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SUMMER LECTURES ON THEORETICAL PHYSICS, 1958

by

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INTRODUCTION

This volume contains some lectures on theoretical physics, mostly in the field of elementary particles and field theory, delivered during the summer of 1958 by visitors at the Argonne National Laboratory. The lectures are essentially summaries or reviews of published material.

We have attempted to preserve the "live" form of the verbal delivery, with interspersed questions and answers, wherever it did not conflict with the clarity of presentation or with the lecturer's wishes. A certain amount of colloquialism and lack of polish will perhaps be excusable.

H. Ekstein
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Collision Theory for Composite Particles

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I was asked to translate the contents of the paper to which Hans Ekstein referred[1] into understandable language. That paper is concerned with the collision processes between composite particles in quantum field theory. Its purpose was to unify and to generalize some previous work which had been done on this subject by various authors.[2-5] The best way to make the contents generally understandable is to begin by reformulating the results for the simpler case of a wave-mechanical problem and leave the case of field theory to a later part of these lectures. In particular, let us consider the system of 2 neutrons plus 1 proton. The Hamiltonian is then

$$H = -\frac{\hbar^2}{2M}(\Delta_1 + \Delta_2 + \Delta_3) + V_{12} + V_{23} + V_{13} \qquad (1)$$

where V_{ik} is the potential energy of the nuclear forces between the particles i and k.

The notion of "a state ψ of the system" will always be understood in the sense of the Heisenberg picture. This is convenient (in fact almost imperative) if one wants to extend the formalism later to relativistic quantum field theory. It means that any particular state ψ_a bears reference only to the experimental arrangement "a" which establishes it and no reference, for instance, to the time at which one chooses to make a subsequent observation on it.

Of course, the description of the establishing measurement "α" itself (the label of the state) may, and in fact must, contain some mentioning of a time.

In our system we can distinguish three classes of states, those which before the collision are composed of

A) 3 isolated nucleons,

B) 1 deuteron and 1 neutron

C) a triton.

Alternatively we could also classify the states according to their status of decomposition after the collision (classes A', B', C'). Of course C' = C since the triton will be considered as a stable particle in our model. Instead of "classes of states" we shall use the customary expression "channels".

Our first aim is to define a complete basis system of states in each channel. Now at a sufficiently large time before or after the collision the fragments will be with overwhelming probability separated far beyond the range of their interaction. Therefore it should be possible to describe the motion of each individual fragment at such a time separately. To formulate this expectation more precisely is just a little tedious, but please be patient for a few minutes. We consider for each of the four types of particles (neutron, proton, deuteron, triton) an arbitrary complete, orthonormal basis system of single-particle states. Let us use indices $\alpha, \alpha' \ldots$ for the neutron basis states, and $\beta, \beta' \ldots$, $\rho, \rho' \ldots$, $\lambda, \lambda' \ldots$ for proton, deuteron and triton states respectively.

Remarks: In actual calculations it is convenient to use the basis of momentum eigenstates. Then the indices $\alpha, \beta..$ just specify the momentum of the particle in question. For the development of the concepts and for a concise general formalism it is, however, necessary to limit the choice of α, etc., to normalizable states (wave packets) since for infinitely extended beams one can not speak of the time "before" or "after" the collision. Still, the identification of α with the momentum can be made in our formulae if proper care is taken and in practical applications this is almost invariably done.

As discussed before, we expect that it makes sense to speak, for instance, about a state which before collision is composed of a neutron moving according to the single-particle state α and a deuteron which moves according to ρ. This state of the total system will be called $|\alpha\,\rho>^{(-)}$. Our first result will be to verify this expectation by giving an explicit construction for $|\alpha\,\rho>^{(-)}$.

Remarks: We single out the states of channel B for this illustration. Of course, the procedure is almost completely analogous for the other channels. E.g., the basis states of channel A will be denoted by $|\alpha\,\alpha'\,\beta>^{(-)}$. Because of the Pauli principle we should put in that case

$$|\alpha\,\alpha'\,\beta>^{(-)} = -|\alpha'\,\alpha\,\beta>^{(-)}. \qquad (2)$$

Similarly, $|\alpha\,\rho>^{(+)}$ and $|\alpha\,\alpha'\,\beta>^{(+)}$ denote states with a prescribed final decomposition (channels B' and A'). Sometimes we will call an array of single-particle states like $\alpha\,\alpha'\,\beta$ or $\alpha\,\rho$ a configuration and abbreviate it by a single Latin index such as k.

Thus $|k\rangle^{(-)}$ is the Heisenberg state corresponding to the initial configuration k, $|k\rangle^{(+)}$ the Heisenberg state with the final configuration k. The collision matrix is defined in this notation as

$$S_{k'k} = {}^{(+)}\langle k'|k\rangle^{(-)} . \tag{3}$$

Warning: In many papers the superscripts (+) and (-) are used with the exactly opposite meaning from the one defined here. In these papers $\psi^{(+)}$ denotes an eigenstate of the Hamiltonian with a wave function consisting of a plane wave plus an outgoing spherical wave. This same state would be called $|\ \rangle^{(-)}$ in our notation (if we allow momentum eigenstates as a basis for the single-particle states; see remark 1). The point is that if one forms a wave packet from these states $\psi^{(+)}$ and studies its motion in time, then the outgoing spherical-wave part of the wave function will disappear at $t \to -\infty$ and only the plane-wave part will survive. This means that these states have a simple decomposition at $t \to -\infty$.

Let us now construct the wave functions for the states $|a\rho\rangle^{(-)}$! We need the following parts: $f_t^a(\vec{x})$ and $f_t^\rho(\vec{x})$ shall be the Schrödinger wave functions for the motion of a single neutron or of the center of mass of a single deuteron, respectively. I.e., they are solutions of

$$i\frac{\partial}{\partial t} f^a = -\frac{\hbar^2}{2M}\Delta f^a \ ; \quad i\frac{\partial}{\partial t} f^\rho = -\frac{\hbar^2}{4M}\Delta f^\rho . \tag{4}$$

Further, $\chi(1,2)$ means the deuteron wave function (describing the relative motion of the two nucleons in the deuteron). E_B is the binding energy. Then consider

$$\psi_t = e^{iHt}\, 2^{-1/2} \left(f_t^{a}(\vec{x}_1)\, f_t^{\rho}\left(\frac{\vec{x}_2+\vec{x}_3}{2}\right) \chi(2,3)\, e^{-iE_B t} - 1 \leftrightarrow 2 \right). \tag{5}$$

The second term in the bracket (interchange of coordinates 1 and 2) is, of course, added so that the total wave function be antisymmetric in the neutron coordinates. We now assert that this expression (5) approaches a limit as $t \to -\infty$ in the sense that there is a well defined state ψ such that

$$\| \psi - \psi_t \| \to 0 \quad \text{as} \quad t \to -\infty. \tag{6}$$

Here $\| \phi \|$ means the length of the vector ϕ. For (6) we also write

$$\psi_t \to \psi \quad . \tag{6a}$$

In other words, the sequence of vectors ψ_t has a "strong limit" (converges strongly) as $t \to -\infty$. Obviously the limit state ψ must be interpreted as the state $|\, a\rho >^{(-)}$ introduced earlier.

Sketch of proof for Eqs. (5), (6a).

Take $\frac{\partial \psi_t}{\partial t}$ using the form (1) for the Hamiltonian, together with the definition of f^a, f^ρ, and χ. This gives, apart from trivial factors

$$\frac{\partial}{\partial t}\psi_t \sim e^{iHt} \left\{ (V_{12} + V_{13}) \left(f_t^{a}(\vec{x}_1)\, f_t^{\rho}\left(\frac{\vec{x}_2+\vec{x}_3}{2}\right) \chi(2,3)\, e^{-iE_B t} - 1 \leftrightarrow 2 \right) \right\}.$$

Since e^{iHt} is a unitary operator we need only prove that the norm of the bracket $\{\}$

decreases as $|t| \to \infty$ and in fact it should decrease stronger than $1/t$ so that we can integrate $\| \frac{\partial \psi_t}{\partial t} \|$ up to infinite times. Using the asymptotic form of the solutions of the free Schrodinger equations (4) for large times, one finds that

$$\| \frac{\partial \psi_t}{\partial t} \| \sim \begin{cases} t^{-\frac{3}{2}} & \text{if } V(r_{ik}) \to 0 \text{ stronger than } r_{ik}^{-3/2} \text{ for } r_{ik} \to \infty \\ t^{-n} & \text{if } V(r_{ik}) \to 0 \text{ as } r_{ik}^{-n};\ n < 3/2. \end{cases} \tag{7}$$

A more rigorous method of proof has been devised by J. Cook and M. Hack.[6] They use the fact that for a dense set of wave functions f (Gauss packets) the solution of Eq. (4) is explicitly known.

