-10 years. If in any case we had found a workable definition of convergence which made the equations solvable, we would have had a well-defined field theory which could be compared with experiment. In fact no such definition of convergence has been found for any of the four

sets of equations. It is justifiable to hope that the Chew-Mandelstam equation may overcome this difficulty which has stultified the three older attempts to formulate a meaningful field theory. However, the Chew-Mandelstam program is at present at least as far as the other methods from honestly facing up to this problem.

THEORY AND APPLICATIONS OF SINGLE VARIABLE DISPERSION RELATIONS

by **M.L. GOLDBERGER,**

OPENING REMARKS

I find myself in a rather peculiar position at the present time. A number of things which I had intended to say have already been pronounced in a solemn if not pompous fashion during the past three days. Since I went to considerable trouble to prepare my talk in advance, I could, of course, insist on giving it largely verbatim and utter again many of the grand sentiments already voiced by others. I have been a little depressed by the fact that we seem to be largely taking notice of the woods at the expense of not recognizing enough that there are trees present. Today's speakers have been invited to throw down the gauntlet and state where the dispersion theorists or if you prefer S-matrix theorists are, what they have done and perhaps what they can do. Yesterday Chew expressed a certain amazement at the fact that people failed to recognize until recently the possibility of making a true S-matrix theory of the strong interactions and insist on playing with field theory. My own feeling is that we have learned a great deal from field theory as we shall see, even dispersion theory came from it; that I am quite happy to discard it as an old, but rather friendly, mistress who I would even be willing to recognize on the street if I should encounter her again. From a philosophical point of view and certainly from a practical one the S-matrix approach at the moment seems to me by far the most attractive.

I have tried to understand some of the reactionary responses I have met in the past about dispersion theory which have been also expressed here or which I have read into the comments of others

at the conference. One point which springs to mind is that within the framework of Lagrangian (or old fashioned) field theory, one writes down a small number of equations such as the field equations, and the Schrödinger equation and it takes only a few lines to do so. It looks then as though one had the whole theory there and one need only start to work. Similarly, in terms of what is called axiomatic field theory, one can produce an infinite set of equations based on only the finest postulates, which with a suitable notation may be written very compactly. This simplicity, which is only superficial, is contrasted with what appear to be rather arbitrary and intuitive procedures on the part of the dispersion theorists who do not seem to express things very neatly.

On the other hand, the axiomatic field theoreticians are very hard pressed to compute the Klein-Nishina formula. The Lagrangian people can do this, but if we allow the discussion at the conference to degenerate to the physics of the strong interactions, they, to say nothing of the axiomaticians, are absolutely helpless. At this level, the challenge of field theory presented to dispersion theory is non existent. Although the axioms if you like, of the dispersion approach have not been stated in what we might imagine to be their final form, no one is really ever in much doubt as to how to proceed. The reason is that the unashamed dispersion theorist has even been willing to resort to experiment to get ideas and to push a little further his understanding of the strong interactions. Thus as Gell-Mann has put it, he is understanding some physics while developing the theory.

It is perhaps correct to say that much of the deeper philosophy of the S-matrix approach held by some of us, in particular Chew, who believe that there are no elementary particles, and that there are no undetermined dimensionless constants in the theory has not yet been put to a successful test. I should also hasten to point out that if there are some elementary particles, they may be easily incorporated into the scheme.

I feel obliged to turn now to some of the topics which are discussed in my formal report which perhaps some of you have not read. I shall also refer to some more applications of dispersion theory that are not mentioned there.

REPORT

Dispersion theory was invented in 1926 by Kramers and Kronig [1] on the basis of what was essentially classical electrodynamics and the first glimmerings of quantum mechanics. It is reasonable to assume that this work was not vigorously pursued at the time because physicists were so actively involved in solving practical quantum mechanical problems. The rebirth of the theory took place in 1946 when Kronig [2] raised the question of whether causality placed any restrictions on the structure of the Wheeler-Heisenberg

TABLE I.

AMERICANS	EUROPEANS	RUSSIANS
R. Blankenbecler	R. Eden	N. Bogoliubov
G. Chew	S. Fubini	E. Fainberg
R. Cutkowsky	R. Jost	A. Lagunov
S. Drell	H. Lehmann	L. Landau
F. Dyson	R. Omnes	I. Pomeranchuk
W. Frazer	J. Polkinghorne	D. Shirkov
M. Gell-Mann	K. Symanzik	
M. Goldberger	J.C. Taylor	
F. Low	J.G. Taylor	
S. Mac Dowell	W. Thirring	
S. Mandelstam	W. Zimmerman	
Y. Nambu		
R. Oehme		
J. Toll		
S. Treiman		
J. Wheeler		
E. Wigner		
D. Wong		
F. Zachariesen		

S-matrix. Here again the excitement of quantum electrodynamics following the Lamb shift discovery caused people to neglect Kronig's remark. The subject was again revived around 1951 by Wheeler, Toll, Wigner, van Kampen and others. The first attempt to study the problem within the framework of quantum field theory was made in 1954 by Gell-Mann, Goldberger and Thirring.[3] Since there was nothing around at that time to distract anyone, there followed an explosion of activity as is evident to the most casual reader of journals over the past seven years. So much has been done that only a few of the high spots can be mentioned here. In

order to partially compensate those important workers whose names I might not mention explicitly later, let me list in Table I some prominent dispersion theorists.

By way of introduction, let me remind you of the original Kramers-Kronig dispersion relation. It is a relation between the forward light scattering amplitude and an integral over the total cross-section for the removal of light from the beam :

$$\begin{aligned} \mathrm{Re}\, f(\omega) &= -\frac{e^2}{mc^2} + \frac{2\omega^2}{\pi}\, \mathrm{P} \int_0^\infty d\omega' \, \frac{\omega' \, \mathrm{Im}\, f(\omega')}{\omega'^2 - \omega^2} \\ &= -\frac{e^2}{mc^2} + \frac{\omega^2}{2\pi^2 c}\, \mathrm{P} \int_0^\infty d\omega' \, \frac{\sigma(\omega')}{\omega'^2 - \omega^2} \end{aligned} \tag{1}$$

where in going from the first line to the second we have used the so-called optical theorem,

$$\mathrm{Im}\, f(\omega) = \frac{\omega\, \sigma(\omega)}{4\pi c} \tag{2}$$

relating the total cross-section to the imaginary part of the forward scattering amplitude. The charge and mass of the target are e and m, respectively. The integral is to be taken in the sense of principal values.

As a practical example of how such an equation may be used, consider the scattering of light by a Coulomb field. The first term is absent in this case. The scattering amplitude being proportional to e^2 contributes to the total cross-section to order e^4. There is however an e^2 cross-section, namely that for the creation of an electron-positron pair in the field. If we have a means of computing this, we get the forward scattering amplitude to order e^2. If it were possible to free ourselves of the restriction to forward scattering we might have a means of computing the whole perturbation series for the scattering amplitude after once having gotten started. It is important to notice that only physically measurable quantities enter into this equation and that there are no renormalization constants of the like. We shall return to this point.

The theoretical basis for the Kramers-Kronig relation is the remark that the measurement of two components of the electro-

magnetic field operators carried out at points with space-like separation should not interfere with each other. Mathematically this statement which sounds eminently reasonable must be expressed in a way which is not so close to physics as we might like. It takes the form that the commutator (or anti-commutator in the case of particles which obey Fermi statistics) of two Heisenberg field operators at space-like points shall vanish. (Actually, in the case of two particle scattering processes, it is only a particular matrix element of such an object which is required to vanish.) For the special case of electrodynamics where we are concerned with light scattering our commutator condition (sometimes called a causality condition, or local commutativity) could perhaps by Bohr-Rosenfeld methods be placed in direct relationship with measurement. For the corresponding situation involving massive bosons which we shall take up shortly, the field operators used for their description have no classical interpretation. The statement that the commutator condition has something to do with measurement or causality is far from clear. It is hardly necessary to comment that the situation is even less clear in the case of Fermi fields. Nevertheless, for whatever it means, the commutator condition plays a fundamental role in all derivations of dispersion relations.

It is frequently suggested that one try to modify the commutator condition by demanding that it hold only for space-like separations greater than some amount, a. It can be shown that such a demand is essentially impossible to achieve if one adheres to the accepted rules of field theory, those which are reverently referred to as the axioms. Physically one would expect that if one could violate causality by times of the order of a/c, then by a clever arrangement of scatterers, one could obtain a macroscopic violation of common sense. At the present stage of field theory it seems that we require the validity of the commutator condition for arbitrarily small space-like separations.

The first important generalization of the Kramers-Kronig relations was to treat the forward scattering of mesons by nucleons.[4] Because of the finite rest mass of the mesons, the energy region between o and μ, the meson mass, is not accessible experimentally. This unphysical region in which the meson has imaginary momentum plays a very important role in the theory. A meson of negative kinetic energy can be absorbed by a nucleon with nothing else but

a nucleon coming out. The dispersion relations which describe forward pion nucleon scattering are shown in the next equations :

$$\begin{aligned} Re\, T^{(+)}(\omega) - Re\, T^{(+)}(\mu) &= \frac{f^2}{m}\frac{k^2}{\omega^2 - (\mu^2/2m)^2} \\ &+ \frac{k^2}{2\pi^2} P \int_{\mu}^{\infty} d\omega' \frac{\omega'}{2k'} \frac{\sigma - (\omega^2) + \sigma + (\omega')}{\omega'^2 - \omega^2} \\ Re\, T^{(-)}(\omega) &= f^2 \frac{\omega}{\omega^2 - (\mu^2/2m)^2} \\ &+ \frac{\omega}{2\pi^2} P \int_{\mu}^{\infty} d\omega' \frac{k'}{2} \frac{\sigma - (\omega') - \sigma + (\omega')}{\omega'^2 - \omega^2} \end{aligned} \tag{3}$$

In these equations, ω is the total energy of the meson in the laboratory system ($\omega^2 = \mu^2 + k^2$) where the nucleon of mass m is initially and finally at rest. If T_{π^+} and T_{π^-} represent the scattering amplitude in the laboratory system for π^+ and π^- mesons, respectively, we have $T^{\pm} = 1/2\,(T_{\pi^-} \pm T_{\pi^+})$. The constant f^2 is so-called pion-nucleon coupling constant and measures the probality of the peculiar process nucleon + meson $\rightarrow$ nucleon. The energy at which such things happen is $\omega = \mu^2/2m$ which is seen to be a place where the amplitudes have a pole; these pole terms represent the contribution of the unphysical region $o < \omega < \mu$ which is otherwise void. The fact that there is such a pole suggests the possibility of extrapolating the experimental data to this point and determining the residue which is essentially f^2. A slight rearrangement of the equation for T^-, namely

$$\begin{aligned} &\left\{\frac{Re\, T^{(-)}(\omega)}{\omega} - \frac{\omega^2}{4\pi^2} P \int_{\mu}^{\infty} d\omega' \frac{k'}{\omega'^2} \frac{\sigma - (\omega') - \sigma + (\omega')}{\omega'^2 - \omega^2}\right\} [\omega^2 - (\mu^2/2m^{\,2}] \\ &= 2f^2 + \frac{\omega^2 - (\mu^2/2m)^2}{4\pi^2} \int_{\mu}^{\infty} d\omega \frac{k}{\omega^2} [\sigma_-(\omega) - \sigma_+(\omega)] \end{aligned} \tag{4}$$

shows that the very complicated combination of experimentally measurable quantities on the left hand side of this equation is a linear function of ω^2. From a measurement of the intercept of this line extrapolated to $\omega^2 = (\mu^2/2m)^2$ we can determine f^2. The numerical value obtained by this method is $f^2 = 0.08$. In mathematical language the dispersion relations state that $T^+(\omega)$ and $T^{(-)}(\omega)$ may be extended to functions of a complex variable $z = \omega^2$ which are analytic everywhere in the z-plane cut from μ^2 to ∞

except for poles at $z = (\mu^2/2m)^2$. An assumption about their behavior at infinity is required to write an explicit representation.

In writing the dispersion relation for T^+ we have made what is referred to in the trade as a substraction, arbitrarily taken at zero kinetic energy. The fact that a new parameter, $T^+(\mu)$, must be introduced is sometimes said to be a reflection of the fact that in meson theory there are two parameters, namely f, the pion-nucleon coupling constant, and the so called pion-pion interaction constant. This subtraction is made in order to suppress the contribution to the integral from very high energies. If the unsubtracted version, analogous to what we wrote for T^-, had been used, on the basis of what is known about the cross-sections experimentally the integral would not converge. As written, the cross-sections must have the property that $\sigma_- + \sigma_+$ not increase as fast as linearly with energy and $\sigma_- - \sigma_+$ must approach zero faster than $1/ln\omega$, assuming that this difference does not oscillate infinitely fast at high energies. This brings us however to the interesting and important question of what cross-sections can be expected to do at high energies to which we return later. Before leaving these pion-nucleon dispersion relations, it is worth pointing out that as written, they are consistent with experiment up to several Bev. It is of the greatest importance that this consistency check be carried out at the higher energies now becoming available. Verification of the theoretical predictions does not by any means prove that quantum field theory is correct any more than the Bohr atom predictions of the Balmer series proved that picture. On the other hand, if there should appear a discrepancy between theory and experiment the shock would be at least as profound as that caused by the non-conservation of parity, if not more so. As we shall see the elements of field theory that go into the derivation are those which would be abandoned with only the greatest reluctance. From the standpoint of a pure dispersion theory form of a dynamical theory (to which we allude later) the effects would be fatal.

The coupling constant determination we referred to in connection with the pion-nucleon dispersion relations is a special case of the science of polology invented by Chew and Low.[5] This concept is based on the observation that scattering amplitudes regarded as analytic functions of energy or momentum transfer may be expected

to have poles, sometimes, at certain unphysical values of these variables; under some circumstances these lie sufficiently close to the physical region that one can make a meaningful extrapolation of the data to them. The residues are frequently directly relatable to quantities of physical interest. Some examples of interest are shown in figure 1. In every case one wants to extrapolate to the

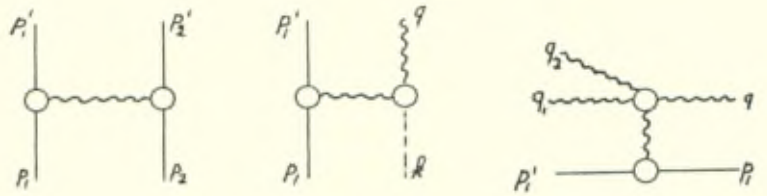

Fig. 1. — Examples of pololog; in every case one attemps to extrapolate to the place where $(p_1 - p_1')^2 = -\mu^2$, that is, where these general classes of Feynman graphs have poles.

point where the four momentum transfer $(p_1 - p_1')^2 = (\vec{p}_1 - \vec{p}_1')^2 - (E_1 - E_1')^2 = -\mu^2$ which of course cannot happen for a physical scattering. [$p_1 - p_1'$, since the masses of the particles are the same, is a space-like vector and hence has a positive square.] In the case of the nucleon-nucleon scattering and the photomeson production, the residue of the pole leads to a coupling constant determination. In the pion production process one hopes to measure the cross-section for π-π scattering.

I have referred only briefly at the beginning to the use of dispersion relations as a dynamical tool. We might imagine them to be either a supplement to field theory or perhaps a replacement. In order to make any progress along these lines in the case of scattering processes it was necessary to generalize the dispersion relations to describe non-forward scattering. It turns out to be possible to do this and one ultimately obtains expressions which have very much the structure of our forward scattering equations. They relate the real part of the scattering amplitude to an integral over the imaginary part but now give the energy dependence at a fixed, non-zero, value of momentum transfer less than some maximum value depending

on various relevant masses. There is no physical basis for this limitation; most people, including me, believe that it is mathematical in origin and that there is no restriction on momentum transfer. The fact that we must now hold the momentum transfer fixed in our dispersion relations causes grave complications because the unphysical region, corresponding to scatterings in which the fixed momentum transfer is larger than what can physically be supplied, now becomes a place of real horror. Furthermore, the previously rather innocuous subtraction constants become functions of momentum transfer. All of these problems are no longer worrisome with the development of the so-called double dispersion representation which will be described by Professor Mandelstam. It should be said that in the hands of determined dispersion theorists [6] a reasonably adequate description of much of low energy pion physics was obtained. We were helped enormously by two "accidents" of nature, namely the resonance in the pion-nucleon system which largely dominates the picture and the smallness of the pion-pion interaction at low energies.

There are a number of other processes which have been effectively studied within the framework of single variable dispersion relations. There are three typical applications worth mentioning briefly. These are the electromagnetic structure of nucleons and pions, the decay of the π-meson, and the Pomeranchuk theorem. Consider the first of these; we discuss the scattering of a nucleon by a virtual photon which transfers four-momentum q to the nucleon. The amplitude for such a process may in all probability be expressed as

$$F(q^2) = \int_{m_0^2}^{\infty} \frac{dm^2}{\pi} \frac{\rho(m^2)}{m^2 + q^2 - i\varepsilon} \tag{5}$$

where m represents the mass of the virtual intermediate states created by the photon with m_0 being the minimum such allowed by selection rules. The computation of $\rho(m^2) = Im\, F(-m^2)$ is clearly the theoretical task. For small q^2, the lowest mass states are expected to be the most important unless ρ is quite peculiar. This qualitative feature is quite characteristic of the dispersion approach and gives rise to profound differences between it and straight perturbation methods. The guiding philosophy is that in low energy

phenomena, the least massive states are the most important — small energy denominators are assumed to win over possibly large energy numerators.