The construction of the other states $|k\rangle^{(\pm)}$ is obvious by analogy. So, for example

$$|\alpha\alpha'\beta\rangle^{(+)} = \lim_{t \to +\infty} e^{iHt} \frac{1}{\sqrt{2}} \left(f_t^{\alpha}(\vec{x}_1)\, f_t^{\alpha}(\vec{x}_2)\, f_t^{\beta}(\vec{x}_3) - 1 \leftrightarrow 2 \right). \tag{8}$$

Formal collision theory.

The objective here is to extract the essential concepts which are used in the construction of the $|k\rangle^{(+)}$ and to eliminate all those features which cannot be carried over to more general types of collision problems, say those of quantum field theory. One well known "formalization" works with the concept of the "unperturbed Hamiltonian" H_o. In our example for channels A or A', we would have to put

$$H_o = -\frac{\hbar^2}{2M} (\Delta_1 + \Delta_2 + \Delta_3). \tag{9}$$

Then, instead of Eq. (8) we could write

$$|\alpha\alpha'\beta\rangle^{(+)} \underset{t\to+\infty}{=} \lim e^{iHt}\, e^{-iH_o t}\, \frac{1}{\sqrt{2}} \left(f_o^{\alpha}(1)\, f_o^{\alpha'}(2)\, f_o^{\beta}(3) - 1 \longleftrightarrow 2 \right). \tag{8a}$$

The treatment of the collision problem is then reduced to a calculation of the matrix elements of $\lim_{t\to\pm\infty} e^{iHt}\, e^{-iH_o t}$ between eigenstates of H_o. Unfortunately this formulation is severaly limited in scope. For every channel a different H_o must be chosen so that H_o incorporates that part of the interaction which is responsible for the binding of each fragment but leaves out the interaction between the different fragments. In channel B of our example this is possible only by forgetting the Pauli principle during the calculation, and in a general field-theoretical case it will be quite impossible to find a suitable operator H_o. We shall therefore not adopt this method but start from the following obvious remark.

Collision theory depends on the fact that, given any two states ϕ_1 and ϕ_2 which at some time t have their localization volumes far separated from each other, then there is also a composite state or "product state"

$$\phi = \phi_1 \overset{(t)}{\Lambda} \phi_2 \tag{10}$$

which describes the physical situation in which at time t there are to practically isolated subsystems respectively in the states ϕ_1 and ϕ_2. Once we have defined how this product combination is obtained in terms of the customary mathematical symbols of the theory, the construction of the states $|k\rangle^{(\pm)}$ is

obvious. For example

$$|\alpha\rho\rangle^{(-)} = \lim_{t\to-\infty} |\alpha\rangle \overset{(t)}{\Lambda} |\rho\rangle$$

$$|\alpha\alpha'\beta\rangle^{(+)} = \lim_{t\to+\infty} |\alpha\rangle \overset{(t)}{\Lambda} |\alpha'\rangle \overset{(t)}{\Lambda} |\beta\rangle \qquad (11)$$

where α and ρ, etc., are the chosen single particle states. *

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* In talking simultaneously about these single particle states and the states on the left-hand side of Eq. (11), we must, of course, drop in our mind the restriction to a fixed nucleon number.

In wave mechanics there is an obvious definition of the product operation $\overset{(t)}{\Lambda}$. It is the one which has been used in Eqs. (5) and (8), namely in the x-representation at the time t (the time to which the product symbol refers) the wave function of the product state shall be just the product of the wave functions of the factor states (antisymmetrized if necessary). If we refer everything to the time t = 0, then

$$\phi_1 \overset{(t)}{\Lambda} \phi_2 = e^{iHt} \left\{ (e^{-iHt}\phi_1) \overset{(o)}{\Lambda} (e^{-iHt}\phi_2) \right\}, \qquad (12)$$

which clearly gives the expressions (5) and (8). For a more detailed discussion of formal collision theory, see references 7 and 8.

Remarks: Since only the product between far separated states has an ambiguous physical meaning and is ultimately needed, we would be free to introduce any modification of the above definition for $\overset{(t)}{\Lambda}$ at small distances. Thereby one may obtain other equivalent formulae for the $|k\rangle^{(\pm)}$ which are sometimes useful. We will, however, not discuss this here.

Transformation of the states $|k\rangle^{(\pm)}$ under the invariance group; conservation laws.

To say "the theory is invariant under a certain transformation" means a) to every state ψ there is assigned a transformed state ψ'. If we write $\psi' = R\,\psi$, then R shall be either a unitary or an antiunitary operator.**

** A thorough discussion of the assumptions which go into the derivation of this alternative is given in reference 9.

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b) Obviously not every unitary operator can be associated with an invariance property. We only speak about invariance if the transformation of the states is such that isolated subsystems are transformed independently, i.e.,

$$R\,(\psi_1 \overset{(t)}{\Lambda} \psi_2) \approx R\psi_1 \overset{(t)}{\Lambda} R\psi_2 \tag{13}$$

whenever ψ_1 and ψ_2 have far separated localization regions at time t. From Eqs. (13) and (11) it follows then that

$$R\;|k\rangle^{(\pm)} = |k'\rangle^{(\pm)}, \tag{14}$$

where the configuration k' is obtained from k by transforming separately all the single particle states entering into k. Equation (14) in turn implies the invariance of the collision matrix

$$S_{k_2 k_1} = S_{k_2' k_1'}\,; \tag{15}$$

or alternatively, if one introduces the operator S by

$$|k\rangle^{(-)} = S\;|k\rangle^{(+)}, \tag{16}$$

it means that R commutes with S (conservation law for R).

Time inversion and detailed balancing.

If the theory is invariant under time inversion then for the time inversion operator T we have instead of Eq. (13)

$$T(\psi_1 \overset{(t)}{\Lambda} \psi_2) \approx T\,\psi_1 \overset{(-t)}{\Lambda} T\,\psi_2 \ . \tag{17}$$

Therefore, in the same notation as above,

$$T\ |k\rangle^{(\pm)} = |k'\rangle^{(\mp)} \ . \tag{18}$$

Also, T will be antiunitary.[9] This gives then for the collision matrix

$$S_{k_2 k_1} = S_{k_1' k_2'} \qquad \text{(Detailed balancing)} \tag{19}$$

Orthogonality of the states $|k\rangle^{(\pm)}$ and unitarity of S.

Two states belonging to different initial channels (or different final channels) are orthogonal even though the corresponding product combinations [right-hand side of Eq. (11)] at a finite time are not orthogonal. More generally, for two configurations k and k' which are composed of single-particle states belonging to some fixed orthogonal system, one has

$$^{(-)}\langle k'|\ k\rangle^{(-)} = \delta_{kk'} \ ; \quad ^{(+)}\langle k'|\ k\rangle^{(+)} = \delta_{kk'} . \tag{20}$$

How this orthogonality arises may be seen from Eq. (11) in the following qualitative way. Imagine a division of space into cells of large but finite volume V. If $|\sigma\rangle$ is a single particle state and v_{max} the maximal velocity which has an appreciable probability in the state σ then the number of cells

in which one has a chance to find the particle at a large time t will be roughly $(v_{max}t)^3 / V$. Conversely, the probability per cell decreases in proportion to t^{-3}. For a product state $|\sigma_1\rangle \overset{(t)}{\Lambda} |\sigma_2\rangle$ the probability of finding both particles in a particular cell will go like t^{-6} and the probability of finding the two particles anywhere together in one cell will decrease like t^{-3}. We can therefore neglect in the limit that part of the product state for which both particles are in the same cell. On the other hand, for a product state between far separated components one has

$$(\phi_1 \Lambda \phi_2, \phi_1' \Lambda \phi_2') = (\phi_1, \phi_1')(\phi_2, \phi_2') \pm (\phi_1, \phi_2')(\phi_2, \phi_1'). \tag{21}$$

(The second term on the right arises from the symmetrization, and the $\pm$ sign refers to Bose or Fermi statistics, respectively.) Therefore, one obtains from Eq. (11), for instance

$$^{(-)}\langle \alpha\rho \mid \alpha'\rho' \rangle^{(-)} = \langle \alpha \mid \alpha' \rangle \langle \rho \mid \rho' \rangle = \delta_{\alpha\alpha'} \delta_{\rho\rho'}, \tag{22}$$

which is a typical example of the relations (20).

The fact that the orthogonality relations are the same for the states $|k\rangle^{(+)}$ as for the states $|k\rangle^{(-)}$ implies that the collision operator S [see Eq. (16)] is isometric. If we also know that the set of states $|\alpha\alpha'\beta\rangle^{(-)}$, $|\alpha\rho\rangle^{(-)}$ and $|\lambda\rangle$ forms a <u>complete</u> basis for all the states of a system comprising 2 neutron + 1 proton system (and that the same is true for the states $|k\rangle^{(+)}$, then we can conclude that S must be unitary.