In figure 2 we give the dispersion analysis of the electromagnetic vertex. According to our announced philosophy we regard the

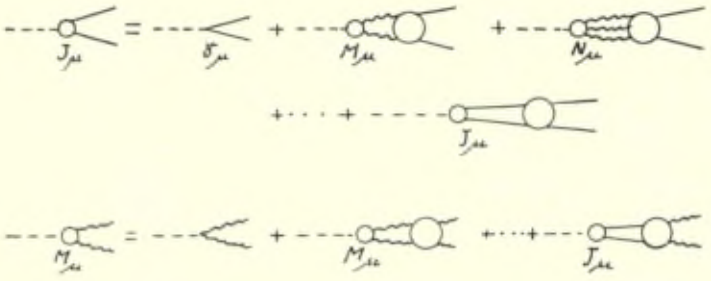

Fig. 2. — Dispersion diagrams for nucleon form factor J_μ [1] M_μ represents the pion form factor and its dispersion analysis is shown in the last line. The three pion vertex N_μ should be similarly discussed.

least massive intermediate states as the most important. We include the nucleon-antinucleon state for historical reasons only. This contribution, which comes along with that of the two pion state in ordinary perturbation theory can be reliably estimated and shown to make a negligible contribution at low energies. The two pion state seems to dominate the isotopic vector nucleon structure. It was pointed out by Federbush, Goldberger, and Treiman [7] that the electromagnetic structure of the pion itself might be of great importance in understanding nucleon structure. Frazer and Fulco [8] subsequently showed that a semi-quantitative understanding of the iso-vector structure could be obtained by attributing a sharp resonance to the π-π interaction. This resonance has now been established experimentally. The isotopic scalar part of the nucleon structure has also been studied by dispersion methods with the result that to obtain agreement with experiment one requires a strong three pion interaction, perhaps almost a bound state of a variety first suggested by Nambu. Such a quasi-bound state has recently been observed.

The problem of the decay of the charged pion into leptons may also be discussed in a very similar fashion. The dispersion analysis of this process is shown in figure 3. In spite of our fondness for

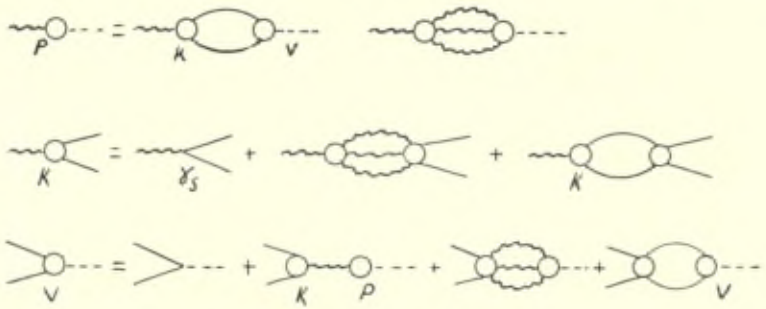

Fig. 3. — Dispersion analysis of π-decay vertex, P, into leptons (shown as a dotted line). The structure of the nucleon vertex K is shown immediately below and that of the nucleon decay into leptons is the last row. The three pion states are consistently omitted.

low mass states we by-pass the three pion state in favor of that with a nucleon-antinucleon pair. There are two reasons for doing this : one is that the subsequent coupling of the three pion state to the leptons is very involved and, two, there is a general feeling that the lepton interactions are more directly associated with nucleons than with pions. Proceeding on the basis of this model, Goldberger and Treiman [9] computed the lifetime of the pion and found phenomenal agreement with experiment. Many people, in particular Gell-Mann and Nambu, have endeavored to show that the result of G.-T. may well be more correct than the methods used to obtain it. In particular, they have shown how the initial step — the trajectory involving the nucleon-antinucleon pairs — may be avoided and replaced by a more convincing procedure.

The final application of single variable dispersion relations which we mention is that leading to a statement about the behavior of cross-sections at high energies. The experimental fact is that they appear to be approaching constant values : π-nucleon, 30 *mb*; nucleon-nucleon, 40 *mb*; K-nucleon, 20 *mb*. There is a simple if not altogether convincing argument by Pomeranchuk [10] which is relevant here. He makes the assumption that if we take a fixed high energy, the phase shifts for all angular momenta greater than a certain maximum one are zero. The scattering amplitude in absolute value can then

not increase more rapidly than linearly with energy if we make the classical association of angular momentum and impact parameter, $l_{max} = kR$ where k is the wave-number and R is some radius. The total cross-section then approaches a constant. If this information is put into the forward dispersion relations one finds that the real part of the amplitude increases like E *ln* E *unless* and this is the Pomeranchuk theorem, the constant cross-sections for particle and antiparticle are the same. This seems to agree with experience for π-N, N-N and N-N but not K-N, $\bar{K}$-N.

It is very hard to understand why cross-sections tend to approach constants. The most natural constant is, of course, zero. An argument has recently been given by Froissart [11] based on the Mandelstam representation which leads to $\sigma < (ln\, E)^2$. I am by definition forbidden to talk about this. A somewhat weaker result has been proved by Greenberg and Low [12] which does fall under my franchise which is very important and is well worth mentioning.

In the course of studying the analytic properties of scattering amplitudes, Lehmann [13] was able to show, using rigorous methods of axiomatic field theory [and making heavy use of a remarkable integral representation of the matrix element of the commutator of two field operators given by Dyson] that for a fixed energy the amplitude is an analytic function of the cosine of the scattering angle inside a certain ellipse. The size of axes of the ellipse which encloses the physical region $-1 < \cos\theta < +1$, depend on the energy and various masses in the problem. Making use of this information and the unitarity relation for partial wave amplitude, Greenberg and Low were able to show that $\sigma < E(ln\, E)^2$. Their method consists simply of the following steps : the scattering amplitude may be written (say for pion-nucleon scattering) as

$$F(W, z) = \frac{4\pi W}{mk} \times \sum_{l=o}^{\infty} (2l + 1)\, a_l(W)\, P_l(z) \qquad (6)$$

where W is the total energy, k the relative momentum and m the target mass. The unitarity requirements on a_l is $|a_l| < 1$. Now note that

$$a_l(W) = \frac{mk}{4\pi W} \int_{-1}^{+1} \frac{dz}{2}\, P_l(z)\, F(W, z)$$

$$= \frac{mk}{4\pi W} \oint \frac{d\zeta}{2\pi i} F(W, \zeta) \int_{-1}^{+1} \frac{dz}{2} \frac{P_l(z)}{\zeta - z}$$

$$= \frac{mk}{4\pi W} \oint \frac{d\zeta}{2\pi i} F(W, \zeta)\, Q_l(\zeta) \qquad (7)$$

where the contour integral extends over an ellipse just inside the Lehmann ellipse and the Q_l are Legendre functions of the second kind. Now form $|F(W, z)|$ and break the partial wave sum into two parts $l < l_o - 1$ and $l > l_o$; in the first region, replace $|a_l|$ by unity, in the second, use the above formula. We obtain in this manner, (for the forward direction, $z = 1$)

$$|F(W, 1)| < \frac{4\pi W}{mk} \sum_{o}^{l_o - 1} (2l + 1)$$

$$+ \left| \oint \frac{d\zeta}{2\pi i} \frac{l_o + 1}{\zeta - 1} [Q_{l_o + 1}(\zeta) - Q_{l_o}(\zeta)] \times F(W, \zeta) \right| \qquad (8)$$

If we assume that $F(W, \zeta)$ does not increase faster than a polynomial in W, an assumption involved in Lehmann's derivation of the analyticity, we may estimate the second term above using a standard limit on the Q_l. Using the geometry of the ellipse and choosing l_o so that the two terms above make equal contribution, one finds

$$l_o \sim W^2 \ln W^2 \qquad (9)$$

for large W. Thus $Im\, F(W, 1) < W^4 \ln^2 W^2$ which in turn leads to a limit on the total cross-section

$$\sigma_{tot} < E \ln^2 E \qquad (10)$$

where E is the laboratory energy ($E \sim W^2$).

One cannot see in the work of Greenberg and Low or of Froissart how one can achieve a constant cross-section. A very crude physical picture of why even elastic cross-sections don't necessarily go to zero at infinite energies and which might almost account for constant total cross-sections is the following : as the energy increases the two particle amplitude has its very existence threatened by the opening up of more and more channels. It is a fundamental quantum mechanical principle that wave functions do not like to die and tend strongly to avoid regions of sudden death. Thus as the energy increases, the wave function is pushed outside the strongly reacting

region. The exact size is of course far from clear but it is doubtless associated with the longest range effects which can lead to inelastic processes directly — hence the one or two pion exchange interactions.

Let me return now finally to the question of the theoretical basis for dispersion relations and their dependence on and perhaps independence from quantum field theory.[14] We have already discussed at length the commutator condition. In the rigorous derivations from the axioms very little of the detailed dynamical aspects of the theory play any role, particularly in the case of the forward scattering relations. For example it is only the pseudo-scalarity of the pion which enters in the evaluation of the residue of the pole terms in the pion-nucleon relations. The Lagrangian type question — is the interaction pseudo-scalar or pseudo-vector — is irrelevant here. There are many important physical processes for which no rigorous demonstrations have yet been given, the most notable example is that of nucleon-nucleon scattering. It has been possible in some cases to derive dispersion relations like that for N-N scattering by studying the perturbation series to all orders. In spite of the fact that perturbation theory does seem to be a solution of the axiomatic equations such derivations are not taken too seriously by the axiomaticians. The derivations of non-forward scattering dispersion relations, first given in an unbelievable tour de force by Bogoliubov,[15] are ugly, involved, unrewarding and uninstructive. It is reasonable to believe that there is a formulation of the theory in which these results would be almost automatic. It is worth commenting that the non-forward relations have never been subjected to experimental verification. The present status of what dispersion relations have been proved is given in Table II.

As we have remarked earlier, the fact that dispersion relations have been thus far in agreement with experiment hardly represents any sort of triumph for quantum field theory. Very little of any such theory has of course ever been explored in detail, even in the best studied example, quantum electrodynamics. This remark applies with equal vigor to "old fashioned" theories based on Lagrangians and canonical quantization methods and to the much more respectable and less detailed axiomatic schemes. Aside from the so-called low energy theorems [16] and the dispersion relations there is only one peculiarly field theoretical principle (not entirely

TABLE II.

DISPERSIONS RELATIONS

a) **Proved relations**

Process $k + p \to k' + p'$	Limitation in invariant momentum transfer $\Delta^2 = \frac{1}{4}\left\{(\vec{k} - \vec{k}')^2 - (k_0 - k_0')^2\right\}$	Continuation of absorptive part into the unphysical region by convergent partial wave expansion
$\pi + N \quad \pi + N$	$\Delta^2 \max = \frac{8\mu^2}{3}\frac{2m + \mu}{2m - \mu}$	$0 < \Delta^2 < \Delta^2 \max$
$\pi + \pi \quad \pi + \pi$	$\Delta^2 \max = 7\mu^2$	$0 < \Delta^2 < \Delta^2 \max$
$\gamma + N \quad \gamma + N$ (*)	$\Delta^2 \max = \mu^2\left\{\frac{(2m + \mu)^2}{4(m + \mu)^2} + \frac{2m + \mu}{m}\right\}$	$0 < \Delta^2 < \Delta^2 \max$
$\gamma + N \quad \pi + N$ (*)	$\Delta^2 \max = F(0)$ (**) $\sim 3\mu^2$	$\Delta^2 th < \Delta^2 < \Delta^2 \max$
$e + N \quad e + \pi + N$ (*)	$\Delta^2 \max = F(\gamma)$ (**); $\gamma = k_0^2 - k_2$ $F(-9\mu^2) \sim 6\mu^2$	$\Delta^2 th = \frac{m}{m + \mu}\frac{\mu^2 - \gamma}{4}$

b) **Some unproved relations**

	Mass restrictions appearing in proof based upon causality and spectrum; $\Delta^2 = 0$	Perturbation theory (every finite order)
$N + N \quad N + N$	$\mu > (2 - 1)m$	proved for $\Delta^2 < \frac{\mu^2}{4}$
$K + N \quad K + N$	complicated; not fulfilled by narrow margin	$\Delta^2 \max > 0$, value uncertain
$\pi + D \quad \pi + D$	$\varepsilon > -\frac{\mu}{3}$; $M_D = 2m - \varepsilon$	

(*) Lowest order in the electromagnetic interaction.

(**) For $\gamma < -3\mu^2$:

$$F(\gamma) = \frac{m}{m + \mu}\frac{\mu^2 - \gamma}{4} + \sqrt{\frac{8\mu^2}{3}\frac{2m + \mu}{2m - \mu}\frac{4\mu^2 - \gamma}{2}\frac{2m + \mu}{2m}}$$

$$\times \frac{1}{2}\left\{1 + \sqrt{1 + \frac{\mu^2 - \gamma}{4\mu^2 - \gamma}\frac{m}{(m + \mu)^2}\frac{(2m + \mu)^2 - \gamma}{2m + \mu}}\right\}$$

for $\gamma < -3\mu^2$: $F(\gamma)$ can be calculated numerically.

TABLE II (*continued*).

VERTEX FUNCTIONS

Representations

Vertex	Mass restrictions	Remarks	Perturbation theory (finite order)
$<N \mid A_\mu \mid N>$	$\mu > (2\sqrt{2} - 1)\, m$	Representations do not follow from causality and spectrum alone	proved
$<N \mid (\Box + \mu^2) \varnothing_\pi \mid N>$	$\mu > (2\sqrt{2} - 1)\, m$		proved
$<\pi \mid (\gamma \frac{\partial}{\partial \gamma} + m)\, \psi \mid N>$	proved		
$<\pi \mid (\Box + \mu^2) \varnothing_\pi \mid \pi>$	proved		

unrelated to dispersion theory) which is known. This is crossing symmetry.[17] A simple illustration of this is the following : if the amplitude for pion nucleon scattering [with initial (final) pion having isotopic spins $j(i)$ and four-momentum $q(q')$] is $M_{ij}(q', q)$, then $M_{ij}(q', q) = M_{ji}(-q, -q')$. This is not a physical symmetry operation like time inversion or charge conjugation. It is a rather strange relation which can be given precise meaning only in terms of analytic continuation of scattering amplitudes. A consequence of it is that at zero total energy pion nucleon scattering should show no isotopic spin dependence. Perhaps it is possible to think of others.

I would like to draw attention to one rather strange relation between dispersion theory and conventional field theory. If one writes down the familiar Källén-Lehmann representation of say the Feynman propagation function for a nucleon it has the form (symbolically)

$$S\ (p) = \frac{1}{i\gamma \cdot p + m} + \left[\int_{-\infty}^{-(m+\mu)} + \int_{(m+\mu)}^{\infty} \right] \frac{d\sigma p\,(\sigma)}{i\gamma \cdot p + \sigma} \quad (11)$$

(where the γ_μ are the usual Dirac matrices, and p the four momentum of the particle, m the nucleon mass, μ the pion mass). This is

essentially a dispersion relation. If now ρ is computed (say in ordinary meson theory) to lowest non-vanishing order of perturbation theory it is finite; when substituted into the representation the result is that of *renormalized perturbation theory*. Somehow the requirement of the dispersion representation has forced the renormalization. It is not clear to me whether this is an accident or perhaps rather deep.

In conclusion, let me say that if we could do serious and accurate computations in quantum field theory the fact that amplitudes had certain analytic properties would be an amusing but relatively uninteresting observation. The fact is that at the present we do not really know what we have theoretically, whether the axiomatic scheme is rich enough to contain all the embarrassingly large number of particles or even more modestly anything at all relevant to experience. In some ways the dispersion approach extracts the best of field theory and has begun to acquire a character of its own. The way this story unfolds, the next logical step after the single variable dispersion relations, and the crucial one, without which there is no hope of formulating a dynamical theory, is the subject of the next address.

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Discussion du rapport de Goldberger

J.R. Oppenheimer. — As Dr. Goldberger said, the approximation scheme used in applying dispersion relations, looking for nearby singularities and thus long range or long lasting effects, is wholly unrelated to perturbation theory, as in quantum electrodynamics. But it is a relativistic generalization of approximation schemes used before, during and after the war, based on the smallness of $\frac{\mu}{M}$.

A. Pais. — In the several successes of the dispersion approach in π-meson physics which Goldberger has mentioned the dominance of the phenomena by the 3 - 3 resonance plays an important role as a given input. What is the status of attempts to find the location and width of this resonance by the dispersion technique itself and like-wise what can one say in this framework about the so-called higher resonances ?

M.L. Goldberger. — A dynamical calculation by Frautschi and Walecka does indeed predict a 3 - 3 resonance. The location and width don't agree very well with experiment nor is the accuracy of the calculation easy to asset. It is remarkable though, that a resonance is predicted at all. The situation with the S-wave or small *p*-wave phase shifts is much less certain.

G. Chew. — By good fortune the single-nucleon exchange force dominates the π - N interaction in the *p*-state and leads to the correct assignment of the quantum numbers and width of the (3 - 3) resonance. The resonance energy, however, if all other forces are neglected, is wrong by about a factor two. Presumably the next most important force is associated with exchange of the (3 - 3) resonance itself, but because the angular momentum of the exchanged " particle " is 3/2, Frautschi and Walecka had here to introduce a cut-off. Mandelstam will explain this afternoon why

forces due to exchange of states with $J \geqslant 1$ have heretofore always required cut-offs.

R.P. Feynman. — 1) For light, or any system of mass zero, the dispersion relation for forward scattering results directly from the assumption that no scattered wave can arrive sooner than the direct wave. Can the forward scattering relation for finite mass be obtained in any analogous manner (other than from the zero commutator condition) ?

2) I understand that $[\varphi(x), \varphi(y)] = 0$ if $x - y$ is space-like and φ are bare particle operators. Does this imply that it is also true for dressed operators, or say if φ represents the creation amplitude for a deuteron ?

M.L. Goldberger. — 1) The commutator relation applies to the renormalized field operators. In a perturbation sense it may be derived from the non interacting field case.

If the renormalization constants are zero or infinite there are no " unrenormalized " quantities and one then simply *postulates* the vanishing of the dressed field commutator.

2) It seems impossible to derive the dispersion relation for massive particles in a manner similar to what one can do for photons. One cannot pass to the classical limit of sharp fronted wave packets and the simple argument fails. A. Bohr & Mottleson tried this a few years ago and it just does not work.

G. Wentzel. — Doesn't the concept of a " dressed particle " imply that this particle has some spatial extension ? If two particles, i.e. their respective dressings, overlap in space, shouldn't they be able to know of each other, to transmit information to each other ?

W. Heisenberg. — Isn't there a paper by Zimmermann in which he treated outgoing compound states and where he could prove that the asymptotic operators of these compound states, taken as functions of the center of gravity of these states, must commute at space-like distances ?

L. Van Hove. — In renormalisable field theory the relation between " dressed " field ψ' and " bare " field ψ is formally quite simple : $\psi' = Z_2^{1/2} \psi$ where Z_2 is a c-number. This provides a

trivial answer to Feynman's question on local commutativity (although Z_2 is probably zero !). This of course also means that the so-called " dressed " field is dressed very little.

S. Mandelstam. — With regard to Zimmermann's " operators " corresponding to " non-elementary " particles, e.g. the deuteron, I do not think the operators correspond to the particles themselves since, in a theory with two particles of the same quantum number, there would not necessarily be two operators. The operators correspond more to creating two units of baryonic charge at a point and, as such, the size of the deuteron does not enter.

A.S. Wightmann. — I think that if there where two deuterons of the same quantum numbers, Zimmermann's construction would yield two local fields provided that the basic field (or fields) in terms of which he works is complete. I agree that the size of the deuteron does not appear explicitly in the commutation relations of the deuteron field.

M.L. Goldberger. — Has anyone ever made any effort to correlate the commutator condition with any measurable properties ?

N. Bohr. — These questions have some connection with the way one tried to study the measurement problem in quantum electrodynamics. The paper which Rosenfeld and I wrote about this problem deals, in spite of its length, with a very simple point : our real concern was actually, what do we understand by a physical description, how can we make a consistent use of the ideas of space and time, in the account of electrodynamic phenomena ? In this connection, such questions arise as the conditions under which field quantities can be said to be exactly commutable. Now Rosenfeld, I think I have expressed myself so loosely. Would you not comment on this ?

L. Rosenfeld. — In quantum electrodynamics, one has a clear situation, because on the one hand, there is a well defined correspondence with classical theory for the idealization of field component and current density, and on the other hand the smallness of the coupling justifies a procedure of successive approximation, in the first stage of which only the field is quantized (while the sources

are treated classically), and in the second stage the quantization of the sources is introduced and the consequences of the pair production process analyzed separately. When dealing with mesons, such a separation is no longer possible, and the predictions of the present theory with respect to possibilities of measurements of field and current averages are not well defined (except in trivial cases).

Historically the incentive to discuss the measurement problem in electrodynamics was that at that time one had doubts about the consistency of the quantization of electrodynamics because of the divergences. The conclusion one could draw from the analysis of the measurement problem was that the scheme was actually consistent, inasmuch as there was no limit within the scheme of electrodynamics to the application of the concepts of field and current density. Of course, now that we have understood the origin of the divergences and recognized that in electrodynamics they are not essential, we realize that the possibility of a detailed check of the theoretical predictions about the measurability of fields and currents is a foregone conclusion : it is just part of the general consistency of electrodynamics. In meson theory, on the other hand, we are faced with inconsistencies at the very first step; if we apply the usual procedures of field theory, everything diverges from the start. It is then difficult to see how the problem of measurability could even be formulated on the present basis.

N. Bohr. — I might perhaps add that the use of the ideas of space and time in electrodynamics is implied in a definite way by the measuring processes. Indeed, in pure field theory, involving as specific constants only the velocity of light and the quantum of action, there is no absolute scale of spatio-temporal dimensions, and it is therefore possible to disregard the atomic constitution of the measuring instruments. In electrodynamics, a scale is introduced by the electron mass, and, as stressed in our second paper, the scope of actual measurements is therefore limited in principle. In meson theory, as emphasized by Rosenfeld, the whole situation is more involved and the scope of measurements cannot be unambiguously determined at the present stage of experimental evidence and theoretical conceptions.

M.L. Goldberger. — Can one relate the commutator condition

for massive particles to measurements perhaps by the method of Gell-Mann's collisions with some pseudo-weak interactions ?

M. Gell-Mann. — Let us speak of local operators in field theory like the currents and densities I discussed. Only two of these are reasonable in the strict sense, namely $j_\mu(x)$ and $\Theta_{\alpha\beta}(x)$ which can be coupled to weak external classical fields. Of course, in practice, we cannot supply such fields with arbitrarily high wave numbers and frequencies.

To treat the weak current J_α for baryons and mesons, we consider the weak interaction with leptons to first order only. (This may not be a good approximation for very high frequencies, of course.) The leptons are free particles except for the perturbation. If we then measure the scattering of leptons (say $\bar{\upsilon} + p \to n + e^+$), we can deduce the matrix element of J_α which is a factor of the effective first order interaction.

We may think of any local field operator in this way, by *inventing* fictitious particles like the leptons which are free except for a first order hypothetical coupling to the operator in question.

W. Heisenberg. — May I go on with a historical remark in order to bring out a special point concerning the practical applicability of dispersion relations ? Dispersion theory was already fashionable some fifty years ago, before the Bohr's theory of the atom. At that time, one had to introduce linear oscillators in order to explain the sharp spectral lines, and one could invent couplings between the oscillators. There was an old professor in Göttingen, Woldemar Voigt, who became interested in the anomalous Zeeman effect of the sodium D-lines. He did not know anything of Bohr's theory. Therefore he invented coupled oscillators to explain the two D-lines and he could arrange the couplings in such a manner that he got the correct anomalous Zeeman effect. Moreover he could get the Paschen-Back effect at very high magnetic fields, and he derived very complicated expressions with long square-roots for frequencies and intensities in the intermediate range. Some 15 years later, Jordan and I calculated the anomalous Zeeman effect of the sodium D-lines from quantum mechanics. To our great surprise we found exactly the same complicated formulas as Voigt, without any change. How could that be ? The answer is very simple. Coupled oscillators mean : a set of linear equations. A secular

determinant in quantum mechanics also means : a set of linear equations. Therefore the two sets had to be identical, if they should describe essentially the same things.

Now I am coming to my point : Voigt's paper was an enormous success of dispersion theory. On the other hand, one could scarcely imagine that Voigt from his line of approach could have found Bohr's theory of the atom. In other words, dispersion theory may work extremely well when one can consider a few lines, or a few elementary particles, as isolated from the rest of lines or particles.

But this will only in rare cases be a good approximation. In the other cases we must consider the whole spectrum and therefore we need first an understanding of the structure of the spectrum or the physical content underlying this spectrum. When we think of the roughly 30 different elementary particles which are known so far, and the roughly 100 excited resonance states that will probably be found by the experimental physicists in a near future, we see that the spectrum we have to explain is at least as complicated as that of the sodium atom or the iron atom. Therefore we must try to describe this spectrum and its structure in a compact way like the Schrödinger equation of the sodium-atom. I doubt whether the dispersion theory can in any simple way lead to such a compact description.

May I go from this historical remark to the question : what is, from an axiomatic point of view, the relation between the two extreme views, expressed at this meeting : orthodox field theory or pure S-matrix theory ? How are these extreme views related to a field theory with indefinite metric in Hilbert space ?

The S-matrix theory has the widest axiomatic frame : one only postulates the existence of a unitary S-matrix with reasonable (causal) analytic properties, representing the group structure given by the experiments.

If one adds, in order to get a more narrow frame, the postulate of the existence of a local field operator (commuting (or anti-commuting) at space-like distances), one comes to a field theory with a (possibly) indefinite metric, which would allow the description of a local interaction.

If one further adds the axioms that the asymptotic operators should be sufficient to construct the complete Hilbert space, and that its metric shall be definite, one comes to the orthodox field theory.

From this comparison, one learns that the theory with indefinite metric is just in the middle between the two extreme views. It seems to me a reasonable compromise, which on the one hand is close enough to the experiments (it allows to calculate mass eigen-values and cross-sections) and which on the other hand may allow to write down in a compact form the structure of the spectrum of elementary particles.

Therefore, I can conclude with my " ceterum censeo " : we should take this possibility of an indefinite metric in Hilbert space very seriously.

R.P. Feynman. — The nearest I have come to getting at the pion-nucleon dispersion relation was to imagine an invented particle (somewhat as Gell-Mann discussed) of zero mass weakly coupled to the nucleon in exactly the same way as the pion. Then for it, of zero mass, the dispersion relation is valid as for light. The scattering at finite mass can then be approximated by writing it as $\frac{Q^2}{\omega^2} a_0(\omega)$ where Q is the amplitude for zero mass scattering. This is because at low energy the coupling is proportional to Q so we correct for this factor, while for high energy $Q^2 = \omega^2$ and $a_0(\omega)$ must be nearly correct for very energetic pions are much like those of zero mass.

(One reason to do this was to try to determine some of the constants usually subtracted by assuming that pions are coupled pseudovectorially, so that as both Q and ω go to zero all scattering amplitudes must go to zero.)

W. Heitler. — How sensitive are the dispersion relations to the basic assumption of local commutativity ? Supposing this is somehow violated — perhaps one could study a specific model — would the dispersion relations be so much changed that the present agreement with the experiments would essentially be lost ? The question is whether or not we can learn anything about local commutativity from the fact that no disagreement with the experiments exists so far.

M.L. Goldberger. — It is very difficult to say what a breakdown on the commutator condition would imply unless one spells it out in detail. For example, saying that the commutator fails to

vanish everywhere outside the light cone requires an entire re-analysis of the problem since the usual representation of the S-matrix element would no longer be relativistically invariant. I don't know what the ultimate effect would be.

G. Källén. — Let me make a comment about Professor Heitler's question and enlarge a little upon Professor Goldberger's answer. It is quite correct that we cannot have the commutator *exactly* equal to zero outside a finite distance without having it equal to zero for all space-like separations. This appears to be a very strong statement but, really it is not so. There is nothing to forbid us to have a *small* but finite value of the commutator for large separations and something more violent happening for small separations. Experimentally that is essentially equivalent to having macroscopic but not microscopic commutativity.

Further as Professor Goldberger has already said only special matrix elements of the commutator enter at all in the dispersion relations and we could have over all local commutativity violated without this showing up at all in a particular dispersion relation.

Lastly, it should be said that one could also think about some new singularities out in the complex plane which would have only an infinitesimal influence on the real axis where cross-sections and so on are measured.

In summary, I believe that an important part of an honest answer to Professor Heitler's question is that one *can* have many violations of local commutativity which would not show up in dispersion relations at all.

G. Chew. — Is it reasonable to take the point of view that a derivation of the analyticity properties of the S-matrix is an acceptable a priori postulate ? The philosophical motivation might be one of maximum smoothness of scattering amplitudes as small changes are made in energies and angles. Some singularities are required by the unitarity condition, but no others should occur.

G. Wentzel. — Abandoning field theory altogether in favor of a dispersion relations scheme, seems to me similar in spirit to abandoning statistical mechanics in favor of phenomenological thermodynamics. We believe in statistical mechanics as a comprehensive theory although only in simple cases can one actually

calculate a partition function. If this calculation is technically too difficult, we may content ourselves with applying thermodynamics at the cost of feeding in more experimental data.

Still, nobody would want to do without statistical mechanics as a higher ranking discipline.

Similarly, I would hope that also field theory in one form or another, will retain its place as a superior discipline.

P.A.M. Dirac. — I think it would be worth while to consider a lack of commutativity in which the commutator of two field quantities at a distance r is of the order $e^{-r/a}$ where a is some universal length. Such a lack of commutativity is suggested by the classical theory of an electron in an electromagnetic field.

If one takes the Lorentz equations of motion, makes them precise by subtracting the infinities and then makes them reasonable by discarding the non-physical solutions (for which an electron not acted on by an external field can run away) one gets a theory without strict causality. In this theory if one takes an electron initially at rest and lets a pulse of electromagnetic radiation fall on it, the electron begins to accelerate and to emit scattered radiation before the pulse has reached it. The amount of this scattering is of the order $e^{-\Delta t/a}$, where Δt is the time interval and a is the classical radius of the electron.

I would like to ask whether this kind of non-causality or lack of commutativity would have a drastic effect on the dispersion relations.

M.L. Goldberger. — Reply to Heitler's question essentially covers this. One must re-analyse the precise problems in question.

W. Heitler. — I suppose the function $e^{-r/a}$ could also be replaced by any other function suitable for analytic purposes which decreases rapidly.

R.P. Feynman. — A term like $e^{-r/a}$ cannot really produce such terrible changes to the behaviour expected. For example, Dirac's preaccelerating electron does not change the theory of the index of refraction of matter for low frequencies (not too close to $1/a$), so it is in close agreement to ordinary dispersion results at low energies. There is a precursor wave, it is true, but (in work

done by Wheeler and myself) one finds that it rises also as $e^{-r/a}$. Thus if direct measurements of time smaller than of order "a" are assumed to be impossible (because for example no test object is smaller than a) no trouble, as far as I know, can be demonstrated. We could not find any arrangement of electrons so that this effect could be built up to make an effect any larger than a in time.

Thus if two electrons are near each other, hit by a wave, the first starts to accelerate and this drims the other but in strength only exponentially as $e^{-r/a}$. Thus an electron still further on feels this indirect effect from the second electron that is not stronger than the effect that the first electron makes directly. (Of course in classical theory if you can measure delicately enough and you can get waves of enormous intensity so you can notice times of pre-action larger than a by the factor of log. (signal sensitivity).

F.J. Dyson. — Dirac asked whether the non-causal interaction of an electron, behaving like exp. $(-r/a)$ at distance r, would have had consequences for the dispersion theory.

In this case, assuming the classical equation of motion of Dirac to hold, the scattering matrix has a pole very high up in the upper half-plane.

If one adopts the point of view that dispersion relations should hold strictly, it is a disaster. If one adopts the Chew philosophy that observable quantities are sensitive only to nearby singularities in the complex energy-plane, the appearance of such a very distant pole creates no difficulty.

L. Van Hove. — In the single dispersion relation analysis of nucleon-nucleon scattering I assume one includes a pole term for the deuteron. How sensitive is the comparison with experiment to this deuteron term ?

M.L. Goldberger. — If the one dimensional nucleon-nucleon dispersion relations are to be compared with experiment directly, the deuteron pole must be included and is very important. I wish to emphasize however that the computation of the deuteron binding energy and the residue of the pole is definitively within the dynamical dispersion scheme.

R.P. Feynman. — There are some points about the electro-

magnetic form factor that I do not know and would like to hear about. First, how is it possible to deduce that there is a *resonance* when you are working at a virtual energy of 5 or 6 pions. It is expected that there are contributions from such high energy virtual states in any case. What leads you to conclude : there must be a resonance in these states ?

My second question results from an impression or memory that the proton form factor has a wide distribution and that energies definitively lower than 100 Mev were required in order to describe it. I thought a good part spread out to a range of 2 or 3 pions.

M.L. Goldberger. — Historically the argument is as follows : the original computations of the contribution to nucleon structure from the 2-pion state neglected entirely the π - π interaction and were in addition essentially equivalent to lowest order perturbation theory. It was shown by Federbush, Goldberger and Treiman that about one-half of the contribution to the magnetic moment, for example, was eliminated on the basis of a rigorous unitarity argument. It was then shown by those authors that the inclusion of a π - π interaction improved agreement with experiment. Frazer and Fulco then showed that one required an actual resonance in the π - π system to get agreement with experiment.

From a study of both the proton and neutron structures one learns also that a three π " bound " state is required. The agreement with experiment is apparently not overwhelmingly in support of the assumption that everything is saturated by the two and three meson resonances.

S. Mandelstam. — I should like to emphasize that the order of magnitude of the disagreement between theory and experiment now is quite different from what it was without any resonances. Previously as Drell pointed out at end 1958 Rochester conference, the disagreement of the contribution of the outer cloud of the nucleon to the magnetic moment was by a factor of about five. This would necessitate, not only a tremendous contribution from the higher intermediate states, but a remarkable cancellation as well. Now with the existing resonances, one can get agreement to within something like 20% at the low momentum-transfer end. One has to abandon an attempt to explain the experimental core in the charge distribution but this may well be due to high intermediate states.