Let me make a remark about this last link in the proof of the unitarity of the S-matrix. Unfortunately it is still a missing link. I mean this in the following sense. We have seen that the existence of the asymptotic product states and their properties (14), (20) follow easily if we know that the interaction between two subsystems vanishes sufficiently rapidly with the distance [see, for instance, Eq. (7)] . It would be nice to have a similarly general criterion for the completeness of these asymptotic product states. The question is what conditions must be satisfied by the mathematical frame so that we can be sure there will be no states in the system which can not be described in terms of asymptotic particle configurations. Is there, for instance, a simple criterion which can tell whether some given mathematical model in quantum field theory will necessarily have a complete particle interpretation? I do not know such a criterion at the moment.

The "Weak-Convergence Method" for constructing $|k>^{(\pm)}$.

If in Eq. (5) we insert instead of the correct deuteron wave function $\chi(2,3)$ any other function $\chi_o(2,3)$ which is not orthogonal on χ, then the limit of ψ_t for $t \to -\infty$ will still in some sense exist and may be used to construct $|\alpha\rho>^{(-)}$. In particular, consider the scalar products $^{(-)}<k|\psi_t>$ as t tends towards $-\infty$ and k runs through the set of basic configurations. Using the same reasoning which led to Eq. (22) we see that these scalar products vanish in the limit except for $k = \alpha\rho$. The latter quantity approaches

$$\lim_{t \to -\infty} {}^{(-)}<\rho\,\alpha|\,\psi_t> = (\chi, \chi_o).$$

Therefore, for an arbitrary state $\phi = \sum C_u \, |k\rangle^{(-)}$ we have

$$\lim_{t \to -\infty} \langle \phi | \psi_t \rangle = \langle \phi | a\rho \rangle^{(-)} \; (\chi, \chi_o). \tag{23}$$

In other words, for any approximate deuteron wave function χ_o, the sequence

$$\left(\frac{1}{(\chi, \chi_o)}\right) \psi_t = \frac{1}{(\chi, \chi_o)} \, e^{iHt} \, \frac{1}{\sqrt{2}} \left(f_t^a(\vec{x}_1) \, f_t^\rho\left(\frac{\vec{x}_2 + \vec{x}_3}{2}\right) \chi_o(2,3) - 1 \leftrightarrow 2 \right) \tag{24}$$

converges weakly towards $|a\rho\rangle^{(-)}$ which means that if one wants the scalar product of $|a\rho\rangle^{(-)}$ with any fixed state ϕ, one can replace $(a\rho)^{(-)}$ by $\frac{1}{(\chi, \chi_o)} \psi_t$ and go to the limit $t \to -\infty$.

In quantum field theory all the standard methods for calculating the S-matrix have been based on this construction. Except in a few particularly simple cases the strong convergence method has not been applied there at all because the expressions for the single-particle states (corresponding to the deuteron wave function χ) which would be needed, can not so easily be obtained or handled. In wave mechanics, on the other hand, the weak convergence method is not very familiar although there are some problems to which it could be applied with advantage. Recently W. Brenig (preprint 1958) has made use of this method in nuclear reactions, taking shell-model wave functions rather than the exact wave functions of the target nucleus. The price for this simplification is just that the relations are then not strongly but only weakly convergent, which for the practical handling of most of the formulae

does not make any difference.

Remark. If there were another stable bound state of the neutron-proton system besides the deuteron (call it D'), then Eq. (23) would be more complicated, namely

$$\langle \phi \mid \psi_t \rangle \underset{-\infty}{\longrightarrow} \langle \phi \mid a\rho \rangle^{(-)} (\chi_1 \chi_0) + \langle \phi \mid a\rho_{D'} \rangle^{(-)} (\chi_{D'} \chi_0) e^{i(E_{D'} - E_D)t}. \tag{25}$$

It is interesting to note that the survival of the oscillatory second term is only due to the fact that in a non-relativistic theory a difference in binding energy does not imply a difference in mass. If one takes the mass change required by relativity theory into account, then he comes back to the simple formula (23).

The case of field theory.

We consider for simplicity a model in which we have a single scalar field A(x) satisfying the local commutation relations

$$[\, A(x)\, A(y)] = 0 \quad \text{for space-like distances } x\text{-}y. \tag{26}$$

Let us assume there are two distinct stable particles of spin zero predicted in this model. The masses of these particles shall be m and M, respectively; $\mid a \rangle$ shall be a state of a single m-particle, and $\mid \rho \rangle$ one of a single M-particle.

Consider now a space-time region $\mathcal{L}$. Associated with it is a set of operators $\mathcal{R}_{\mathcal{L}}$, namely all the polynomials in the field with coefficient functions differing from zero only in $\mathcal{L}$. In other words, a general operator from $\mathcal{R}_{\mathcal{L}}$

is of the form

$$q = c + \int c^{(1)}(x)\, A(x)\, dx + \int c^{(2)}(x_1, x_2)\, A(x_1) A(x_2)\, dx_1\, dx_2 + \ldots \tag{27}$$

where $c^{(n)}(x_1, \ldots, x_n) = 0$ if any x_i lies outside of $\mathcal{L}$. Furthermore, there is a subspace $\mathcal{G}_{\mathcal{L}}$ of states associated with $\mathcal{L}$, namely the set of all those states which are obtained by applying the operators belonging to $\mathcal{R}_{\mathcal{L}}$ on the vacuum state $|0>$. We call the states of $\mathcal{G}_{\mathcal{L}}$ "localized states in $\mathcal{L}$". A better term would perhaps be "local disturbances in the vacuum".

The reason for introducing $\mathcal{R}_{\mathcal{L}}$ and $\mathcal{G}_{\mathcal{L}}$ is that we want to define in field theory the notion of a product between far separated states (our previous symbol $\overset{(t)}{\Lambda}$). Now, of course, any state ψ may be written as

$$\psi = \chi\, |0> \tag{28}$$

where χ is some polynomial of the field A. In defining the product between two states, the most obvious idea is to make use of the product between two operators which create these states from the vacuum. There is, however, the following difficulty. The relation between χ and ψ in Eq. (28) is not unique; there are many operators χ which produce the same state ψ. If we wrote

$$\psi_1 \,\Lambda\, \psi_2 = \chi_1 \chi_2\, |0>, \tag{29}$$

where

$$\psi_1 = \chi_1\, |0>; \quad \psi_2 = \chi_2\, |0>$$

the result would vary with the choice for χ_1. A sensible and unambiguous

definition of the product state is given by Eq. (29), however, under the following restrictions. If ψ_1 belongs to $\mathcal{F}_{\mathcal{A}_1}$ and ψ_2 to $\mathcal{F}_{\mathcal{A}_2}$ (where the regions $\mathcal{A}_1$ and $\mathcal{A}_2$ are situated totally space-like with respect to each other) and if the choice of χ_1 and χ_2 is limited to the operators from $\mathcal{R}_{\mathcal{A}_1}$ and $\mathcal{R}_{\mathcal{A}_2}$, respectively, then the assumed local commutativity (26) of the field guarantees that the remaining arbitrariness in choice of the operators χ_1, χ_2 will not change the result of $\chi_1, \chi_2 |0>$.

If we apply these ideas then to the construction of the states $|a\rho>^{(\pm)}$ we are led to consider "almost local operators B(x). By this we mean polynomials in the field A [see Eq. (27)] with coefficient functions $c^{(n)}$ which decrease sufficiently rapidly as any point of the points x_i moves away from x. To be specific let us allow only a finite time interval in which the functions are different from zero and require that they decrease faster than any power with the spatial separation $|\vec{x}_i - \vec{x}|$. The counterpart of Eq. (5) is then

$$|a\rho>^{(-)} = \lim_{-\infty} \int f_t^a(\vec{x}_1, t)\, f_t^\rho(\vec{x}_2)\, q^{(m)}(\vec{x}_1, t)\, q^{(M)}(\vec{x}_2, t)\, d^3x_1\, d^3x_2\, 0> \tag{30}$$

if $q^m(\vec{x}, t)$ and $q^{(M)}(\vec{x}, t)$ are any almost local creation operators for a state of a single m-particle (or M-particle) which is localized in the sense of Newton and Wigner at time t at the position $\vec{x}$. To find such operators $q^{(m)}$ and $q^{(M)}$ means to solve the one-particle problem, just as in the wave-mechanical example we need to know the deuteron wave function χ in order to apply Eq. (5).

One last remark: - these considerations depend on the assumption that the effective interaction vanishes sufficiently fast with the distance. In wave mechanics this means that the potential shall drop faster than $1/r$ [see Eq. (7) and the preceding discussion] . In field theory let us call the counterpart of this assumption "the asymptotic condition in space". It can be explicitly formulated as a condition for the behavior of vacuum expectation values of products of field operators for large space-like distances.[1]

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These lectures on the quantum theory of fields were delivered by Professor Suraj N. Gupta at the Argonne National Laboratory, Lemont, Illinois, during the summer of 1958.

Quantum Theory of Fields

Suraj N. Gupta

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It is a great pleasure for me to visit the Argonne National Laboratory this summer. I understand that Argonne has big plans for the future in the field of high-energy physics, which no doubt will be of tremendous importance to all the physicists throughout the midwest. I am therefore particularly glad to acquaint myself with the physicists here.