I would therefore say we have no evidence to predict any other possible resonances though, of course, if they exist, they could improve the agreement between theory and experiments.

E.P. Wigner. — It is difficult to know whether solutions of the equations of Quantum Electrodynamics exist and are unique. Is there a clean formulation of the axioms of dispersion relations and are there some proofs of their consistency ?

G. Chew. — The only attempt so far to formulate a clean set of S-matrix postulates is by Stapp. His postulates, however, are incomplete with respect to poles and certainly have not been shown to be self consistent. He has shown that they lead to TCP invariance and the connection between spin and statistics.

TWO-DIMENSIONAL REPRESENTATIONS OF SCATTERING AMPLITUDES AND THEIR APPLICATIONS

by **S. MANDELSTAM**

This report is divided into three sections. The first will be concerned with the foundations of two-dimensional representations and of analytic properties of transition amplitudes in general. The view will be taken that they are probably consequences of quantum field theory, though our mathematical tools are not yet sufficiently powerful to carry out the proof. In the second section an account will be given of the results so far obtained with the representations in conjunction with approximation schemes. Finally, recent proposals for overcoming some of the more serious difficulties of the present approximation scheme will be treated.

FOUNDATIONS OF DOUBLE-DISPERSION RELATIONS

Calculations in elementary-particle physics performed with the aid of the analytic properties of transition amplitudes, if successful, would be a means of implementing an approach to quantum field theory originally put forward in 1955 by Lehmann, Symanzik and Zimmermann [1] and developed by other physicists. The main departure of this approach from the conventional one is in the suggestion that it is unnecessary to specify the Lagrangian in a local field theory; the other postulates of the theory determine it to within a small number of coupling constants. Though this is in striking contrast to non-relativistic quantum mechanics, where a knowledge of the Lagrangian is necessary to define the problem, it has in fact been tacitly assumed in quantum field theory. For

instance, in the pion-nucleon system, the only relativistic Lagrangian which leads to a consistent theory is

$$\mathscr{L} = \mathscr{L}_0 + ig\bar{\psi}\gamma_5\psi\tau_i\varphi_i + \lambda\varphi_i^2\varphi_j^2,$$

where $\mathscr{L}_0$ is the Lagrangian for non-interacting fields, and g and λ are arbitrary constants. Any other local Lagrangian is unrenormalizable, i.e. it leads to infinities in observable quantities, and is therefore unacceptable. This conclusion has only been proved in any order of perturbation theory and is therefore not rigorous, but it is at least very plausible.

If one accepts the hypothesis that the Lagrangian is determined by the other postulates of quantum field theory, one should be able to reformulate the theory without using the Lagrangian at all. By doing so one would avoid having to mention infinite unrenormalized coupling constants and masses, since the only place where they occur is in the Lagrangian itself. It is then necessary to introduce directly the requirements of unitarity and causality, which were previously introduced by suitable restricting the Lagrangian — more precisely, by demanding that it be Hermitian and local. The unitarity equation is straightforward, though in applying it approximations have to be made since it involves an infinite number of intermediate states. The consequences of the causality condition are less simple to determine. We adopt the condition in the form $[\Phi_1(x_1),\Phi_2(x_2)] = O^2$, where Φ_1 and Φ_2 are any two operators and x_1 and x_2 are spacelike separated. To take this as the statement of the causality condition is physically reasonable, as the lack of commutativity of two operators is associated with the interference between measurements of the corresponding observables and, if the points are spacelike separated, such interference can obviously not occur without signals propagating faster than light.

The consequences of the causality condition usually take the form of dispersion relations such as Dr. Goldberger has described. They imply that the function satisfying them can be continued into the complex plane as an analytic function with cuts along the real axis only. Certain single-variable dispersion relations have been proved rigorously from the postulates of quantum field theory. However, scattering amplitudes are functions of two variables — the energy and the angle — and, in order to apply the dispersion relations to calculations, one requires the analytic properties when both variables

are allowed to become complex. The determination of the analytic properties in two variables which follow from the causality condition is a complicated problem which has not yet been solved. Further, the unitarity condition probably enables one to extend the results — this has been proved in certain cases — and, as this condition connects several transition amplitudes, the problem is complicated further. In the absence of a complete solution to the problem, one would have to postulate a plausible form of the result in order to perform calculations.

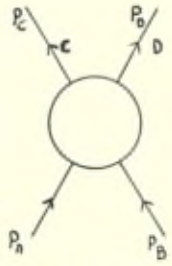


Fig 1

We shall confine our attentions for the most part to two-particle transition amplitudes. The kinematical variables are defined as follows

$$\begin{aligned} s &= -(p_A + p_B)^2 \\ t &= -(p_A + p_C)^2 \\ u &= -(p_A + p_D)^2 \\ s + t + u &= M_A^2 + M_B^2 + M_C^2 + M_D^2 . \end{aligned} \qquad (1)$$

The diagram in figure 1 represents three reactions

$$\begin{aligned} &A + B \to C + D \quad \text{I} \\ &A + \overline{D} \to \overline{B} + C \quad \text{II} \\ &A + \overline{C} \to \overline{B} + D \quad \text{III} \end{aligned}$$

In the reaction I, s is the square of the centre-of-mass energy, and t and u minus the square of the momentum transfer between the incoming and outgoing particles. Similarly, in the reaction II, u is the square of the energy and s and t the squares of the momentum transfers while, in the reaction III, t is the square of the energy, s and u the squares of the momentum transfers.

According to the single-variable dispersion relations, the transition amplitude A is an analytic function of s with t fixed and sufficiently small, except for a cut on the real axis. The simplest generalization to two variables is to suppose that A is analytic in both variables except for cuts on the double real axis. By applying Cauchy's theorem, one can then derive the following representation, on the assumption that A approaches zero as s or t approach infinity [3] :

$$A(s,t) = \frac{1}{\pi^2}\int ds'dt' \frac{A_{13}(s',t')}{(s'-s)(t'-t)} + \frac{1}{\pi^2}\int du'dt' \frac{A_{23}(u',t')}{(u'-u)(t'-t)} + \frac{1}{\pi^2}\int ds'du' \frac{A_{12}(s',u')}{(s'-s)(u'-u)}. \qquad (2)$$

If A does not approach zero as s or t approach infinity the representation has a more complicated form. The " double spectral functions " A_{13}, A_{23} and A_{12} are non-zero in regions of the form depicted in figure 2. The values of s', u' and t' at the asymptotes

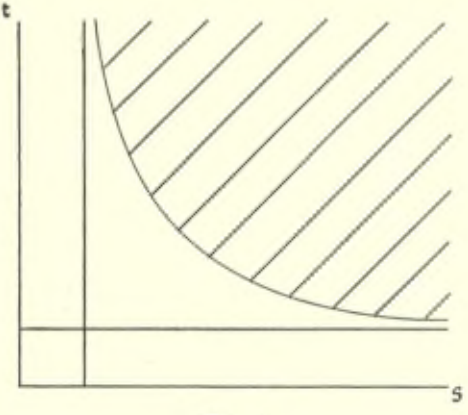


Fig 2

correspond to the energies of the lowest intermediate states in the reactions I, II and III. It frequently happens that the region is bounded by two different curves.

We shall not be able to go further into the elementary consequences of the double-dispersion relations in this report.

Thus far double dispersion relations have not even been proved in perturbation theory — proofs by Eden [4] and by Landshoff, Polkinghorne and Taylor [5] were subsequently found to have a

gap [6]. We should therefore stress that the validity of calculations which have been performed or suggested do not depend on the general validity of the relations, but only on a less restrictive conjecture. This conjecture, which may be termed " maximum analyticity ", requires that transition amplitudes be analytic functions of their variables, considered complex, everywhere except where singularities are forced on them by the unitarity condition. As it appears that this assumption enables the perturbation series to be constructed term by term, an alternative form of the conjecture would be that transition amplitudes are analytic except where singularities occur in one of the terms of the perturbation series. On the basis of this conjecture, general rules for finding the singularities of transition amplitudes have been given by Landau and Bjorken and have been amplified by Tarski [8] and Polkinghorne and Screaton [9].

The conjecture of maximum analyticity must be supplemented by another for practical calculations, since the determination of the singularities of all perturbation diagrams from the Landau rules is still a difficult problem. We therefore assume that singularities due to high-energy intermediate states in the unitarity condition occur at correspondingly large values of s or t. In the approximation scheme with which dispersion relations must be combined for practical calculations, distant singularities must in any case be neglected. We therefore have only to determine the singularities associated with low-energy intermediate states in the unitarity condition, and this is usually quite a tractable problem. For scattering amplitudes not involving strange particles, in the approximation where only two-particle intermediate states are included in the unitarity condition, the singularities turn out to be those given by the double-dispersion relation. However, the conjecture of maximum analyticity should also be applicable to production processes or to scattering processes in higher approximations. Since the unitarity equation has in any case to be used in the calculation, the determination of the singularities implied by it should not prove to be the most difficult part of the problem.

As we have stated, the question whether the conjecture of maximum analyticity implies the validity of the double-dispersion relations in general is still open; it is of interest but not of vital importance.

The point at issue is therefore whether the principle of maximum analyticity follows from the postulates of local quantum field theory (without the introduction of a specific Lagrangian). In the absence of proof one cannot of course give a definite answer, but we may mention two reasons for believing that the conjecture is at least plausible.

(i) It has been shown by Nishijima and others ([10,3]) that one can construct at least a subset of the perturbation series from the principles of field theory and the causality postulate. The solutions obtained from the principle of maximum analyticity can also be expanded in a perturbation series, and the terms obtained by the two methods agree — in fact they both agree with the renormalized perturbation series obtained from the conventional Lagrangian theory. If the postulate of quantum field theory and the principle of maximum analyticity predict the same perturbation series it seems likely that they lead to the same theory, even though the perturbation series in all probability diverges.

(ii) In certain cases and, in particular, for pion-pion scattering, one can actually prove that the principle of maximum analyticity holds within a limited domain of the two complex variables [11]. Furthermore, for " neutral scalar " pion-pion scattering, where a three-pion vertex is allowed, the size of this domain can be increased with the aid of the unitarity condition. This condition could only be used at energies below the threshold for production processes, since the analytic properties of the production amplitudes have not yet been studied, and it was this fact which limited the size of the rigorous domain of analyticity. Thus, if production processes could be taken into account, and there appears no reason in principle why they could not, one would hope to extend the domain further and, by taking sufficient processes, to extend it arbitrarily far.

One advantage of using dispersion relations in calculations is that the equations involve only S-matrix elements, whereas other methods of calculation also involve " off-shell " matrix elements. Some physicists have suggested retaining only the S-matrix in our theory and discarding the remainder of the field-theoretic framework, which they regard as an unnecessary encumbrance. One cannot then carry through the proofs of dispersion relations and analyticity properties, since the concepts on which they are based, and in particular the

concept of a local operator, are essentially field-theoretic and are not contained in an S-matrix theory. The principle of maximum analyticity is therefore a fundamental postulate in the new formalism. While not closing our minds to this or other new approaches, we should like to retain the field-theoretical basis of dispersion relations for several reasons. To begin with, the principle of maximum analyticity does not give a unique prescription for locating singularities if it is applied to the S-matrix instead of to the field-theoretic Green's functions. The ambiguity occurs in the case of the so-called " anomalous thresholds " when one cannot determine whether or not they are present by examining the S-matrix alone. The difficulty is essentially connected with the fact that there is not always a unique prescription for using the unitarity condition in the unphysical region where some of the momenta are imaginary. On a more theoretical plane, another argument is that the possibility of analytically continuing a function into a certain region is a very mathematical notion, and to adopt it as a fundamental postulate rather than a derived theorem appears to us to be rather artificial. The concept of a local field operators, though it may well have to be modified or abandoned in the future, seems more physical. Finally, it is not at all evident to us that all physics is contained in the S-matrix. No convincing arguments have been given that our ordinary macroscopic measurements are limiting cases of S-matrix elements. For this reason it is preferable to have a theory which involves concepts more general than that of the S-matrix.

APPLICATIONS OF DOUBLE-DISPERSION RELATIONS

In principle, calculations of transition amplitudes using double-dispersion relations divide themselves into two parts :

(i) The determination of the double spectral functions,

(ii) The determination of the low angular-momentum states. As we have written the representation, the entire scattering amplitude appears to be determined by the double-spectral functions. However, we made simplifying assumptions about the asymptotic behaviour, and actually the S-waves (for spinless particles) are not determined by the double spectral functions, but contain additional information. In the calculations that have been performed up till

now the double spectral functions are severely approximated and any partial wave with appreciable scattering, in particular, any partial wave with a resonance, even if it is theoretically obtainable from the double spectral functions, must be determined by a separate calculation. In the final section of the report we shall discuss suggestions for calculations without this feature.

Most recent calculations have been concerned with the low-angular-momentum waves, usually with S-waves or P-waves. From the double-dispersion representation, it can be shown that the partial-wave amplitudes satisfy dispersion relations. For simplicity we shall take the case of pion-pion scattering, so that the dispersion relation takes the form

$$A^{(l)I}(s) = \frac{1}{\pi}\int_{4\mu^2}^{\infty} ds' \frac{\operatorname{Im} A^{(l)I}(s')}{s' - s} + \frac{1}{\pi}\int_{-\infty}^{'o} ds' \frac{\operatorname{Im} A^{(l)I}(s')}{s' - s}. \quad (3)$$

The superscript (l) represents the angular-momentum and I the isotopic spin.

One requires next equations for the imaginary part of $A^{(l)I}$ which appear on the right of Eq. (3). The imaginary parts of A for positive s, which appears in the first term, is given by the unitarity equation :

$$\operatorname{Im} A^{(l)I}(s) = \frac{1}{16\pi}\sqrt{\frac{s - 4\pi^2}{s}} \sum_i \left| A^{(l)I}(s) \right|^2. \quad (4)$$

The subscript i indicates a transition amplitude between the initial two-pion state and any intermediate state with the same energy and quantum numbers, the summation being over all intermediate states. To obtain the imaginary part of A for negative s, which appears in the second term of Eq. (3) we make use of the fact that figure 1 represents the amplitude for the three reactions $A + B \to C + D$, $A + D \to B + C$ and $A + C \to B + D$. From this it can be shown that the imaginary part of the transition amplitude for one of the reactions at negative s can be obtained in terms of the imaginary

part of the amplitude for the other two reactions at positive s. For pion-pion scattering, the equation is

$$\operatorname{Im} A^{(l)I}(s) = \frac{1}{s-4\pi^2} \sum_{I'} \alpha_{II'} \int_{-s+4\mu^2}^{\infty} ds' \quad \operatorname{Im} A^{I'}(s', s)$$

$$\times P_l\left(1 + \frac{2s'}{s-4\pi^2}\right). \qquad (s<0) \qquad (5)$$

The factor $\alpha_{II'}$ is a known isotopic-spin matrix. Note that, in the factor $A(s', s)$, the first variable s' is the energy and the second s the momentum transfer and, according to the limits on the integral, s' is positive as is required.

The momentum transfer s in the factor $A(s', s)$ corresponds to an unphysical angle whose cosine is not between ± 1, and we therefore have to be careful when using a partial-wave expansion. We can always obtain A from the double-dispersion relation, however, and Eq. (5) can then be written in the form

$$\operatorname{Im} A^{(l)I}(s) = \frac{1}{s-4\mu^2} \sum \alpha_{II'} \int_{-s+4\mu^2}^{\infty} ds' \Big\{ \operatorname{Im} A^{(0)I'}(s')$$

$$+ 3 \operatorname{Im} A^{(1)}(s') P_1\left(1 + \frac{2s}{s'-4\mu^2}\right) + ds'' A_{13}^{I'}(s', s'')$$

$$\times \left[\frac{1}{s-s''} - Q_0\left(1 + \frac{2s''}{s'-4\mu^2}\right)\right.$$

$$\left. - P_1\left(1 + \frac{2s}{s'-4\mu^2}\right) Q_1\left(1 + \frac{2s''}{s'-4\mu^2}\right)\right]\Big\}$$

$$\times P_l\left(1 + \frac{2s}{s'-4\mu^2}\right) \qquad (6)$$

The first two terms in the curly bracket in Eq. (6) represent the contributions of the S- and P-waves to $A^I(s', s)$, while the remainder is an integral over the double-spectral function from which the S- and P-wave contributions have been substracted out. Formally there is no reason why we should subtract out just the S- and P-waves from the integral and treat them explicitly. As we are going to approximate the double spectral functions, however, we must treat explicitly the waves where we expect appreciable scattering.

One feature of Eq. (6) is that, though the values of l and I on the left are fixed, all values of l' and I′ appear on the right. If we were dealing with a reaction other than pion-pion scattering, the terms on the right of Eq. (6) would refer to the two " crossed " reactions given by the same diagram. For pion-pion scattering the three reactions are identical apart from changes of charge.

As long as the double-dispersion representation is correct, equations (3), (4) and (6) are exact. One now approximates them by assuming that, if the amplitudes are required at low values of s, the dispersion integrals will be dominated by fairly low values of s', so that equations can be used which become inaccurate at high s'. The approximation is made at two points. In the summation (4) only pion-pion states i are included. The equation thus becomes inaccurate when production processes are important but, on the other hand, it now involves only the pion-pion scattering amplitude on the right as well as on the left. The second place where one makes an approximation is in the integral over $A^I_{13}(s', s'')$ occurring in Eq. (6). One uses equations, derived from the unitarity condition, for the double-spectral function A^I_{13}, which are exact if s' and s'' are both between $4\mu^2$ and $16\mu^2$ (the production threshold) but which begin to break down above this limit. With these approximations, the system of equations (3), (4) and (6) can be solved for the low partial-wave amplitudes. Similar equations, can be derived for other scattering processes, and the equations for the three reactions of figure 1 are always coupled.

For both pion-pion and pion-nucleon scattering it turns out that, in the lowest approximation, the s''-integral in Eq. (6) vanishes. The double spectral function is zero when both s' and s'' are between $4\mu^2$ and $16\mu^2$. The reason for this is connected with the fact that there is no three-pion vertex, and that certain processes are therefore forbidden. For nucleon-nucleon scattering, and for all processes in higher approximations, the s''-integral in Eq. (6) cannot be neglected. The right-hand side of Eq. (6) without the s''-integral looks like the first terms of a partial-wave expansion, and it has been interpreted as such by some physicists. We wish to point out that this is not the case — subsequent stages in the approximation scheme, and even the lowest stage for nucleon-nucleon scattering, contain contributions from all partial waves.

In the lowest approximation to scattering problems (apart from nucleon-nucleon scattering), then, the double dispersion relation is not used explicitly. It is appealed to in deriving dispersion relations for fixed partial waves and also in justifying the expression used on the right of Eq. (6).

Probably the most significant result obtained to date from dynamical calculations with dispersion relations is in connection with the analysis of nucleon electromagnetic structure. The process is essentially $\gamma \to N + \bar{N}$. The unitarity equation has the form

$$\text{Im } A_{\gamma \to N + \bar{N}} = k A^{*}_{\gamma \to 2\pi} A_{2\pi \to N + \bar{N}}, \qquad (7)$$

on taking into account only the lowest intermediate state, the two-pion state. For simplicity spin has been omitted, and k is a kinematic factor. The equations for the reaction $2\pi \to N + \bar{N}$ couple with those for the crossed reaction $\pi + N \to \pi + N$, so that a knowledge of pion-nucleon scattering is required. Further, the unitarity equation for the reaction $2\pi \to N + \bar{N}$ is

$$\text{Im } A_{2\pi \to N + \bar{N}} = k A^{*}_{2\pi \to 2\pi} A_{2\pi \to N + \bar{N}} \qquad (8)$$

and a knowledge of pion-pion scattering is also required. It turns out that, once the amplitudes for pion-nucleon scattering and pion-pion scattering are known, the integral equations for the electromagnetic-structure problem can be solved analytically.

Initial treatments of the problem assumed no pion-pion interaction and failed hopelessly to agree with experiment [12,13]. The contributions of the outer cloud of the nucleon to the charge and magnetic-moment from factors were underestimated by a factor of about five [14]. Chew then suggested that a resonance in the P-state of pion-pion scattering might bring the results into agreement with experiment, and this was shown to be the case by Frazer and Fulco [15]. They originally proposed a resonance energy of 3 - 3.5 pion masses, but later work showed that this would give pion-nucleon cross-sections in disagreement with experiment, and that an energy of 4.5 - 5 pion masses would be better.

Though the existence of this resonance was not obtained from a purely theoretical calculation, it did come from a theoretical analysis of electromagnetic structure. If we adopt the somewhat heuristic definition of a dynamical calculation, as opposed to a

phenomenological analysis, as one which was widely disbelieved before direct experimental evidence was available, it was certainly a dynamical calculation. It is therefore encouraging that $\pi - \pi$ Q-value measurements in the reaction $\pi + N \to 2\pi + N$ provide strong direct evidence in favour of a resonance at about 5 pion masses [16-18].

Calculations of pion-pion scattering based on dispersion theory have been performed [19-21]. They show that two classes of solutions exist, one of which has a resonance in the P-wave. (The fact that there are two classes is a consequence of the way the renormalized coupling constant was defined.) Unfortunately it was found that, in the resonant solution, the integral equation became singular, so that a cut-off had to be introduced. An extra parameter had thus to be brought in, and the position of the resonance could be adjusted. When this was done the width turned out to be rather larger than that required by the electromagnetic-structure calculations.

Pion-nucleon scattering has also been treated by dispersion relations [22,23]. The 3 - 3 resonance is predicted, but comes at rather too low an energy, just above threshold, in fact. In view of the approximations made this is not too surprising, and a more accurate calculation is desirable. The small P-waves are now in much better agreement with experiment than they were in calculations which neglected pion-pion scattering, though recent relativistic analysis suggests that the effect of the pion-pion scattering is rather less than had been thought.

Nucleon-nucleon scattering has also been treated [24,25] and preliminary results have been found. However, the uncertainty about the pion-nucleon scattering amplitude is hindering further progress.

A number of other processes, some involving strange particles, have been treated with double-dispersion relations.

SUGGESTIONS FOR FURTHER CALCULATIONS

The most unpleasant feature of the calculations reported in the previous section was the necessity of having to introduce a cut-off in certain cases. With pion-pion scattering, for instance, the func-

tion Im A (s) given by Eq. (6) increases rapidly enough as s approaches infinity to make the integral equation singular. The term causing the difficulty is the second in the curly bracket, associated with the P-wave, where the first Legendre polynomial $P_1\left(1+\frac{2s}{s'-4\mu^2}\right)$ becomes infinite with s.

It is therefore probable that the necessity for the cut-off arises from the separation of the P-wave component on the right of Eq. (6). If we knew the double spectral function with sufficient accuracy, and if it became infinite less strongly than s'' as s'' became infinite, we could rewrite the curly bracket as

$$\int ds' \left\{ \text{Im } A^{(o)1'}(s') + \int ds'' A_{13}^{1'}(s', s'') \left[\frac{1}{s-s''} - Q_o\left(1+\frac{2s''}{s'-4\mu^2}\right)\right]\right\}. \tag{9}$$

In Eq. (9) we have split off only the S-wave from the integral over the double spectral function. With this expression inserted on the right of Eq. (6), the integral equation would no longer be singular and a solution could be obtained without a cut-off. From an examination of the potential-theory problem it appears unlikely that A_{13} does tend to infinity less rapidly than s; however, the function may contain oscillations which have the required effect.

The potential-theory problem therefore leads us to believe that the singular nature of our integral equations may be due to the approximations made and not to an inconsistency in the theory. One must not of course take results derived from potential theory too seriously, but they indicate at least that the singular nature of the integral equations does not *imply* the inconsistency of the theory.

On the assumption that the necessity for a cut-off is due to the approximations, we are left with the problem of finding a practical method to avoid the cut-off. One possibility is simply to go to higher approximations, in which parts of the double spectral functions on the right of Eq. (6) are calculated. One will probably not decrease of degree of divergence by going a finite number of stages, but the cut-off would be expected to move further away, as our hypothesis is that it is absent in the complete theory. The results will therefore become less and less sensitive to the position of the cut-off. In fact, a cut-off is probably necessary only if Im A (s) is positive at large negative values of s. This is physically reasonable,

as a singularity of A (s) at infinite s corresponds to a singularity of the potential at the origin in co-ordinate space, and such a singularity only gives difficulties if its sign corresponds to an attraction. Thus, if Im A (s) tended to $-\infty$ instead of to ∞ at a stage of the approximation scheme, results could be obtained without a cut-off.

A much more ambitious procedure for avoiding the cut-off has been proposed independently by Chew and Frautschi [26], Wilson [27] and McCauley. To understand the philosophy of their approach, let us examine the following kinematical diagram, in which s has been plotted against t in a triangular co-ordinate system to emphasize the symmetry between s, t and u. The regions P are the physical

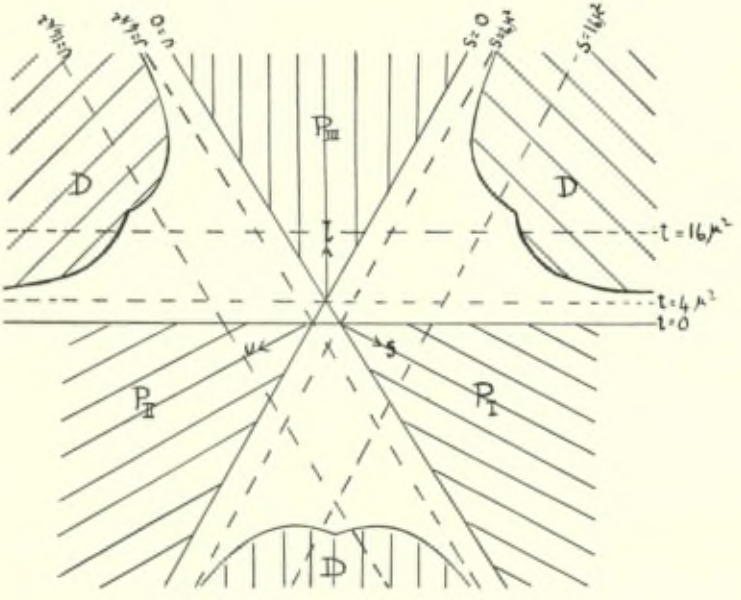
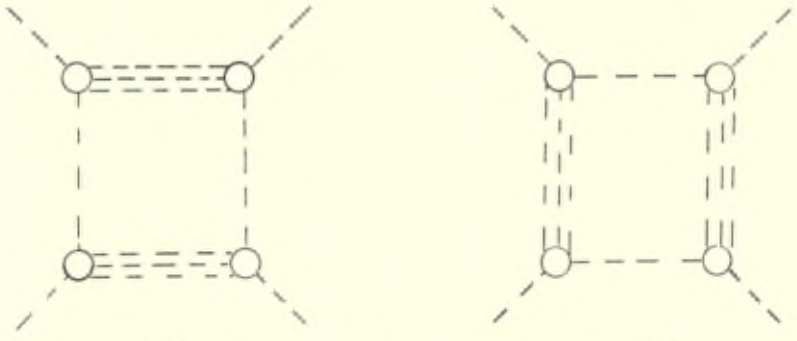


Fig. 3

regions for the three reactions, and the regions D are those in which the double spectral functions are non-zero. The authors point out that, from the unitarity for the three reactions with multi-particle states neglected, one can obtain equations for the double spectral functions which are exact in the strips $4\mu^2<s<16\mu^2$, $4\mu^2<t<16\mu^2$, $4\mu^2<u<16\mu^2$, and in a certain area beyond the strips [28,29]. The equations take into account processes such as those shown in

figure 4 and corresponding processes with A and B, or A and C, interchanged. The two diagrams are obtained by considering two-



(a) Fig 4 (b)

particle intermediate states in the unitarity equation for the reactions $A + B \to C + D$ and $A + C \to B + D$ respectively. From the point of view of the reaction $A + B \to C + D$, figure 4(b) considers multi-particle intermediate states, but the relevant production amplitudes are taken in the " peripheral " approximation where only one-pion exchange is included. Thus the approximation takes elastic processes into account completely, but it only treats inelastic processes in the peripheral approximation.

Arguing from the potential-theory analogy, Chew and Frautschi hope to reproduce the oscillatory behaviour of the double spectral function and thus to avoid the necessity for a cut-off. All waves except the S-waves would be determined from the double spectral functions, the S-waves requiring a separate calculation. The carrying out of the procedure is by no means simple. Even in the absence of any difficulties from the asymptotic region the equations are fairly complicated and will require an extensive computing programme. The asymptotic behaviour complicates matters further, as we are dealing with functions which oscillate with increasing amplitude and frequency in such a way that certain integrals taken over then remain finite. Cancellations must therefore be taking place, In the potential-theory problem we know that these cancellations occur, but the mechanism which brings them about remains somewhat of a mystery. Until further light is shed on this point one cannot know whether the cancellations occur in the Chew-

Frautschi approximation. If they do, one still has to perform the numerical calculation in such a way that the cancellations are not masked by numerical errors, such as those due to finite mesh size, in the asymptotic region.

Chew and Frautschi also hope that their calculations will be reasonably accurate for high energies and low momentum transfers in the region of the diffraction peak. They base their argument on the rapid fall-off of the cross-section as we go from the direction of forward scattering ($t = 0$, say) into the physical region. They would therefore expect the contribution of the strip $4\mu^2 < t < 16\mu^2$ to dominate the dispersion integral. However, the situation may well be more complicated, as the diffraction peak is probably a consequence of unitarity, and the strip approximation does not incorporate unitarity in production processes. It therefore may happen that contributions to the integral from values of t greater than $16\mu^2$ are also important, and that, because of unitarity, these contributions build up within the diffraction peak and cancel outside it. It may even happen that the Chew-Frautschi approximation gives inelastic cross-sections which exceed the unitarity limit.

Whether or not one accepts the arguments of Chew and Frautschi for the accuracy of their approximation, the scheme does possibly avoid some of the formal disadvantages of the present approximation scheme and, if they can overcome the difficulties alluded to above, their progress will be watched with great interest.

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Discussion du rapport de Mandelstam

G.C. Wick. — I wish to make two remarks concerning the resonances recently observed in the 2 - π and 3 - π systems. The first remark concerns the agreement between the measured value of the mass of the di-pion and the values required by the theoretical calculations. The fact that the agreement is only very crude may, of course, be due to the fact that the calculations themselves are quite crude; a much better agreement was in fact not expected. It is interesting to point out, however, that also the experimental value of the resonant mass is subject to large uncertainties. There are two ways to obtain the mass from the observed data; one is the extrapolation procedure of Chew and Low; this is a most ingenious idea, but as you know in practice it is beset with considerable difficulties. The values that are quoted are obtained by a much less sophisticated application of the one-pole approximation slightly off the mass-shell. This means that the measured mass, specially in the case of the rather wide di-pion resonance, is subject to unknown and possibly large corrections due to final state interactions, form factors, etc.

The second remark I wish to make, is to point out the really essential role that the dispersion-theoretical calculations have played in the discovery of these particles. Without the direct hint from the calculations which Prof. Mandelstam has quoted, it is unlikely that the very involved and painstaking analysis of these complicated reactions would have been undertaken at this time. The discovery of the resonances would in all probability have been delayed by many months or even years.

S. Mandelstam. — I agree with what Wick said about experimental uncertainties. As, however, the agreement between the present experimental and theoretical values is as good as can be expected, I do not think it is reasonable to hope for better agreement when the experiments are improved or if $\pi\pi$ scattering is measured by less uncertain methods.

R.P. Feynman. — In analysing the electromagnetic structure of the nucleon you were able to deduce that 1 or 3 pions formed a resonance at energy near $5m_{\pi}$. How did you distinguish the effect that one would expect from several pions even if they did not interact from that of a resonance ? In short, how did you conclude " resonance " was necessary ? Was there any estimate of its maximum width, for clearly a resonance of width $5m_{\pi}$ would be no resonance at all.

G. Chew. — The prediction of the 2π resonance rested on a theoretical calculation of the *magnitude* as well as the extension in space of the nucleon vector anomalous magnetic moment.

To get the observed large moment from the 2π configuration, without a resonance to enhance the probability of this state, seemed impossible. No quantitative calculation has been made of the isoscalar 3π configuration, but since experimentally the isoscalar charge distribution is approximately the same as the isovector, it seemed plausible that a 3π resonance would have to occur at roughly the same energy as for the 2π.

C. Møller. — Is it possible at the present moment on the basis of your definition, to tell which of the known particles are elementary ?

S. Mandelstam. — I was not proposing the criterion for elementary particles to be used experimentally, but in calculations, when putting in the information about the number of elementary particles present. In practice, we would consider a particle non-elementary if its existence can be predicted by calculation. Thus we consider nuclei (other than hydrogen) to be non-elementary. The same applies to the 3 - 3 resonance, whose existence came out of calculations performed from 1943 onwards and which was predicted from the present theory by Frautschi and Walecka, even though we do not have a quantitative determination of its position.

M. Cini. — May I state my point of view concerning the question of the " prediction " of π - π resonances. The point is that, apart from the strip approximation which is being investigated at present by Chew and Frautschi but has not given predictions about resonances up to now, the simplest way to use the double representation

is to approximate it with a sum of one-dimensional dispersion integrals. This procedure corresponds to keep only the lowest angular momentum states in the three channels and leads to equations for the lowest partial wave amplitude, which, as stressed by Mandelstam, are not really equations unless you introduce artificial cut-offs. In these equations one has integrals over the absorptive part of partial wave amplitudes for the physical processes described by the three channels.

It is reasonable therefore to keep in these integrals only the contribution of those phase shifts which have a resonance and in this sense one finds that unless one introduces a π - π resonance in the $T = 1$, $J = 1$ state there is no way of explaining the isovector part of the electromagnetic form factors of nucleons. The parameters introduced to get the fit in this case give reasonable predictions for quite a number of other phenomena in low energy pion physics such as π - n scattering, π photoproduction and n - n scattering. In the latter case for instance, one gets, in addition to the spin-orbit force, the change of sign of the $^1S_0$ phase shift at high energies.

L. Van Hove. — In the discussion of the nuclear form factor one heavily relies on the " nearest-by-singularity " approximation. Now that the $T = 1$, $J = 1$ resonance of two pions appears to be near 5 pion masses one must seriously worry about the 4-pion contribution, at least by introducing one additional constant. Has this been done ?

M. Cini. — Yes, one introduces constant parameters in addition to the resonances in order to take approximately into account the higher mass states.

G. Wentzel. — How do the experts react to Richard Eden's findings in his perturbational analysis of double dispersion relations?