In these lectures on the Quantum Theory of Fields, I shall cover the following topics:

I. Quantum electrodynamics

II. Quantum Theory of Gravitation

III. π Meson Theory

IV. Strange Particles

V. General Principles of Quantum Field Theory

VI. Future Outlook

I. QUANTUM ELECTRODYNAMICS

As you know, until about 10 years ago quantum electrodynamics was in bad shape. For instance, it was pointed out by various people that the usual method of quantization of the photon field, which was developed by Heisenberg, Pauli and Fermi around 1930, was inconsistent. This, I think, can be seen very

easily in the following way. We know that the commutation relations for the photon field can be expressed as

$$[A_\mu(x),\ A_\nu(x')] = i\hbar c\, \delta_{\mu\nu}\, D(x-x') . \tag{I.1}$$

We differentiate each side and take the expectation value.

$$\left\langle \left[\frac{\partial A_\mu(x)}{\partial x_\mu},\ A_\nu(x') \right] \right\rangle = i\hbar c\, \delta_{\mu\nu}\, \frac{\partial D(x-x')}{\partial x_\mu} . \tag{I.2}$$

If we now make use of the older supplementary condition

$$\frac{\partial A_\mu}{\partial x_\mu}\,\Psi = 0, \tag{I.3}$$

we see that one side of Eq. (I.2) vanishes while the other side is non-vanishing. There are other complications, but I do not want to go into details. I just want to point out that the older formalism of quantum electrodynamics was known to be inconsistent.

Then I gave a theory of the quantization of the photon field which is now well known.[1] It differed from the old formulation in two ways. Firstly, I made use of an indefinite metric and secondly the supplementary condition (I.3) was replaced by a weaker supplementary condition which can be expressed as

$$\left[\frac{\partial A_\mu}{\partial x_\mu} \right]^+ \Psi = 0. \tag{I.4}$$

After this paper was published it was generally agreed that this work should replace the older work which was done, as I said before, around 1930.

I do not wish to discuss the mathematical details of the quantization of the photon field here, but I would like to mention that recently I have received a reprint from Sunakawa,[2] in which he claims that my treatment of the quantization of the photon field is not Lorentz covariant. Now this problem of Lorentz covariance has been discussed in my own paper as well as by several people. I might particularly mention a paper by Belinfante,[3] in which he has discussed the covariance of my formalism in very great detail, and I have received a letter from him and some other people pointing out the error in this paper of Sunakawa.

The point is that in the formalism of indefinite metric two sets of quantities are introduced, which are called the Hermitian conjugate and the adjoint, and unless a person is very familiar with the formalism he is liable to make a mistake by confusing the two. But I think it is all now not of much interest, because I have published a paper in the Canadian Journal of Physics[4] which contains a much simpler formulation of quantum mechanics with an indefinite metric. In this formulation only one set of quantities is introduced, and thus the treatment of indefinite metric becomes as simple as a theory with a positive definite metric.

Further, in quantum electrodynamics one of the major problems, as you know, has been the problem of divergencies. This problem can be split

into two different problems, and these two problems are:

1. how to remove the divergencies, and

2. how to handle the divergent quantities.

Of course, if there were no divergencies the second problem would not arise at all, but we know the results are divergent and then these are the two problems which arise. As we all know, these divergencies can be removed by renormalization. This is well known. People may differ very slightly in their interpretation of what renormalization means. My own interpretation, which is of course provisional because at the moment we cannot say what we shall find ultimately, is to look upon renormalization purely as a subtraction procedure. That is, in the usual theory we introduce an interaction term

$$L_{int} = \frac{1}{c} A_\mu j_\mu , \tag{I.5}$$

while renormalization simply means, at least to me, that we replace the above interaction by the new interaction

$$L_{int} = \frac{1}{c} A_\mu j_\mu + \delta m \, c^2 \overline{\psi} \psi - \frac{\delta e}{ce} A_\mu j_\mu , \tag{I.6}$$

as suggested by Schwinger. The quantities δm and δe are just some constants which one could regard as mass and charge renormalizations. I myself suggested a slightly different interaction[5]

$$L'_{int} = \frac{1}{c} A_\mu j_\mu + \delta m \, c^2 \overline{\psi} \psi + \frac{1}{4} \delta f F_{\mu\nu}^2 , \tag{I.7}$$

and many physicists have found that for mathematical purposes Eq. (I. 7) is more convenient. So renormalization in some way or other as a philosophy can be adjusted according to one's taste, and by renormalization we can remove or subtract the divergencies out of the present formalism of quantum electrodynamics.

But then this second problem, which is essentially related to the first, is also a very serious problem. In fact, some people, who do not know renormalization very well, say almost jokingly that one just subtracts infinities out of infinities and one does not know what all this means. From a mathematical viewpoint when one is dealing with infinities the equations are not entirely meaningful. This is a problem on which a great deal of work has been done. I have myself done work on this problem [6] and I definitely feel that at the moment this is the best thing we have. What I have done is essentially to introduce auxiliary fields in quantum electrodynamics. Physicists have been talking of introducing additional fields to remove divergencies for a long time. In fact I myself do not know who published the first paper on introducing a neutral vector meson field in quantum electrodynamics. There are quite a few very old papers and many physicists have worked on this problem. But the difficulty always has been that if there is an additional field then why don't we observe that field. In my paper for the first time, by using an argument which has never been used before (at least I am not aware of anyone using the argument), I have shown that it is possible to introduce auxiliary fields in quantum electro-

dynamics in such a way that these fields remain completely unobservable. In this case also it is necessary to use an indefinite metric.

So by introducing auxiliary fields in such a way that they cannot be observed, it is possible to remove all divergencies in a purely formal way. But in order to insure that these auxiliary fields are unobservable, one has to impose a restriction; and the restriction is that the masses of the auxiliary fields should be greater than the total energy of the physical system under consideration in that frame of reference in which the physical system has minimum energy. This condition, of course, can be satisfied in any physical problem, because in any problem we deal with a system containing a finite amount of energy and we can always choose the masses in such a way that they are larger than the energy of the system under consideration. With this condition one can work out anything. We can carry out renormalization and obtain any result of physical interest. Then ultimately, because it would be very artificial to impose an upper limit to energy, at least at the moment, the best thing would be to let these auxiliary masses tend to infinity. Thus, throughout the entire calculations in any physical problem it is possible to proceed in such a way that there are no infinities anywhere. Then in the final finite result we can make the auxiliary masses tend to infinity, and everything is unambiguous and finite. Thus, by using the formalism of auxiliary fields the self-mass and self-charge, δm and δe, for all practical purposes can be treated as finite. Since in all manipulations we can treat δm and δe as finite quantities, many objections

which are raised against quantum electrodynamics disappear in this way. I think from a purely mathematical point of view one can say that quantum electrodynamics is in a form where it is entirely self-consistent and one can remove all the divergencies and there is no real problem left.

One can also show that practically all the low-energy phenomena can be explained up to a fantastic accuracy by using quantum electrodynamics. Of course, I mean only those phenomena which lie wholly within the domain of quantum electrodynamics. But if one does not like the present formalism of quantum electrodynamics one could raise two objections: Firstly, one could say that the whole idea of renormalization is mathematically very inelegant and crude and, because there is a belief which we all share that the ultimate laws of nature are very beautiful and simple, one might say that renormalization cannot be the final solution. There it becomes a matter of philosophy and personal feeling, and it is very difficult to be dogmatic in these things, but I will personally say that I share the belief that renormalization is only a provisional solution and ultimately we shall have to find something else. The second objection, which is more serious, is that it has been claimed that quantum electrodynamics as it exists now becomes inconsistent at high energies. Now there are many proofs of this — there are proofs of divergence of the perturbation series and all kinds of things. They are not very clear and I don't think there is a single paper which is free from all objections and which shows that quantum electrodynamics definitely breaks down at high energies, though some arguments do create a

feeling that quantum electrodynamics might break down. But at high energies even if we ignore this unclear work which is purely theoretical, there is something more tangible which has also been found. This is experimental work, and I do not consider myself competent enough to give a definite opinion, but there is some evidence which seems to show that the present quantum electrodynamics is unable to explain phenomena taking place at high energies. One example which you probably all know is the photon shower discovered by Schein.[7] He has observed a photon shower which shows that the probability for multiple photon production at high energies (energies of the order of 10^{14} ev) is far greater than what can be accounted for by using quantum electrodynamics.

I have investigated this problem of multiple photon production in quantum electrodynamics[8] in great detail. The net result is that within the framework of quantum electrodynamics it is absolutely impossible to think of any process in which one could produce a photon shower of the type observed by Schein. I might say that very few examples of this type of shower have been observed so far. Some have been observed in Italy, but then the number is so small that one cannot say with certainty that here one has found something to show that quantum electrodynamics really breaks down. Still it is much more tangible than purely theoretical arguments which are not very clear. Of course, some people have also argued that it may not mean multiple photon production, but I think that was in the beginning. Ultimately I think the majority of physicists agreed that it must be multiple photon production, because when we consider the half-angle of the cone in which the shower is contained and the photon energies, we can show

that these photons could not be produced by the decay of π^0 mesons.

Then there are some other experiments. I do not have their references here, but you can easily find them in the Physical Review. They are about electron tridents. When an electron is scattered by a nucleus, then we all know it can emit a photon. Instead of this we can have a process in which the photon appears only in a virtual state and then it forms an electron pair. This is the process which has been studied by many physicists. Again they seem to differ, but I think several physicists have claimed that the cross section for electron tridents at high energies is considerably higher than that predicted by theory. So there seems to be a strong indication that at high energies electromagnetic interactions become much stronger than what is predicted by the usual quantum electrodynamics, and at the moment we do not know how to explain this.

One way would be to do more theoretical work as people are doing. But to me it seems that the difficulties which they point out are so formal that they talk like pure mathematicians, and the objections that they raise do not seem to have anything to do with physics. I personally do not like the type of work, purely mathematical work, which is being done to establish the fact that quantum electrodynamics really breaks down at high energies. But of course it is a very important problem. If a person expects to find something sensible he should certainly devote himself to this problem. In fact, this is the only clear problem left in quantum electrodynamics. At low energies we know everything. So what happens at very high energies? This is the only problem in quantum electrodynamics which is worth investigation at the present time.

I think the more promising approach to this would be purely experimental. When we have more high-energy machines and if we are able to study electromagnetic interactions at high energies under controlled conditions, then I think it will greatly add to our understanding of quantum electrodynamics. It is indeed very essential that some more quantitative experiments should be carried out with very high-energy machines on problems which lie wholly within the domain of quantum electrodynamics. Of course, one might say that at high energies other fields will also produce important effects in virtual states. All these things are very complicated and one could speculate as much as one would like, but I think it is quite obvious that more experimental information is needed in this field. Now I think I have said whatever I wanted to say about quantum electrodynamics.

Tanaka: In regard to this question of evidence that quantum electrodynamics may break down at high energy, do you have any opinion as to whether or not it may break down in nucleon structure?

Gupta: I think it will be better if we discuss this either when I come to π meson theory or general principles of quantum field theory. All I can say at the moment is that when we talk of mesons then there is no need to go to high energies. If one has no theory even at low energies, one cannot say the theory breaks down at high energies.

Rosenzweig: Can you conceive of an experiment at high energy that would be a real test of quantum electrodynamics and not involve just

the other things that you have mentioned — mesons and other particles that can be created at such energies?

Gupta: There are experiments according to which some physicists claim that at high energies the cross section for electron tridents becomes about twice the theoretical cross section and one could no doubt think of other similar experiments. But I have seen very little of how these experiments are done.

Peshkin: Is it quite certain that a more accurate investigation of the Lamb shift will not show that quantum electrodynamics may break down at low energies?

Gupta: Well, perhaps I should put it like this. There are many problems to which a person can devote his attention and certainly there is some possibility that even by devoting oneself to low-energy processes one might come up with some discrepancy. In fact, in the discussion of the Lamb shift (not for hydrogen but for atoms containing several electrons) some physicists feel that there is a discrepancy. But these are extremely tedious calculations, and I try to keep away from such calculations. There are some very laborious calculations carried out by several people at Cornell, in Canada, and in Sweden. The discrepancies between the various calculations are far greater than those with the experimental results. I think Kinoshita and Salpeter have been doing some work of this kind at Cornell.

Peshkin: Is the calculation of the logarithmic term for the Lamb shift entirely satisfactory?

Gupta: There are certain phenomena which are very clean, like the magnetic moment. They are very clean from a theoretical point of view, and there are other problems in which a person has to make some crude calculations. This is purely a matter of calculations, and it is not a difficulty of principle. When it comes to bound states and problems of that kind, it is impossible to carry out very clean calculations. So I don't think it has anything to do with the principle, but as I have said before, some people have claimed that in the case of the Lamb shift for atoms containing more than one electron, the theory does not agree with experiments. Yet their results are so different from each other that I wouldn't care to make any comment. It is true that there are only a limited number of experiments for which the theoretical results can be obtained in a well defined way, but in most cases one has to make many crude assumptions and approximations which are often justified but still are not very pleasant for a theoretical physicist. So when I have nothing else to do and no fundamental problem to think about, then I might start doing that kind of calculation. At the moment there are many fundamental problems which have to be investigated.

II. QUANTUM THEORY OF GRAVITATION

The theory of gravitation is a subject about which most physicists, I am sorry to say, know very little; and therefore I think I shall first discuss the background very briefly before I actually discuss the quantum theory of gravitation. We know that at the beginning of this century there were two field theories which were well known. One was Maxwell's theory of electromagnetism and the other was Newton's theory of gravitation. Then Einstein proposed his special theory of relativity, and the special theory of relativity was able to express Maxwell's theory in a very beautiful form. But at the same time it gave a fatal blow to Newton's theory. That is, as soon as the special theory of relativity was accepted it became clear that Newton's theory cannot be an exact theory of gravitation, because it is not Lorentz covariant. Then the question arose, "What is the exact theory of gravitation?" What I want to emphasize at the moment is that whether we believe in Einstein's theory of gravitation or not, there is no competition between Newton's theory and Einstein's theory, because we know for certain that the special theory of relativity is correct and therefore we know for certain that Newton's theory of gravitation cannot be an exact theory of gravitation. At first many attempts were made, and various theories of gravitation were proposed. But then Einstein himself gave his theory and it was universally accepted, and at the moment most physicists are not even aware of what those other theories were which people suggested at first.

Let us look upon this problem in a more modernistic way and let us ask ourselves the question, "If Newton's theory of gravitation is not an exact theory then what are the requirements which an acceptable theory of gravitation must satisfy?" From a purely scientific point of view one could lay down three conditions:

1. It should be Lorentz covariant, because after all if a theory is not even Lorentz covariant it can hardly be accepted as an exact theory.
2. It should reduce to Newton's theory as a very good approximation.
3. It should also explain the three crucial tests.

Now in the usual textbooks on relativity one finds a lot of philosophy of general covariance, principle of equivalence, etc. But from a purely scientific point of view if a theory satisfies the above three requirements the theory is good enough. This problem of constructing various theories of gravitation with the above requirements is one I have discussed in a paper in the Reviews of Modern Physics.[9]

The way I have approached this problem is that every classical field must correspond, on quantization, to particles of integral spin. So the gravitational field could have spin 0, 1, 2 or something more. Let us proceed using the usual principles of the field theory, and let us start with spin 0. If we construct a theory of spin 0 then it corresponds to a theory which was actually proposed before Einstein gave his theory of gravitation. It was proposed by Nordström.[10] One finds that this theory, of course, is Lorentz covariant and

it reduces to Newton's theory as a very good approximation, but for the advance of the perihelion of Mercury it gives a value which is one-sixth of Einstein's value in magnitude and opposite in sign. Thus this theory certainly cannot explain the advance of the perihelion of Mercury, and it fails in this respect. Now if we come to spin 1, we can show by using simple arguments that a theory of spin 1 would be absolutely identical with the electromagnetic field. The only difference could be that the gravitational charge of a particle might be different from the electric charge. One certainly cannot use a field of the electromagnetic type to describe the gravitational field because of many reasons. One obvious reason is that according to the electromagnetic theory the force between two like particles has to be repulsive, and there is no way in which one could modify the Maxwell-Lorentz equations such that the force between two electrons becomes attractive. But in gravitation we know that the force not only between like but between all particles is always attractive. Thus, the Maxwell theory cannot be used to describe the gravitational field, and spin 1 can also be left out.

When we go to particles of spin higher than 1, the theory becomes more complicated. In fact, as we go to higher spins the theory becomes more and more involved, and I think it will be enough not to go beyond 2. If we consider the case of spin 2, then this is a case which has also been discussed before by several physicists. I think the most important of them is Birkhoff.[11] He gave a theory of gravitation, which is essentially a theory of a field of spin 2. His theory is Lorentz covariant, and it reduces to Newton's theory as a very good

approximation. It also explains the three crucial tests. However, this theory is mathematically very clumsy, and when he did this work there was a controversy between Birkhoff and Einstein and his followers. This took place around 1945, largely between Birkhoff and Weyl, and they were both pure mathematicians. If anybody thinks that only physicists lose their tempers, they should read the papers of Birkhoff and Weyl. Weyl first published some papers in the Proceedings of the National Academy of Science. Then he published a paper in the American Journal of Mathematics.[12] If one were to look into these papers, one would find that what Birkhoff essentially did was to point to the facts which can be experimentally observed, and Weyl appealed largely to emotions and all he could say was that Birkhoff's theory was very clumsy. Einstein put it this way — that he does not want to go backwards, but his arguments were hardly scientific.