S. Mandelstam. — The work done by Eden on the general validity of the double-dispersion representation in perturbation theory is of interest, but the proposed applications do not depend on it. The important thing is that all singularities given by perturbation theory and that singularities due to high intermediate states should be far away. This latter requirement has been satisfied by

those singularities found by Eden and his co-workers for scattering of particles of certain masses which violate the double dispersion representation. In lowest order (taking only two-particles intermediate states), the relation is true for processes involving pions and nucleons. If the singularities in higher order turn out to be complex, this in itself should not make them more difficult to handle.

J.R. Oppenheimer. — I remember that in work on the π-nucleon systems Goldberger and his colleagues found that one could introduce either the meson mass and residue or the deuteron mass and residue whereas Mandelstam finds trouble with the latter. Is that because you did not look at high energies in the crossed channel ?

M.L. Goldberger. — Perhaps.

M. Gell-Mann. — The way to realize the difference is to use Chew and Goldberger approach. Maybe Chew could give his point of view on the subject ?

G. Chew. — The notion, inherent in conventional Lagrangian field theory, that certain particles are fundamental while others are complex, is becoming less and less palatable for baryons and mesons as the number of candidates for elementary status continues to increase. Sakata has proposed that only the neutron, proton and Λ are elementary, but this choice is rather arbitrary and strong interaction consequences of the Sakata model merely reflect the established symmetries. Heisenberg some years ago proposed an underlying spinor field that corresponds to no particular particle but which is supposed to generate all the observed particles on an equivalent basis. The spirit of this approach satisfies Feynman's criterion that the correct theory should not allow a decision as to which particles are elementary, but it has proved difficult to find a convincing mathematical framework in which to fit the fundamental spinor field. On the other hand, the analytically continued S-matrix — with only those singularities required by unitarity — has progressively over the past half decade appeared more and more promising as a basis for describing the strongly interacting particles. I should like to propose a formulation of the Feynman principle within the S-matrix framework.

Particles appear as energy poles of the S-matrix, on the physical sheet if stable and if unstable on an unphysical sheet. If one analyzes partial-wave *elastic* scattering amplitudes it should be possible to distinguish between poles that correspond primarily to bound states or dynamical resonances of the two-body system in question and CDD poles that correspond to particles " independent " of the two-body system. However, all such CDD poles need not necessarily correspond to elementary particles. For example, the Dalitz-Tuan model of the Y* describes this particle as a K - N bound state, but with respect to the partial-wave elastic π - Λ amplitude such a model would have to be considered as a CDD pole. Evidently one needs a criterion that emphasizes no particular configuration of particles; the criterion we propose below rests on the analytic structure of the S-matrix regarded as a simultaneous function of angular momentum and energy, quantities which are meaningful for arbitrary particle combinations.

Regge has shown for elastic potential scattering and Froissart for any amplitude satisfying the Mandelstam representation that the S-matrix can be simultaneously continued into the complex energy and angular momentum planes; for scattering by a superposition of Yukawa potentials, all poles are associated with bound states and resonances and may be viewed either in the E plane for fixed J or in the J plane for fixed E. A corollary for the latter viewpoint is that the position, α_i, of a particular pole in the J plane is an analytic function of E, and $\alpha_i(E)$ = constant turns out not to be allowed. If at some energy the value of $Re\alpha_i(E)$ passes through a positive integer or zero (with $dRe\alpha_i/dE>0$) one has here a physical resonance or bound state for J equal to this integer, so in general the trajectory of a single pole in the J plane as E changes corresponds to a family of " particles " — some stable and some unstable — of different J and different mass. It is possible for the trajectory of a particular pole to cross only the integer 0, but the failure to reach higher physical J values would in such case be a dynamical circumstance and would not reflect a special role for J = 0. It seems intuitively clear, therefore, that any such pole appearing in the union of the complex J and E planes of the full (relativistic) S-matrix cannot be associated with the usual notion of elementary particle — which emphasizes a particular value of J. We may satisfy Feynman's principle therefore by postulating that *all* poles of the S-matrix are of this type (Regge poles).

You may wonder how *anything* but Regge poles can occur if simultaneous continuation to complex J and E is possible. The point is that for certain internal quantum numbers the relativistic continuation in J may be restricted to a region somewhat smaller than that for the non relativistic case (where the limitation is $ReJ > -1/2$). In particular, the region $ReJ < J_{min}^{(E)}$, $J_{min}^{(E)} > 0$ might be excluded. Following arguments given by Froissart on the basis of unitarity and analyticity in linear momenta, one can show that there are some E for which $J_{min} < 1$, but " elementary particle " energy poles, admitting no continuation in J, could be associated with the unique angular momenta $J = 0$ or $J = 1/2$. Such a conclusion coincides with renormalizability requirements of conventional field theory, so if $J = 1/2$ and $J = 0$ elementary particle poles actually occur in nature it may be argued that working directly with the S-matrix is simply a technique for evaluating conventional field theory. On the other hand, if all baryon and meson poles admit continuation in J plane, then conventional field theory for strong interactions is not only unnecessary but grossly misleading and perhaps even wrong.

How is one to distinguish experimentally between Regge poles and elementary particles poles ? An essential characteristic of a Regge pole is that it moves in the J plane as a function of E, the trajectory being the same — regardless of multiplicity — for all S-matrix elements having the internal quantum numbers of the pole. Experiments to establish this trajectory will be of two types, depending on the value of $s = E^2$. For $s > 0$ one will seek to identify the existence of families of particles. Blankenbecler and Goldberger, for example, have mentioned the possibility that the nucleon is only the $J = 1/2$ member of a family that may have unstable higher J member ($J = 5/2$, $9/2$, etc.) to be found among the resonances of multiparticles systems with the same baryon number, isotopic spin, etc. as the nucleon.

One will seek to show that the angular momentum of such particles is a monotonic function of their masses, but since only discrete J values can be observed and the total number of family members is not necessarily large (there may be only one physical value crossed by the trajectory) one may confidently anticipate situations where the Regge character of a pole is not convincingly established by experiments with $s > 0$.

For $s < 0$, on the other hand if the qualitative arguments about "strips" in the Mandelstam diagram presented by Frautschi and me can be taken seriously, then one should be able to study the trajectory $\alpha_t(s)$ in a continuous sense within the strip $s_{min}^t < s < 0$, where s_{min}^t is defined by $\alpha_t(s_{min}^t) = J_{min}^{(s_{min}^t)}$. Here one is working experimentally in a "crossed" reaction where s is the negative square of a momentum transfer. It may turn out for some poles that s_{min}^t is greater than zero, so that the strip in question does not exist, but the essence of our argument is that there should be important situations where the trajectory of the Regge pole is still inside the region of analyticity $ReJ > J_{min}$ for a range of negative s. Consider, for instance, the possibility that the recently discovered ρ meson is associated with a Regge pole whose internal quantum numbers are those of an $I = 1$ two-pion configuration. Then we know experimentally one point on the curve $Re\alpha_\rho(s)$, namely $Re\alpha_\rho(28m_\pi^2) = 1$, since the spin of the ρ meson is 1 and its mass $5.3m_\pi$. By analogy with potential theory, dRe_ρ/ds is probably positive for energies below this resonance, but it seems likely for these quantum numbers that $J_{min} \leqslant 0$, so there is a chance that at zero or even slightly negative s the value of α_ρ still is larger than J_{min}. ($\alpha(s)$ is real for real s below the lowest two-particle threshold, which here occurs at $s = 4m_\pi^2$.) In such a circumstance the Regge pole should dominate the high energy behavior of the crossed channel near the forward (or backward) direction where $|S|$ is small. In particular, in a two-body reaction, such as neutron-proton exchange-scattering with center-of-mass energy $\sqrt{t}$, the amplitude will have an energy dependence $\sim t^{\alpha(s)}$. Thus it may be possible by studying the asymptotic energy variation of the backward peak in n - p scattering to trace out a portion of the trajectory of the Regge pole associated with the ρ meson.

One might expect the pole associated with the π, whether Regge-like or elementary, to contribute to the backward n - p peak. Since the spin of the π is zero, however, such a pole would probably lead to a lower power of t than that associated with the ρ. The ω has spin 1 but because its isotopic spin is zero it will not appear in backward n - p scattering.

Of course, we already know from its spin that the ρ meson cannot be an elementary particle in the conventional sense and that it almost certainly is associated with a Regge pole. The interesting

cases will be those where the spin is equal to 0 or 1/2. Here the effect on backward peaks of crossed reactions may be difficult to find experimentally (because J is smaller) but a detailed theoretical analysis of the various possibilities seems well worthwhile. According to Frautschi and me all high energy *forward* (diffraction) peaks may be associated with a Regge pole having the same internal quantum numbers as the vacuum but with no particle manifestations if $Re\alpha_o(s) < 2$ for all s. For this one set of internal quantum numbers it appears necessary that $J_{min} \geqslant 0$, so that the $J = 0$ state here is not related to higher J values by analytic continuation. An exceptional status for the quantum numbers of the vacuum is perhaps not surprising.

W. Heisenberg. — I may have completely misunderstood your distinction between CDD poles (" real " elementary particles) and ordinary poles (corresponding to any particles like deuterons, etc.). But would the following picture be compatible with your mathematical description ?

In orthodox field theory, we would be inclined to picture an elementary particle as a thing having a δ-function in the center and some dressing around it, while a compound particle would have no δ-function in the center. Would this δ-function in the center be the cause of what you call a *CDD-pole* ? If this should be correct I would certainly prefer to think that such particles with a δ-function in the center and therefore CDD-poles do not exist. I would further like to believe that a theory with indefinite metric which avoids the δ-function on the light cone of the commutator will thereby also avoid the δ-function in the center of a particle and consequently perhaps the CDD poles ? Would you agree ?

G. Chew. — I am afraid that I do not understand the indefinite metric well enough to have an opinion.

EXTENSIONS AND MODIFICATIONS OF QUANTUM FIELD THEORY

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1. Quantum field theory as a relativistic extension of quantum mechanics has proved to be successful in explaining to some extent, at least, various phenomena relating to elementary particles. However, it is also true that there are unsatisfactory features in the present form of quantum field theory

We have been worrying for many years about the divergency inherent in relativistic field theory. Since unexpected discoveries of strange particles around 1950, we have begun to complain of the inability of the theory to predict new particles and new phenomena and of the lack of unifying principles. Thus, we have long been awaiting the emergence of a theory which is not only free from the logical and structural defects, but is also able to give cogent reasons for the existence and interaction of various types of particles. It is very hard to tell how far or how close we are now from such a theory. What seems to me most troublesome is the lack of an undeniable contradiction between experiment and theory. On the one hand, real discrepancies between quantum electrodynamics and relevant experiments are not yet detected in spite of the fact that quantum electrodynamics can not be a completely isolated theory On the other hand, a comparison of a theory of strong interaction with experiments can not yet be sufficiently accurate. Fifty years ago Planck's quantum theory was there and was clearly contradictory to the whole body of classical theory.

This very contraction, however, proved to be a great motive power for further developments in theoretical physics. In contradistinction to it, we can not easily locate a contradiction today

Under these circumstances, opinions of us may differ widely from each other. What the present author can do would be nothing more than to report on some of the recent attempts which were chosen according to his inevitably biased point of view.

2. One of the decisive points in constructing a theory of elementary particles is whether we may or may not depart from the picture of a point particle. If the whole content of quantum theory of fields is completely represented by wave functions in Fock space, there is no departure from point particle picture. However, one can not exclude the possibility of extending or modifying quantum field theory in such a way that it contains something more than the assembly of point particles. Attempts were made with the aim to construct a theory of particles with their own internal structure. However, it turned out, among other things, that there would exist too many quantum mechanical states for internal motion, if the particles were assumed to be extended in Minkowski space. The number of internal states could be reduced by imposing supplementary conditions in addition to field equations.[1] These conditions are to be compatible with field equations, corresponding to an image of particles with deformable form factors. However, it is extremely difficult, in general, to find such conditions which are compatible with field equations.[2] This was one of the main obstacles in developing a nonlocal field theory much further.

Another way of avoiding the appearance of too many internal states is the introduction of Hilbert space with an indefinite metric.[3] However, once an indefinite metric is introduced, a clear-cut distinction between local and nonlocal theories is no longer possible. Namely, one could start from a local field in Hilbert space with an indefinite metric which is also a departure from the picture of point particles in the narrow sense, because we have to admit in some way or other the existence of abnormal particles which are associated with the strange concept of negative probability as introduced first by Dirac.[4] If one could succeed in eliminating such abnormal particles, the result would be equivalent to a field theory with a nonlocal interaction in Hilbert space with a definite

metric. Intuitively, one can interpret the result as an action at distance between normal particles due to intermediary particles, among which abnormal and possibly some of normal particles are included. As well known, quantum electrodynamics can be regarded as an example of such cases, Coulomb interaction between charged particles being the result of elimination of abnormal photons together with normal longitudinal photons.[5]

It should be noticed that there is a case in which we started from a field theory with a definite metric and end up with a theory with indefinite metric. In fact, it was shown by Källen and Pauli [6] that Lee model results in the appearance of a state with the negative norm, unless a cut-off factor is introduced in such a way that the renormalized coupling constant becomes smaller than the critical value of the coupling constant. It is not yet clear, however, whether the situation is the same or not in completely local and relativistic field theories.[7]

3. In view of all this, it is of great interest to consider various possible ways of extension or modification of present field theory by introducing Hilbert space with indefinite metric which will be referred to as pseudo-Hilbert space in the following

What we hope to achieve in such attempts is to construct a field theory which satisfies the following conditions, if it is ever possible :

1) Lorentz invariance
2) convergence
3) probabilistic interpretation
4) macroscopic causality

Now any Lorentz invariant theory in pseudo-Hilbert space presupposes the existence of a total energy-momentum four vector P_μ which reflects the homogeneity of Minkowski space, only difference from a theory in ordinary Hilbert space being the pseudo-hermiticity of P_μ instead of the hermiticity. A hermitian operator in ordinary Hilbert space can be diagonalized, in general, by a unitary transformation. It is not so, in general, for a pseudo-hermitian operator in pseudo-Hilbert space. A pseudo-hermitian operator H is defined as

$$H^* = \eta H \eta \tag{1}$$

and a pseudo-unitary operator U as

$$U^{-1} = \eta U^* \eta \tag{2}$$

where η is the metric operator of the pseudo-Hilbert space and H^*, U^* denote hermitian conjugates of H, U respectively.

Let us consider, for illustration, the simplest possible case of two-dimensional pseudo-Hilbert space with the metric operator

$$\eta = \begin{pmatrix} 1 & 0 \\ 0 & -1 \end{pmatrix} \tag{3}$$

It can be shown easily that there is a pseudo-unitary transformation U which satisfies the relation

$$U H U^{-1} = E \tag{4}$$

where E is a diagonal matrix, only when the relation

$$(H_{11} - H_{22})^2 + 4H_{12}H_{21} \geq 0 \tag{5}$$

is satisfied and the diagonal elements of E become real.

In this connection, it should be noticed that we could start from another metric operator such as

$$\eta' = \begin{pmatrix} 0 & 1 \\ 1 & 0 \end{pmatrix} \tag{3'}$$

Thus we obtain another representation of operators satisfying the same algebraic relations as when we started from the metric operator (3). These two representations are inequivalent to each other in the sense that η and η' are not connected by a pseudo-unitary transformation, although they could be connected by a unitary transformation.

We know that the structure of Hilbert space underlying field theory is different from that of a quantum mechanical system with a finite number of degrees of freedom, even when we do not take account of the indefinite metric. Namely, there can be great many inequivalent representations of a field theory as shown by van Hove and many others.[8] In the case of a theory in pseudo-Hilbert space, however, inequivalent representations appear, even when the number of dimensions of pseudo-Hilbert space is as small as 2.

The new situation such as this gives rise to the question whether all these inequivalent representations can be admitted from the physical point of view. Suppose that we have started from the metric operator η' as given by (3') instead of η as given by (3). The eigenvalues E_1, E_2 of a pseudo-hermitian operator H can be shown

to be complex conjugate to each other. Accordingly H can be diagonalized, only when the relation

$$(H_{11} - H_{22}) + 4H_{12}H_{21} \leq 0 \qquad (5')$$

is satisfied. Moreover, the conditions imposed on matrix elements of a pseudo-hermitian operator H differ from each other in the two cases. Namely, if we choose η as the metric operator, H_{11}, H_{22} are to be real, whereas iH_{12}, iH_{21} are to be complex conjugate to each other. However, if we choose η' as the metric operator, H_{11}, H_{22} are to be complex conjugate to each other, whereas H_{12}, H_{21} are to be real. Thus the two eigenvalues of H are to be complex conjugate to each other in this case.[9]

Although the above example appears to be much too simple to be compared with a field theory in pseudo-Hilbert space, we can infer from it that there would be many possibilities which are precluded in field theories in ordinary Hilbert space. In particular, the singular case of dipole states which plays an important role in Heisenberg's unified field theory,[10] lies just inbetween the above two cases.

4. Now let us consider the problem of convergence. Feynman, Pauli and Villars tried to achieve the regularization by making use of auxiliary fields which were associated with abnormal as well as normal particles with very large masses.[11] This kind of approach is usually regarded as a formal procedure for getting rid of divergences, because the unitarity of S-matrix would be violated, if we go to the very high energy region where the abnormal auxiliary particles can be produced by the collision of ordinary particles.