Now because I became very much interested in this thing, I read the various papers very thoroughly and thought about all these problems. Then by studying Birkhoff's theory more seriously, I could actually show that his theory is incorrect and that it contains a very serious difficulty.

This difficulty does not arise in classical theory but in quantum theory. The difficulty is that in Birkhoff's theory the energy of the gravitational field is not positive definite. This means that if we quantize Birkhoff's field, it corresponds to gravitons of negative as well as positive energies. Particles of negative energy are strictly forbidden in quantum field theory because of the

following reason: We know that according to the perturbation theory, virtual particles can be created in vacuum but they remain virtual because the perturbation theory contains the law of conservation of energy within it. However, if there are particles of negative energy then, without violating the law of conservation of energy and momentum, one can have creation of real particles of positive and negative energies even in vacuum. Thus, if we allow particles of negative energy, the state of the vacuum becomes unstable, which is absolutely fantastic. When we think of Birkhoff's theory in terms of quantum theory, we can see that this theory is unacceptable. But what about Einstein's theory? Is it possible to quantize Einstein's theory of gravitation?

Before we discuss the quantization of Einstein's gravitational field, let us look at this difficulty of negative energy in Birkhoff's theory a little more closely. In Birkhoff's theory the field equation for the gravitational field is

$$\Box^2 U_{\mu\nu} = \kappa T_{\mu\nu}, \tag{II.1}$$

where κ is the gravitational coupling constant, and $T_{\mu\nu}$ is the energy-momentum tensor of the matter field. By matter we mean everything except the gravitational field. The reason why the energy of this field is not positive definite is that to make the energy positive definite one has to impose the supplementary condition

$$\frac{\partial U_{\mu\nu}}{\partial x_{\mu}} = 0 . \tag{II.2}$$

This condition will be compatible with (II. 1) only if

$$\frac{\partial T_{\mu\nu}}{\partial x_{\mu}} = 0 .$$

But $T_{\mu\nu}$ is the energy-momentum tensor of the matter field alone, and therefore $\partial T_{\mu\nu}/\partial x_{\mu}$ cannot be equal to zero, because only the divergence of the total energy-momentum tensor, which must include the gravitational field also, can vanish. If we want to remove this difficulty of negative energy, then (II. 1) must be replaced by

$$\Box^2 U_{\mu\nu} = K (T_{\mu\nu} + t_{\mu\nu}) , \qquad \text{(II. 3)}$$

where $t_{\mu\nu}$ is the energy-momentum tensor of the gravitational field. It is then possible to impose the supplementary condition (II. 2) which removes the difficulty of negative energy.

Since the energy-momentum tensor of the gravitational field must be at least a quadratic function of the U's, we can see that Eq. (II. 3) has become non-linear. Moreover, as I have discussed in a paper in the Physical Review[13] it can be shown that if we derive an equation of the type (II. 3) from a Lagrangian density, then the Lagrangian density must contain an infinite number of terms, that is, the Lagrangian density is of the form

$$L = L_0 + k L_1 + k^2 L_2 + k^3 L_3 + \cdots . \qquad \text{(II. 4)}$$

But there is no simple way in which one could directly find the various terms in

Eq. (II. 4). One could easily find one or two, but in general it will be so difficult to obtain L_n that this approach does not seem very worthwhile. So, we see that if we want to remove the difficulty of negative energy, the field equation has to be non-linear and the Lagrangian density has to contain an infinite number of terms in various powers of the gravitational coupling constant.

Now let us look at this situation from Einstein's point of view. First of all, as everyone knows, Einstein's theory of gravitation is very beautiful from a mathematical point of view. Secondly, from Einstein's theory one can obtain a reasonable explanation of the three crucial tests without fiddling with any coupling constants or other manipulations as people often do in meson theory. Thirdly, after attempts for about half a century, nobody could find a reasonable alternative to Einstein's theory. Considering all these things, Einstein was firmly convinced that his theory of gravitation is certainly correct. But then he was faced with two problems: Firstly, due to the use of the Riemannian space, Einstein's theory of gravitation is strikingly different from other field theories; and secondly, it does not seem possible to quantize this field. In his later years Einstein was particularly obsessed with these two things. Because his theory is different from other field theories, he tried to construct unified field theories, and because he could not see how his theory in Riemannian space could possibly be quantized, he attacked quantum mechanics.

Many people have found it very curious that Einstein, who himself played a pioneering role in the development of quantum physics, should so persistently

criticize quantum mechanics. There has been a great deal of controversy and the majority of physicists do not agree with Einstein in his criticism of quantum mechanics, and I am prepared to say that I do not find anything illogical in the basic formalism of quantum mechanics. Of course, it is possible that ultimately one may have to find something new, and of course there are difficulties in quantum mechanics, but the basic formalism seems to me perfectly logical.

Because relativity and quantum mechanics provide the basic foundations of modern theoretical physics, it certainly seems very surprising that there should be a conflict between general relativity and quantum mechanics. In fact, it will not be an exaggeration to say that from an intellectual point of view this has been the most outstanding problem facing the theoretical physicists for half a century. I have shown that this situation can be cleared up by quantizing the gravitational field, and what I am going to say is based on my two papers.[14]

First of all it is obvious that to carry out the quantization of the gravitational field, one may have to depart from Einstein's ideas in some ways. My treatment, in fact, is based on a new approach to Einstein's theory of gravitation. I have shown that by a certain mathematical expansion it is possible to reduce Riemannian space to flat space in such a way that the theory remains Lorentz covariant. When this expansion is carried out and the theory becomes a theory of gravitation in flat space, then something exceedingly interesting happens. One can show that in flat space Einstein's field equations for the gravitational field can be reduced to the form (II. 3) with the supplementary condition (II. 2) . In Eq. (II. 3), $t_{\mu\nu}$ consists of an infinite series, and therefore, although

the field equation contains an infinite number of terms, still it can be written in a very neat and simple form. The term $t_{\mu\nu}$ in (II. 3) is also very satisfactory from a purely philosophical point of view, because in Einstein's theory the energy-momentum tensor is the source function and there is no reason why we should leave out the energy-momentum tensor of the gravitational field itself. In fact, this theory, as you can see, has a very striking correspondence with Maxwell's theory, which is based on the field equations

$$\Box^2 A_\mu = -\frac{1}{c} j_\mu \qquad \text{(II. 5)}$$

$$\partial A_\mu / \partial x_\mu = 0 . \qquad \text{(II. 6)}$$

One can also see why Maxwell's field equations are linear while Einstein's field equations are non-linear. This difference of non-linearity is produced because the photon field does not contribute to the current four-vector while the gravitational field contributes to the energy-momentum tensor.

After the Lagrangian density for Einstein's gravitational field is expanded as an infinite series, we can regard the first term as the unperturbed Lagrangian density for the gravitational field, while the remaining terms can be regarded as a direct interaction between the gravitons. Then by following the same procedure as I have used for the photon field, that is by using an indefinite metric, it is possible to quantize the gravitational field. We can develop the theory in such a form that we can calculate any quantity which is of experimental interest.

On quantization one can show that gravitons are particles of spin 2. They have two states of polarization, and of course besides two real gravitons there are virtual gravitons which produce the static force. In this way we are not only able to quantize the gravitational field, but the theory is in a form where, just as in quantum electrodynamics, one can calculate anything. One might ask what one could possibly calculate with the quantum theory of gravitation. I must say that because the gravitational coupling constant is exceedingly small, the quantum theory of gravitation certainly can never become as important as, let us say, quantum electrodynamics. But still, until the gravitational field was quantized, one always wondered whether Einstein's theory was really the correct theory of gravitation.

There is one very interesting application which has been carried out, not by me but by Corinaldesi,[15] by using my treatment of quantization of the gravitational field. He also reported his work in Switzerland at the International Conference on Relativity. Using the quantum theory of gravitation, he considered the mutual scattering of two particles of spin 0. He only considered the lowest-order diagram in which just one graviton is exchanged between two particles of spin 0, and thus he obtained the effective potential between these two particles. Then he worked out the two-body equation of motion using the two-body potential. It is very interesting to see that although his procedure is so different from any which has been used before, he was able to obtain exactly the same two-body equation of motion as obtained by Einstein, Infeld and Hoffmann.[16] This well-known paper of Einstein et al. is extremely difficult

to read and it contains very laborious calculations. But by using mathematical techniques, which are entirely different from what they used, one finds that what they obtained is nothing but the effect of the exchange of one graviton between two particles of spin 0. There are other problems which one can investigate. One can show that the gravitational interaction between particles depends on their spin, and there are some other interesting things. But from a purely experimental point of view, one cannot do much in this line.