However, one can take a different point of view.[12] Namely, one may think of the possibility that the unitarity of S-matrix is not an absolute restriction which is imposed on the theory, but is applicable only to those collision processes in which the total proper energy of the system of colliding particles does not exceed the proper energy of the abnormal auxiliary particle with the smallest mass. A real trouble with such a realistic rather than formalistic point of view seems to be related to the instability of some of the auxiliary particles. In order that the auxiliary fields are effective in regularizing the singularities of the propagators for ordinary particles, auxiliary particles must interact with ordinary particles

as strongly as the latter interact with each other. Thus the auxiliary particles will, in general, become more and more unstable as their masses become larger and larger. This means that the mass spectrum of the whole system will have sharp peaks only for comparatively small values of mass, whereas it is smeared out for large values of mass, but the continuous distribution function is not positive definite for large values of mass, because of the presence of very unstable abnormal auxiliary particles. Such very unstable particles can hardly be called particles.

Thus the conclusions to which we reached seems to be very much different from the assumption which we accepted in the beginning. Namely, we started with auxiliary particles with definite masses in addition to ordinary particles, but we came to conclude that some, at least, of the masses of auxiliary particles are so much broadened that it contradicts the image of auxiliary particles which we had at the beginning. Therefore, it seems to be more reasonable to assume from the outset a mass spectrum which has sharp peaks only at comparatively small values of mass and to assume that the continuous distribution function is not positive definite for large values of mass. The form of the mass spectrum is to be determined in such a way as to guarrantee the self-consistency of the formalism as a whole. This is nothing but a program, according to which a field theory in pseudo-Hilbert space would be constructed. For the time being, it is a completely open question and we can not preclude the cases in which dipole states or even the complex eigenvalues as illustrated in the preceding section eventually come into the formalism.

5. Now, let us return to the point which was discussed already. That is the relation between indefinite metric and nonlocality. They are so intimately related to each other that it is hard to tell which one is more fundamental than the other. On the one hand, an apparent nonlocality could be understood in some cases as the consequence of indefinite metric as discussed in section 2, but on the other hand the following argument seems to be also plausible.

Suppose that a particle is created at a certain point $x\mu$ in Minkowski space and annihilated at another point $y\mu$. If the particle is not a simple point particle, but is extended in space somehow, the quantities which were calculated in local field theory are to be modified by some suitable averaging procedure for internal

variables. For instance, the propagator which is associated with the creation and annihilation of such a particle may have the form

$$\int \cdots \int \mathrm{D}(x\mu + c\mu - y\mu - d\mu)\, w(c\mu)\, w(d\mu)\, d^4c\mu\, d^4d\mu \qquad (6)$$

where D is the usual Green function and w is a function which is presumably determined by the structure of the particle in question. Suppose further that the function w is so chosen as to give (6) the form

$$\int_{-\infty}^{+\infty} \mathrm{D}[(x\mu - y\mu)^2 + a^2]\, p(a^2)\, da \qquad (7)$$

where $p(a)$ represents, roughly speaking, the weight function in radial direction. Such a case was discussed recently by Takano [13] as the generalization of an example given by Markov.[14] If we start from the D-function for the zero-mass particle, the expression (7) becomes

$$\mathrm{D}[(x\mu - y\mu)^2] - \int_0^{\infty} \Delta[(x\mu - y\mu)^2; \varkappa^2]\, \mathrm{G}(\varkappa)\, d\varkappa \qquad (8)$$

where Δ is the corresponding Green function for the particle with the mass $\varkappa$ and

$$\mathrm{G}(\varkappa) \approx \int_{-\infty}^{+\infty} |a|\, \mathrm{J}_1(|a|\varkappa)\, p(a)\, da \qquad (9)$$

Thus the indefinite metric could be regarded as the result of an averaging process with respect to internal variables and this is so, even when $p(a)$ is positive definite.[15] It should be noticed further that we have, in general, a continuous mass spectrum with indefinite distribution function due to the extended structure of the particle in question.

Although the above reasoning is very crude, it can be inferred that pseudo-Hilbert space with desirable properties may come out, if we find out a suitable model and method of calculation for the particle with internal structure. The method of calculation, however, may differ from that of quantum mechanics. The latter may turn out to be applied after averaged out for internal variable, but it is premature to say anything definite in this connection.

In conclusion, the author would like to emphasize that there are still many other possibilities in extending or modifying present field theory which were not touched in this report.

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(12) Such a point of view was proposed very recently by S. Tanaka.

(13) Y. Takano, *Progr. Theor. Phys.*, **26** (1961), 304.

(14) M.A. Markov, *Nuclear Physics*, **10** (1959), 140.

(15) For instance, if we put

$$p(a) = \frac{1}{2\varkappa_0^2|a|}\, exp\,(-\varkappa_0|a|)$$

we obtain

$$G(\varkappa) = \frac{3\varkappa}{\varkappa_0^2}\left[1 + \left(\frac{\varkappa}{\varkappa_0}\right)^2\right]^{-5/2}$$

which corresponds to one of Feynman's cut-off functions.

Discussion du rapport de Yukawa

H. Yukawa. — I would like to know exactly where the Lorentz non-invariance comes out in Heitler's theory with invariant form factor.

W. Heitler. — I repeat the logic of the theory I explained at the end of my report :
1) We insist on finiteness (for good reasons I believe);
2) We do not maintain local commutativity;
3) We keep to definite metric.
The consequence was that relativistic invariance of the results was violated, although the form factor was invariant.

The mass of the electron for example was $m_0 + \delta m(p)$, $m_0 =$ invariant, $\delta m(p)$ depends on p $[p_0^2 - \vec{p}^2 = (m_0 + \delta m(p))^2]$. In spite of the lack of local commutativity, however, *macroscopic causality* can still be fulfilled in the sense that it is valid after averaging over a small space and time region. Some form factors are known where this is the case. Local commutativity is violated in the sense

$$[H(x), H(y)] \neq 0 \quad \text{for} \quad x - y = \text{space like.}$$

P.A.M. Dirac. — Does the term $\delta m(p)$ vary like the mass of the Abraham electron ? One has a tendency to get something like the Abraham electron when one brings in a form factor.

W. Heitler. — I do not think one gets Abraham's electron. The p-dependence of $\delta m(p)$ depends very much on the form factor chosen. Besides $\delta m(p)$ is only a small fraction of m, it varies between 0 and about 3% of m_0.

W. Heisenberg. — I would like to make two minor remarks, one concerning the dipol-ghost, and the other concerning the last part of Yukawa's lecture which I think was very important.

In our papers we have not considered the dipol-ghost as a very essential feature of the attempts with the indefinite metric. The essential point is that we start from the postulates of pure S-matrix theory (unitarity + analyticity) and add the postulate that there shall exist a local field operator $\psi(x)$, anticommuting at space-like distances. We allow then the mathematical scheme (which is sometimes cleverer than the physicists) to decide about the metric in Hilbert space and the singularities on the light-cone. The Lee-model without " cut-off " is an example, where the mathematical scheme decides in favour of the indefinite metric, but only in the case of the dipol-ghost or two complex roots. In the Pauli-Källén case there is no solution. If one would add the further restrictions of orthodox field theory (positive metric), there would be no solution at all. Therefore the axioms from which we start leave certainly more space than those of the orthodox field theory without introducing any serious danger concerning the probability interpretation in the asymptotic range. We have written down approximate expressions for the commutator using the dipol-ghost, only because this seemed natural from the comparison with the Lee-model and comparatively simple.

The last part of Yukawa's lecture gives an interesting example that by washing out the elementary particles in a relativistic fashion one keeps the property of the commutator (or anticommutator) to vanish at space-like distances (causality), at the same time one abandones the δ-function on the light-cone and thereby introduces the indefinite metric. Generally speaking : if one assumes that one cannot distinguish between elementary or non-elementary particles, i.e. that there are no particles with a δ-function in the centre, then one is probably forced into the indefinite metric, unless one retires into the pure S-matrix theory.

M. Gell-Mann. — If there is an S-matrix theory with CDD poles then it is possible, in a high-energy limit, to extract from the S-matrix some knowledge of off-shell quantities like vertex functions and propagators. If there are only Regge poles, the same mathematical procedure yields not a propagator but zero. Since the propagator is no longer directly relevant to the S-matrix, it may not matter whether it is singular.

W. Heisenberg. — We seem to agree that those expressions that in a scattering problem correspond to the propagator of an intermediate particle in orthodox field theory, would probably vanish at very high momenta in a realistic theory, as Chew suggested.

This would certainly also be true in a theory of the type we have in mind, with an indefinite metric.

Still it would probably be useful for practical calculations to have a local field operator $\psi(x)$ and to know its commutator (without δ-functions!).

This would be possible only in an indefinite metric.

G. Chew. — I am afraid that my response to this question cannot be constructive because, as evident from previous remarks during this conference, my understanding of the field concept for strongly interacting particles is essentially non-existent.

W. Heisenberg. — If the connecting line between the two sides of a scattering diagram is a deuteron-line, wouldn't you expect a pole at the mass of the deuteron and in that sense a deuteron propagator?

M. Gell-Mann. — There would be a pole, but the limiting process on the S-matrix that would give the propagator, in the case of an " elementary " or CDD particle, would yield zero instead.

E.P. Wigner. — I should like to know whether Heisenberg's idea of an indefinite metric is different from Gupta's ?

W. Heisenberg. — When one starts from the postulates of pure S-matrix theory and adds, as we do, as further restriction the postulate that there shall exist a local operator $\psi(x)$, commuting (or anticommuting) at space-like distances, then the Bleuler-Gupta case and the Lee-model (without " cut-off ") and our idea of a unified field theory appear as essentially equivalent. They fulfill the axioms mentioned in a Hilbert space with indefinite metric. Of course there are differences in other points.

L. Van Hove. — Can one roughly say that a genuine elementary particle gives in the Mandelstam representation a pole *and* a single integral (in the corresponding channel); whereas a " non-elementary " particle would give rise to a pole but no single integral ?

M. Gell-Mann. — That is roughly correct.

W. Heitler. — Indefinite metric as a general mathematical theory is not at all developed very far (Nevanlina). The cases where it occurs in physics are all cases where alongside with indefinite metric also a definite metric exists. This is true for the Gupta-Bleuler method and also for the particle equation of spin 0 and 1 particles before second quantization. It would therefore be very difficult to use indefinite metric in its general form.

E.P. Wigner. — It seems to me that Haag gave a clear explanation of the relation between the definite and the indefinite metric in the Bleuler-Gupta theory. The real Hilbert space, which describes the physical states and in which the Lorentz condition is valid, has a positive definite metric. The indefinite metric applies only in the space the vectors of which are obtained by applying one of the components of the electromagnetic potential to the physical states. The vectors obtained in this way are not state vectors which correspond to real states and they do not satisfy the Lorentz conditions. However, their consideration makes it easier to put the Lorentz invariance into evidence.

S. Mandelstam. — I think that, even for composite particles, one can construct a local operator. By taking the Fourier transforms of the vacuum expectation values of its time-ordered products, one can define propagators and continue the S-matrix off the mass shell. I think the procedure is non-singular, perhaps Wightman can confirm or correct me on this. I therefore don't think the procedure of constructing local fields is connected with the existence of elementary particles.

A.S. Wightman. — I think that the local field operator constructed by Zimmerman to describe composite particles is non-singular although the limiting process used for its construction is rather singular.

W. Heisenberg. — May I state clearly the relation between the omission of the δ-function on the light-cone and the indefinite metric. If there is a local field operator $\psi(x)$ commuting (or anti-commuting) at space-like distances, and if the singularity of the

commutator at the light-cone is weaker than a δ-function, the metric in Hilbert-space must necessarily be indefinite. Could Dyson say his opinion on the problem, whether there can be elementary particles having a δ-function in the centre ?

F. Dyson. — I agree with Professor Heisenberg that the non-existence of a delta-function singularity in a particle propagator mathematically implies that the metric be indefinite.

I now wish to ask another question : does the experimental fact that elastic scattering indicates no delta-function singularities in the particle form-factors imply anything about the particle propagators ? My own opinion is that the particles could well have non-singular form-factors, even when the propagators are singular.

E.P. Wigner. — Since we have a few minutes left, we can perhaps return to some of the questions which were not discussed adequately. One of these is the distinction between elementary and composite particles, as defined by Dr. Mandelstam.

S. Mandelstam. — To begin with, let me take the case which one can treat rigorously. One has a field or potential theory with only one two-particle channel, and cut off so that the phase shifts tend to zero or a multiple of π at infinity. If one were to imagine the system put in a box, the high energy levels will then be unaffected by the interaction, and one can count the levels below a certain energy. It can be shown that the number of levels is unaffected when the interaction is switched on and the strength increases since, when a state becomes bound, another disappears from the continuum. When there is no coupling, the number of states depends on the number of elementary particles; so that this must also be the case when the interaction is present as well. If the box is now removed, the counting procedure is expressed as the behaviour of phase shifts. The relation is

$$N(U) - N(C) = \frac{S(\infty) - S(O)}{\pi}$$

where $S(\infty)$ is the phase shift in the relevant channel at infinite energy, $S(O)$ at zero energy. $N(U)$ is the number of states of the uncoupled problem — in other words, the number of *stable* and

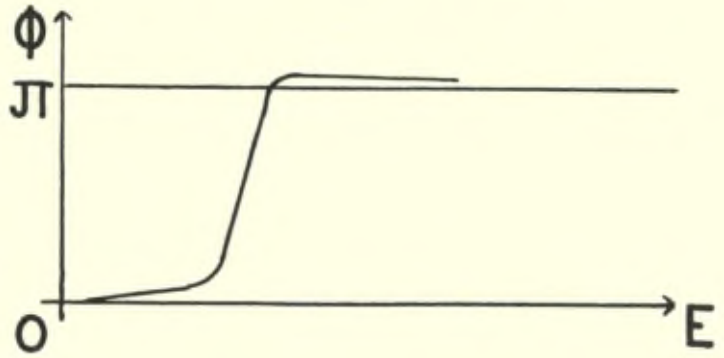
unstable elementary particles. N(C) is the number of states in the coupled problem — in other words the number of stable elementary particles and bound states. The above relation is Levinsohn's theorem, and was derived from him from entirely different principles. As used here it is simply a consequence of the counting procedure and involves no dynamics.

If there are a finite number of channels, S is the sum of the eigenphase-shifts for a particular quantum number, or, the trace of the logarithm of the S-matrix multiplied by $\frac{1}{2i}$. For inelastic scattering involving production there are an infinite number of channels, and we shall assume that Tr(lnS) is still defined. We are proposing the criterion to give us the means of introducing into a calculation the information regarding the number of particles rather than as a rigorous definition and as such it should be applicable even if there is production.

Now to turn to the case of interest we shall still assume that the phase shifts tend to zero or a multiple of π at infinity. This has been found to be the case in the approximations so far treated and, as we have stated, we are proposing a criterion within the framework of these approximations. We now define the number of elementary particles, stable or unstable, according to the number of states with the system in a box. Levinsohn's theorem is then still true, as it results simply from counting the states. We can then use this theorem to distinguish between elementary particles and bound states.

When treating a problem using dispersion relations, one has to put in the information about the number of unstable elementary particles. For instance, in a theory with pions and nucleons there may also be a baryon (of zero strangeness) whose mass is greater than the sum of the masses of the pion and nucleon. Such a baryon would be unstable to strong interactions and would not contribute a term to the dispersion relations. Nevertheless, if we were working with the conventional formalism it would have a different Lagrangian and would therefore give different predictions. An ambiguity corresponding to this has been found in the dispersion relation formalism by Castillejo, Dalitz and Dyson and has since been found to be a general feature. There are an infinity of solutions, whose phase

shifts at infinity differs from that at zero by 0, π, 2π, etc. We therefore associate these solutions with a theory containing 0, 1, 2, etc., unstable particles. It is found that, for a problem with only one channel, the number of arbitrary parameters increases by two with each solution, they correspond to the mass and coupling constant of the unstable particle. If the coupling is weak, the phase shift in, for instance, the second C.D.D. solution behaves as follows, which



is the behaviour one expects with a weakly coupled unstable particle.

G.C. Wick. — There is a quite simple example that shows that the statement that phase shifts go to zero as the energy tends to infinity may not be as general as one assumes. It is the case of a Dirac electron in a central electrostatic field. It is well known that in this case the phase shift for a partial wave tends to a value that is neither zero nor a multiple of π. Perhaps this is nothing more than a mathematical curiosity that is not relevant to the question discussed by Professor Mandelstam. I wonder how he feels about it.

S. Mandelstam. — (Or the audience ?)
Does this arise when the potential is singular ?

E.P. Wigner. — It is quite simple to see how this arises. In the limit of high energy the momentum is a linear function of the energy so that the addition of a potential V changes the momentum by a finite amount in the region where $V \neq 0$. Hence the phase shift remains finite.

M.L. Goldberger. — No particular singularity of the potential is involved. As I recall, the only requirement is that the integral of the potential from zero to infinity be finite. It is also interesting to notice that the phenomenon only occurs when the potential is introduced as the fourth component of a vector potential. This is made clear by Professor Wigner's remark. If the potential is a scalar, that is if it is introduced as an addition to the mass-term in the Dirac equation, then the phase shifts tend to zero at infinity in a normal way.