This quantum theory of gravitation is not just something which one would look at only out of curiosity. In fact, it is possible to obtain some extremely important information out of it. One very interesting point, which I have investigated, is whether it is possible to remove the divergence difficulties in the quantum theory of gravitation by renormalization, and the answer is that it is not possible. So, in the interaction of the gravitational field with electrons and photons it is not possible to remove the divergencies by renormalization, and this I believe is another strong point against the renormalization theory. It seems to be only a provisional approach. Further, let us consider the interaction of two electrons. Then we know that the gravitational force between two electrons is very small compared with the electromagnetic force. But it is easy to show that at exceedingly high energies, which are certainly not of experimental interest, the gravitational interaction exceeds the electromagnetic interaction. One could either show this from the quantum theory of gravitation or one could even see without any calculations, because at very high energies

the masses of electrons become very large while the electron charge remains the same. So it is obvious that in the ultimate solution of the divergence difficulties the gravitational field is bound to play an important role. That is, when ultimately our knowledge becomes clearer about the structure of elementary particles and what happens at high energies, the gravitational field certainly can not be ignored.

But the most interesting thing, at least the thing that seemed to me more interesting than anything else, is the implications of this work in cosmology. Cosmology is a branch of knowledge which is so speculative that perhaps many people will not be interested in it. But still if one would look into the work which has been done in this line, I think one would find that it is not all that absurd. One thing which is widely known is that using Einstein's theory of gravitation it is possible to construct various cosmological models. There is a variety of these models, and there is a great difference of opinion as to which model is correct. But if we believe in this quantum theory of gravitation, then two things come out of it. Firstly, in Einstein's gravitational field one sometimes introduces a cosmological term. Now I have carried out this quantization without the cosmological term. But if we introduce this term, it becomes meaningless in the quantum theory of gravitation. At least I don't know how to handle this term in the quantum theory of gravitation. It is therefore reasonable to conclude that the cosmological term must vanish. Secondly, the whole basis of this work is that space is flat. Therefore, if we accept this quantum theory of gravitation, then we must also accept the fact that space is flat. It is not curved either with

negative or positive curvature — it is flat. When we accept the above two facts, the cosmological model becomes more or less fixed. There are still a few things which are rather doubtful, for instance the density of matter in the universe about which various speculations differ widely. But still these two facts eliminate most of the cosmological models. This is all I wanted to say about the quantum theory of gravitation, and before I come to the next topic, the π meson theory, I shall be very glad to answer any questions.

Havas: I would like to make a few remarks. No doubt the mathematics of what you have done is quite correct, but the question of the interpretation of what you have done is something else again. I am especially talking about the equations of motion of particles in a gravitational field. When you look at the gravitational radiation of particles, for example, you do not get a sensible answer if you treat the gravitational field as an independent quantity. I think that in the way you proceed you necessarily treat the gravitational field as an independent quantity and therefore you do not get the results which are physically correct. The other thing which I would like to comment on is the interpretation you put on writing things in terms of the flat-space metric. It is quite true that you can write the Einstein equations in terms of the flat-space metric, but this does not mean that one has reduced the Riemannian space to flat space. It is mathematically equivalent, but says nothing about what the actual metric of that space is.

Gupta: I shall answer the second question first. When we reduce the theory to flat space, it is quite true that there is no apparent basis for deciding whether the space is flat or curved. Now my outlook is that if we regard the space as flat then we can quantize the field. If we regard the space as curved we cannot quantize it. Therefore, I believe that the space is flat. If somebody could show me how he can quantize the gravitational field in the Riemannian space, then I shall say that we do not know whether the space is flat or curved. But I think it is now fairly clear that it is impossible to quantize the gravitational field in the Riemannian space. It can be quantized only in flat space, and that is why I say that the space is flat. Coming back to the first thing, there is a great deal of controversy, and there is a very large number of papers. Einstein himself has discussed this problem of gravitational radiation. This is discussed in the book of Landau and Lifschitz, and several other physicist maintain that there is gravitational radiation. Rosen at first, at the International Conference in Switzerland, claimed that this conception of gravitational radiation is incorrect. But a few months ago he published a letter in the Physical Review in which he has realized his mistake, and now he claims that he believes in gravitational radiation. I certainly very much believe in gravitational radiation, and I have no doubt that slowly everyone will accept that gravitational radiation can exist just like electromagnetic radiation.

Havas: I think you misunderstood my objection. I did not say that there was no gravitational radiation. As a matter of fact, I did calculate the equations of motion with the radiation reaction in a recent paper. The point is that, because of the vanishing of the gravitational dipole radiation, it is impossible to treat the gravitational field as an independent external field in calculating the emission of gravitational radiation by a particle.

Gupta: I have looked into your paper, and I very much doubt that what you have done is correct.

III. π MESON THEORY

The next thing I want to discuss is the π meson theory. As everyone knows, π mesons are pseudoscalar particles and they play a major role in nuclear forces. However, although physicists have been working for more than 20 years on meson theory, still it is not possible to explain things in a quantitative way. So the first question which a person would ask is that if after 20 years we cannot obtain anything which can be checked quantitatively by experiment, what is the point of working on this nonsense. The answer is that if the meson theory were something which was not of any practical interest and which did not have much to do with experiments, by this time people would have grown tired of it and left it alone. But we know that the problem of nuclear forces and other associated problems are such that they cannot be put aside. However bad

the theory may be and even if it takes a hundred years, I am sure that the various governments will be prepared to finance research in this line and people will go on working. Meson theory is one of the most important topics in physics, and therefore a lot of work is being done on this subject. On the other hand, most of the work which is being done is, I might say, second-rate contribution to increasing our real knowledge. Therefore, there are only a few things which I want to discuss, because I really do not know what else to do at present. There are many difficulties and I could enumerate the difficulties as most people do, but everyone knows them and so I shall discuss just a few things.

I am sure everyone has heard a great deal about the fixed extended-source meson theory of Chew and Low. They have concentrated their attention particularly on pion-nucleon scattering. In their papers they develop a non-relativistic pseudoscalar meson theory and they neglect the nucleon recoil, and then by introducing a high-energy cut-off they are able to explain the major features of the pion-nucleon scattering. Now this theory is very crude, and they do not claim that even at low energies can they explain the finer features. The importance of this work lies in the fact that there must be some truth in the calculations, because they do seem to agree reasonably well with the observations. But what is the real pseudoscalar meson theory?

First of all, it is obvious that the theory must be relativistic, because if a person starts with a basic theory which is non-relativistic, then he certainly is living in the nineteenth century. The one problem, which is extremely

interesting and on which many papers are written but is still unsolved, is whether from the relativistic pseudoscalar meson theory it is possible to derive this "static model" by some plausible arguments. I think it is generally agreed that at the moment it is not possible to do so. There is at the moment no rigorous derivation of the static model from the relativistic pseudoscalar meson theory, and I think it is a very important problem if somebody could do it.

I have confined myself so far to a few basic problems, which I would like to describe here. One of these problems is the problem of the nuclear potential, that is we have two nucleons and we want to calculate the nuclear potential between these two nucleons by using the pseudoscalar meson theory. I have a great distaste for anything that is not relativistic, unless one could justify the non-relativistic treatment by using the relativistic equations. This is a problem on which I must say I have spent a great deal of time, and what I have done is as follows:

If we use the relativistic pseudoscalar meson theory, then, as one knows, it is very doubtful that one could apply the perturbation theory to mesonic interactions. But, what could one do instead of using the perturbation theory? There are various things which have been suggested, but to me they seem very unclean and I am not greatly impressed by them. From a purely logical point of view, I do not find the intermediate coupling or the strong coupling very appealing. Therefore, one might ask whether by using the relativistic

pseudoscalar theory and the perturbation theory, one could obtain some kind of information about the nuclear potential. In this connection a very interesting argument was given by Taketani et al.[17] This argument, which is, of course, not a rigorous one, is that since the nuclear forces are short-range forces, one could probably apply the perturbation theory to obtain the nuclear potential except in the immediate neighborhood of the nucleons. Then one could introduce a phenomenological potential for short distances; and the distance which they have suggested is about one-half of the Compton wavelength of the pion. Up to this distance one could introduce a phenomenological potential, and beyond this one could perhaps apply the perturbation theory. According to the above argument, which might or might not be correct, it is tempting to use the perturbation theory to calculate the nuclear potential between two nucleons at distances that are larger than a certain distance. Now there are a very large number of papers on this topic, as you know. Probably the papers of Levy and Klein in the Physical Review describe the best work that has been done so far. In these papers, however, they have neglected the recoil of the nucleons, and this neglect of the recoil is something which is very strange. The situation really is that if one calculates this problem seriously, then the integrals that one obtains are exceedingly complicated. They involve μ/M, where μ and M are the rest masses of the meson and the nucleon, respectively. What people essentially do is to simplify the integrals by putting μ/M equal to zero. But, putting μ/M equal to zero amounts to assuming that the mass of the nucleon is infinitely large in comparison with the mass of the pion, which physically means that

we are neglecting the recoil of the nucleon. Thus, this phrase "neglecting the recoil of the nucleon" has been invented just to justify putting μ/M equal to zero in integrals, where there is no mathematical justification for putting it equal to zero. What I have been doing is that by using relativistic pseudoscalar meson theory, I have calculated the two-nucleon potential up to the fourth order as rigorously as is possible without neglecting recoil or anything else. Of course, this is very tedious work, and it obviously cannot be done without numerical integrations. The results I have obtained are considerably different from what Klein has obtained. Moreover, in Klein's paper the fourth-order nuclear potential does not involve the isotopic spin operators, but my result for the fourth-order potential does involve the isotopic spin operators. I shall submit this work for publication in the near future.