E.P. Wigner. — There is a question whether the procedure which you outline converges because at really high energies a multitude of particles becomes possible and the density of states increases for this reason. Similarly, the number of characteristic values of the S-matrix becomes infinite.

L. Van Hove. — It is correct that the counting method of unstable elementary particles proposed by Mandelstam is based in an essential way on a comparison between coupled and uncoupled cases ?

S. Mandelstam. — Yes, I should say so. With an uncoupled system in a box, the number of states depends on the number of elementary particles in an essentially trivial way, and we assume that this number is unchanged by the coupling.

A.S. Wightman. — I would like to ask a simple question about the application of these criteria to a concrete case. How would one apply them to the π-meson ?

S. Mandelstam. — The difference between an elementary and a non-elementary π-meson would only arise if we could handle multiparticle states well enough to be able to treat $N - \overline{N}$ scattering as a physical process. The solution (with the quantum numbers of the pion) where S tended to zero at infinity would have no arbitrary parameters. It would probably, of course, depend on other coupling constants, e.g. the coupling constant of nucleons to other elementary particles. If this solution were to have a pole at the mass of the pion with the right residue, we would conclude that the pion was not elementary. Its mass and coupling constants would in principle

be calculable. If, however, the pion did not thus appear " of its own accord ", we would have to take that solution of $N - \overline{N}$ scattering where S tended to π at infinity. We would then have freedom to *put in* constants corresponding to the mass and coupling constants of the pion and, by Levinsohn's theorem, the pion would be elementary.

R.E. Peierls. — Could the spirit of this definition of elementary particles be expressed in a more simple-minded way ? If the interactions at high energy are weak enough to allow the counting of states, or the asymptotic value of the phase to apply, one would expect that particles at high energy should behave like the uncoupled particles. We might, for example, look at collisions at extremely high energy, and expect to see only elementary particles produced with appreciable momentum.

Discussion générale

R.P. Feynman. — Källén said that some features of the Q.E.D. usually obtained, say, by perturbation theory cannot be reproduced by the axiomatic approach. What were they ? Could anyone tell me how complete are the expositions of Q.E.D. starting from axiomatics, and also starting from S-matrix or dispersion theory?

G. Källén. — One can give various answers to this question but what is mainly on my mind is a subject which we have already touched upon several times in our earlier discussions, viz. the time development of a physical system. It appears to me that one basic feature of a pure S-matrix theory and also of some of the more extreme versions of the axiomatic approach is just that one completely forgets the development in time. Of course, it is true that many experimental situations, perhaps nearly all of them, can conveniently be described in terms of scattering processes. However, a pure S-matrix theory goes further and assumes that everything can be described as a *scattering during an infinite time interval.* Everything one really observes is a process which occurs in a finite time interval. To illustrate what I mean I can exaggerate the difficulty and say that one thing which can *never* be measured the way it is mathematically defined is an S-matrix element as we cannot afford to wait that long. This does not mean that the concept of an S-matrix is useless, because even a time interval of, say, 10^{-10} sec can be a very good approximation of an infinite time interval for many purposes. However, I doubt that one can approximate the whole of physics as isolated events taking place over infinite time intervals. Especially, I am thinking of problems connected with measurements of the field itself or, more generally, about the classical limit of a field theory. At least for electrodynamics this is something physically meaningful and I do not see how one can avoid discussing also off-shell quantities in this connection.

R. Peierls. — I think Feynman's question is a little different. I think the question was : to what extend do we know whether all the results from perturbation theory can be or have been reproduced from the other theories ?

G. Källén. — These two points are connected. Of course, if you compute the Lamb shift or a scattering cross section you can restrict yourself to infinite time intervals if you like. However, you cannot do this if you are off the mass shell in a very essential way as e.g. in a classical limit when you speak of a system developing in time and interacting with classical sources. However, perturbation theory with equations of motion gives us a useful technique to handle such situations.

Of course, this remark is not very relevant for strong interactions. I quite realize this and that is the reason why I mainly talk of electrodynamics where it appears to me that there is something more than an S-matrix in the formalism.

S. Mandelstam. — One can reproduce the lower order perturbation terms starting from the so-called axiomatic theory, or from dispersion theory without using a Lagrangian. It has not been proved that one can derive the whole perturbation theory but one gets the main subsets and the rest is a question of mathematical developments.

N. Bohr. — It is clear that in many of the problems of interest in quantum electrodynamics, such as the Lamb shift, one is chiefly concerned with energy relationships, which are determined by spectral instruments, so that one is cut off from any time description; thus in a general way, the question of upholding this description depends very much on what one has in mind. Everyone is aware of the fact that the process of observation implies the use of irreversible amplifying devices. In this connection we may reflect that Boltzmann's suggestion of possible worlds in which the course of all processes would be reversed — with all respect for the logical acuity of his argument — has proved a fallacy. We realize now that every process connected with life is essentially irreversible. Also in the observation problem, one may lose sight of essential points if one does not realize the whole situation in which we find ourselves when we describe experience. Even when speaking of phenomena at very high energies, we must somehow maintain at any rate an asymptotic connexion with the classical model of description. It is the question how far the technical mathematical devices used in the account of such phenomena take into consideration all aspects of the situation. I think that this is something similar to what Källén had in mind.

R. Peierls. — If I understand correctly what Mandelstam said, one can derive from the " axiomatic " approach the renormalized perturbation series, with all subtractions already carried out ?

G. Källén. — When you say that you derive perturbation theory from these general relations, you really start from some approximation which effectively means that you specify the Lagrangian. I think one can say that if you put something in which corresponds to the lowest approximation of a certain theory you can sometimes generate higher order terms by a skilful use of unitarity, etc. But you must always put something in which tells you if you are talking about electrons or about μ-mesons or about pions, etc. At the same time you have to put something in which effectively specifies what would be called the interaction (scalar coupling, vector coupling, etc.) in a Lagrangian language even if you can avoid to use the very word " Lagrangian " explicitly.

S. Mandelstam. — Yes, I have to say clearly which particles are associated with each particle pole.

G. Chew. — I want to call your attention to a fact whose significance I do not completely understand but which gives me great comfort. In a non-relativistic approximation it is possible to identify a piece of the analytically continued S-matrix that plays the role of the potential (always a superposition of Yukawa's). In other words the equations by which the full matrix is generated in terms of this part are identical with those one would deduce from a Schrödinger equation with such a potential. Evidently, then, starting from the S-matrix one can compute the *complete* solution of this equivalent Schrödinger equation, not just the asymptotic behaviour. Of course, a physical significance for such a wave function has not yet been deduced from S-matrix principles, but it is comforting to know that the information content of the analytically continued S-matrix is potentially as complete as that of a wave function.

R.P. Feynman. — I want to look at both sides of the question. First of all, about what Professor Bohr said, I have the impression

that we have analyzed the quantum mechanics of the more complicated objects far enough, apart some cosmological problems about the irreversibility of time, to have a feeling that the details we are worrying about now, like what happens when we hit 2 neutrons together and produce various pions, etc. is not twisted up with the difficulties of the measurement process in the usual sense. We don't have to go all the way back; it is only a guess about what to forget about because obviously we can't always say that we have to study everything at once. I wouldn't bother too much to ask how I measure the quantities except to figure out whether I get a phenomena which I can check.

On the other hand there is a feature of the study of S-matrices that may warrant further analysis. Given the S-matrix for A + B scattering, for B + C and for C + A what limitations, etc. can we deduce on the scattering for A, B and C together. We try to understand physics in complicated situations by combining knowledge in simpler systems. The way to do this for electrodynamics and for non-relativistic systems is known, so that we can deal with solids of 10^{23} particles in terms of knowledge of simple scattering amplitudes of two particles, but I don't know the analogue for the relativistic S-matrix.

N. Bohr. — Of course I am aware that, as Feynman said, the simple problems I mentioned are familiar to everybody. However there are different approaches to the new problems, following different purposes : thus the use of dispersion relations is a very ingenious way of trying to see what one can say about these problems without a field theory; but one must consider the convenience of such methods, and whether one is not concentrating on things which are not the most fundamental.

In this connection, it will not be superfluous to tell of some historical events which may illustrate the situation. I am thinking of a discussion with Schrödinger soon after the publication of his great paper, in which he treated the dispersion problems. Heisenberg and I tried to explain to Schrödinger that if one took seriously his point of view, one would never get the Planck formula, since the latter depended on certain features of individuality of the quantum processes which he had not taken into account. I don't think we convinced him, but at the end he declared that if there was no way to escape from this " Herumspringerei " — he alluded to the tran-

sitions between stationary states — he wished he had never mixed himself up with the problem. This may serve to illustrate the kind of situation to which one may be led by a definite approach, however useful it may seem for some limited kind of problems. Of course the present discussion is not on the same level; but when it comes to asking whether one has got hold of all the points and whether the whole mathematical approach has not been twisted in some particular direction it is not unnatural to think of keeping an open mind for new aspects which may perhaps have to come into consideration.

R. Peierls. — I would like to raise the question how strong are the strong interactions. We tend to assume that, as in the usual field theory, we deal with a coupling constant of 15 (or, in terms of pseudovector coupling, a smaller constant but a matrix element rising with energy) and this tends to colour our thinking about these problems. How sure are we that this is necessarily right ? At one time the high probability of producing two pions rather than one was adduced as evidence of very strong coupling but we now know that this can be deduced from the (3/2, 3/2) resonance. The renormalization of the axial β-decay constant, if one believes in universality, amounts to only a 20% correction, which is not as spectacular as we would have expected. The one striking fact I know in evidence for very strong coupling is the high multiplicity in anti-nucleon annihilation. What do other people think ?

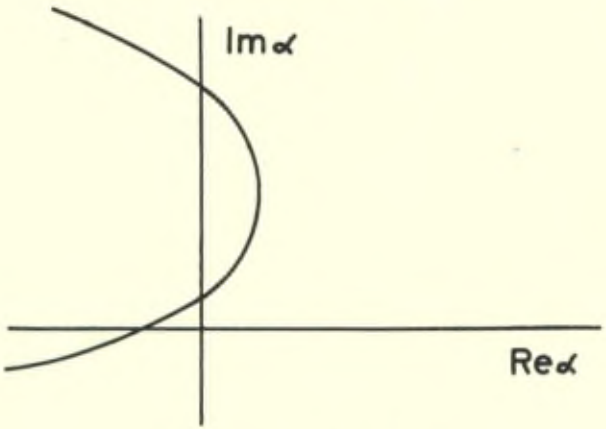
R.P. Feynman. — The newly discovered resonances seem quite narrow. At first sight you would expect a rapid disintegration from strong interaction.

Do you think that the narrow resonances result, in a manner analogous to nuclear resonances, from an inability to find the right channel Σ, π among the hort of others (KN, KNπ, etc.) all connected by strong coupling ?

That is, is the narrowness the possible result of what you call unitarity limitation ?

G. Chew. — I believe the narrow resonances to result from the same mechanism that produces sharp resonances in complex nuclei: with a many-body system but only a few channels energetically open, the intermediate state takes a long time to find its way into an open channel.

M. Gell-Mann. — I want to ask people a question. We heard a description of how Regge showed, for a Schrödinger equation for one particle without exchange forces, that the behavior of poles in angular momentum was something like this :



E is parametric along the curve.

As E increases, when Re α réaches 0 or 1 or 2 or 3, etc. you get something happening in that state of angular momentum. So when the real part passes through 0, if we have a negative energy we have a bound state; with a positive energy, we have a resonance. When the real part passes through 1, at a positive energy we have a resonance, etc. The places where the real part goes back with the energy presumably are connected with the places where the phase shift in each of those states goes back to 90^{0} so that in each angular momentum state, the net change in the phase shift between 0 and infinity is just connected with the number of bound states as it should be. What this means then is that for an individual bound state or resonance we can in general construct a family of states related by this continuation in angular momentum with different angular momentum eigenvalues, connected however in the physical sense to the first state. If you put in exchange forces, as has been said, then the potentials in the even and the odd states become independent. In that case, for example, if we consider the Schrödinger equation for the even states, the existence of these places where the real part of α goes through 1, 3, 5 ... becomes physically meaningless because we are solving with $l = 1$ the Schrödinger

equation which applies only to $l = 0, 2, 4 \ldots$ So we have 2 α-curves (an α even and an α odd). We get a pair of series : a 0, 2, 4, 6 ... series and a 1, 3, 5 ... series.

Now in the study of many-body systems like nuclei, such series are very familiar. For some nuclei, we may think of the levels as representing rotational excitations of the nucleus, I understand, and in other cases the behavior is more like that of a rigid rotator. When the physical conditions are such that the model is near to that of the rigid rotator, then the excitation energies are roughly proportional to $j(j+1)$. In the other case, where the rigid rotator interpretation is quite poor, the spacings are different but you still get these sequences. Another thing which is true is that these sequences may begin with quite high values of the angular momentum so that, for example, you have a ground state with $3/2^+$, then a $7/2^+$, etc.

I think that it would be very good if someone tried to find a formalism that would include at the same time the Regge analysis of this situation in the Schrödinger equation and the physics of these nuclear levels. Probably a simple extension of Regge's work would do.

As pointed out by Goldberger and Blankenbecler, the nucleon, together with the third resonance in the πN system, can be considered to form part of a Regge series ($I = 1/2$; $J = 1/2^+, 5/2^+, \ldots$).

For $I = 3/2$, the ground state is the famous 3 - 3 resonance which has $J = 3/2^+$. Higher up there seems to be some structure in πN scattering that might be interpreted as indicating a level with $J = 5/2^-$. Suppose, for a moment, that there really is a $J = 5/2^-$ state with no $J = 1/2^-$ state below it. Does that pose a very difficult problem ? I cannot believe that it really is terribly serious since it happens all the time in nuclei. It could be that the explanation of the nuclear situation requires anomalous thresholds, but I doubt it. In any case, it would be nice to look for a formalism which would describe the nuclear problem as well as the simple one-particle Schrödinger equation problem because it might be useful for understanding how Regge series can start with high spin.

R. Peierls. — I really am rather doubtful whether one can learn about this problem very much further by looking at nuclei. I think that the case of rotational levels is much more complicated from

many points of view, but you have far much simpler examples in the case of very simple nuclei which follow the shell model.

M. Gell-Mann. — In any case, there are generalizations of Regge's model, without going into field theory which permit us to have a 5/2 state without a 1/2 below it. I would like to find out why this is so.

R. Peierls. — I doubt whether one should worry too much about the application of Regge's theorem to nuclei, since its present justification relates to a very different situation. The existence of ground states with spin greater than zero is very common in nuclei as a result of the Pauli principle. This is connected with a point about Levinsohn's theorem, which may be of interest to Mandelstam's definition of elementary particles. Consider the scattering of a nucleon by an α-particle. At low energy this will be described by an optical potential, so this suggests that the S-phase will run through π, although there is no bound state of the system. This is because there exists in the potential the 1S-state into which the nucleon cannot go because of the Pauli principle. It is an interesting question whether the Levinsohn's theorem can be generalized to this situation and if so what form it takes.

R.P. Feynman. — Usually, in a simple potential lower angular momentum excited states come in before higher ones. Thus the next state above the nucleon $+ 1/2$ would be also $1/2^+$. Under what physical circumstances is this not true ? How can we picture the N, π system so that we can see that $3/2^-$ and $5/2^+$ preceed the next $1/2^+$.

M. Gell-Mann. — In the Schrödinger equation, Regge takes as an assumption that the potential is a superposition of Yukawa potentials.

W. Heisenberg. — May I ask one more question about what we discussed this morning with Gell-Mann ? There was a picture on the blackboard showing how one might connect 2 types of vertices in a scattering diagram and I understood, or possibly misunderstood Gell-Mann saying that there was an essential difference (not only a quantitative difference in mass) according to whether

this line was a nucleon or a deuteron. If that was so simple, one could simply say that the one particle is an elementary particle, and the second one is a compound particle. But I suppose you wouldn't really mean that because if you would mean it, then you would for instance be able at once to decide whether the π is elementary or is a compound of proton and anti-proton.

M. Gell-Mann. — I have hopes that if the things turn out the way we think, by such experiments one can decide in this sense (whether the thing is a CDD pole or a Regge pole), whether a given particle is elementary or not. The first proposal is to try with nucleons, by looking at backward π scattering at very, very high energies and seeing whether in this limit the amplitude goes to zero or to a non-zero function of u.

More precisely, what you investigate by these means is not whether a given thing is elementary but whether there exists a CDD pole in that channel.

M. Cini. — Is it right to say that this sort of behaviour for the scattering amplitude is analogous to what happens with the form factor which would go to a constant at very high energy transfers if the particle is " elementary " and would go to zero if it is not ?

M. Gell-Mann. — That has to do with whether the function $f(u)$ that I wrote down goes to zero or to infinity as $u \to \infty$. This other proposal is different. In the scattering amplitude, if you let $s \to \infty$ at some finite u, do you find $f(u)$, which is a propagator times form factors in field theory, or do you find zero ?

G. Chew. — Our first major objective is : given that the π is the least massive particle of zero strangeness and zero baryon number, to predict the next least massive particles in this family. We hope that it will suffice to consider only singularities corresponding to 2-pion configurations. If such is the case it should be possible in the reasonable future to predict the ratio of ρ- to π-masses without any input beyond the principle of " saturation " of the unitarity condition. However Blankenbecler has given arguments that suggest one may have to predict ρ and ω simultaneously. Equations for such an extended program have not been developed, but they are sure to be extremely complicated.