I have also carried out another calculation, and I might briefly describe it. It is well known that in low-energy processes we can take into account the effect of the anomalous magnetic moment of nucleons by adding to the Lagrangian density the interaction term

$$L_{eff} = -ie(\mu_o/4K)\, F_{\mu\nu}\, \bar{\psi}\, \gamma_\mu\, \gamma_\nu\, \psi\,, \qquad \text{(III. 1)}$$

where μ_o is equal to the static value of the anomalous magnetic moment. This is true, strictly speaking, only in the limit of photon energy tending to zero. But there are many processes in which the photons have quite appreciable energy. Then what happens in this case, and how far is it justified to introduce

a phenomenological term like (III. 1)? Of course, one thing which we can at once see is that this term cannot be used for photons of arbitrary energy, because it is well known that this type of interaction cannot be renormalized. In fact, if we use this term without any discrimination, we can show that at high energies, which are available in cosmic rays, the interaction between a photon and nucleon becomes extremely large. So at one time when I was looking into everything to see whether one could somehow explain the multiple photon production to account for the photon shower observed by Schein, I also looked into this problem. After carrying out some unpleasant calculations, I found that the anomalous magnetic moment of nucleons begins to decrease so rapidly with increasing photon energy that at high energies in cosmic rays the effect of the meson field on nucleon-photon interaction becomes absolutely negligible. In this way one can see that no photons associated with the additional interaction produced by the virtual meson field will be emitted by a proton or neutron in high-energy nuclear collisions. Of course, it will emit some photons sometimes because of the same interaction as in quantum electrodynamics. I have described this work in a paper in the Physical Review.[18]

IV. STRANGE PARTICLES

The work of Gell-Mann and Nishijima on strange particles is very well known, and their scheme involves the relation

$$Q = I_3 + \frac{1}{2} n + \frac{1}{2} S, \qquad \text{(IV. 1)}$$

where Q is the charge of the system in units of e, I_3 is the third component of the isotopic spin, n is the number of baryons minus the number of anti-baryons, and S is the strangeness number. After Gell-Mann and Nishijima, I believe that the most important work has been done by D'Espagnat and Prentki. They have published a number of papers, the most important of which is in Nuclear Physics.[19] This paper contains the mathematical formulation of the field theory of strange particles. It is really very beautiful, and I must say that it greatly increased my respect for the French.

They have given the field theory of the strong interaction of strange particles, and of course, this theory involves the same difficulty as the π meson theory, that is, the difficulty of large coupling constants. Otherwise it is as reasonable as the theory of pion interactions. They have written down the interaction Lagrangian density, and I don't want to write that down here. But I will only mention that the interaction of pions and baryons contains four coupling constants, and the interaction terms for K mesons and baryons contain four more coupling constants. So the most general form of the strong interaction of strange particles contains eight arbitrary coupling constants. I do not wish to say anything about the speculations on the various possible relations between these eight coupling constants. At present we don't know whether they are all equal, or some of them are equal, or what. But, there are several papers by Schwinger, Gell-Mann and Pais which contain speculations on the possible connections between these eight coupling constants.

D'Espagnet and Prentki have also suggested that instead of Eq. (IV.1)

it is perhaps a little better to take as the basic relation

$$Q = I_3 + \frac{1}{2} Y . \qquad \text{(IV. 2)}$$

Of course, Eq. (IV. 2) is the same thing as Eq. (IV. 1) because Y = n + S. This quantity Y has been given the name "hypercharge" by Schwinger. Now in recent years quite a few particles have been discovered. In fact, people who do not work in this line think that every day somebody or other comes up with some new particle and there are all kinds of mesons and hyperons. Then one begins to feel as if nature has gone crazy and everything has become completely messy. So why do we have all these particles? Of course, it may take some time before this mystery is completely cleared up, but there is one extremely interesting fact which I think one ought to know. If we assume that I, Q and Y in the relation (IV. 2) can take only the simple values

$$\begin{aligned} I &= 0, \ \frac{1}{2}, \ 1, \\ Q &= 0, \ \pm 1, \\ Y &= 0, \pm 1, \end{aligned} \qquad \text{(IV. 3)}$$

Then we obtain the following distinct mathematical possibilities for baryons and mesons:

	Baryons	
$I = 0$	$Y = 0$	Λ
$I = \frac{1}{2}$	$\begin{cases} Y = 1 \\ Y = -1 \end{cases}$	p, n,
$I = 1$	$Y = 0$	$\Sigma^+, \Sigma^0, \Sigma^-$

(IV. 4)

Mesons

$I = 0$	$Y = 0$	?
$I = \frac{1}{2}$	$Y = 1$	K^+, K^0
	$Y = -1$	$\overline{K}^0, K^-$
$I = 1$	$Y = 0$	π^+, π^0, π^-

In the above scheme, all the known baryons exactly correspond to all the distinct mathematical possibilities. Similarly, in the case of mesons, to each mathematical possibility there exists a meson, except that no meson has been observed which corresponds to $I = 0$, $Y = 0$ and $Q = 0$. Of course, you know that the muon is no longer regarded as a meson — it is now called a lepton. In the interaction of mesons and baryons, there are still several things which we don't know. First of all, we don't know the relation between the eight coupling constants. Then we do not know the spins of all of these particles, although I think there is a general feeling that unless there is some powerful experimental evidence to the contrary we should just assume that all the baryons have spin $\frac{1}{2}$, while all the mesons have spin 0. Another thing which we don't know is the parity of all these particles with respect to each other. At present one problem which is particularly being investigated is whether these K mesons behave as scalar or pseudoscalar particles in strong interactions.

However, the thing which seems to me most intriguing is this missing meson with $I = 0$, $Y = 0$ and $Q = 0$. I have spent considerable time on this

missing particle because I think it is certainly a very attractive idea that there exists another meson corresponding to $I = 0$, $Y = 0$ and $Q = 0$, which has not been observed so far. When I looked into various experimental papers and carried out some calculations, it seemed to me that the experimental work does not rule out the possibility of the existence of this particle. In fact, there is some evidence that this particle does exist. Of course, because this particle has not been observed, it is quite obvious that it is highly unstable. But the more I looked into this problem the more I was surprised to find that as far as we know the experimental evidence seems to favor the existence of this additional highly unstable particle rather than to rule out its existence.

One problem which I have investigated in particular is the problem of pion production in high-energy processes. This work is in the course of publication in the Physical Review, and in this I have examined the experimental papers on pion production in high-energy processes to see if they rule out or favor the existence of this missing particle. We know that this missing particle, which I have called the ρ^0 meson, is highly unstable, and it is neutral and identical with its anti-particle. There are also some arguments to show that it is scalar. Further, we can assume that its mass is greater than twice the pion mass, so that the ρ^0 meson rapidly disintegrates into pions. Now if such a particle does exist, then what can it do in these high-energy processes? It is quite obvious that what it can do are the following things: First of all, it will increase the apparent multiplicity of pions because in addition to π mesons often

this ρ^0 meson will be produced and it will decay into pions. This lifetome of the ρ^0 meson is so short that the whole thing will look like multiple pion production. Another thing the ρ^0 meson will lead to is that it will change the ratio of different modes of multiple meson production, because the ρ^0 meson can decay as

$$\rho^0 \rightarrow \pi^+ + \pi^- \text{ or } 2\pi^0 ,$$

but it cannot decay into

$$\pi^+ + \pi^0 \text{ or } \pi^- + \pi^0 .$$

Further, the ρ^0 meson will lead to some angular correlation between pion pairs, which are produced during apparent multiple pion production.

There are, in fact, a very large number of papers on pion production. Practically all this work has been carried out at Brookhaven or at the Radiation Laboratory in California, and a lot of data has been collected. I think the most important experimental work on this topic is by Shutt et al. I don't think I shall write down these various references because there is, for instance, a reference from California which contains 18 names. This is really becoming rather ridiculous, but these papers are very well known. In all these papers everybody has pointed out that in all observed processes the multiplicity of pions has been unexpectedly large. Thus, the ρ^0 meson will help in obtaining better agreement between the theoretical and the observed multiplicities.

Now let us consider the processes:

$$n + p \rightarrow n + p + \pi^+ + \pi^- \qquad \text{(IV.6)}$$

and

$$n + p \rightarrow p + p + \pi^- + \pi^0 . \qquad \text{(IV. 7)}$$

These are two processes which have been investigated by Shutt et al. According to Fermi's statistical theory, the cross sections for the above processes should have the ratio 3.3:1. There are, of course, also other theories of pion production such as the isobar models. By using some of these models the above ratio can be made a little larger but not too large. What do Shutt and collaborators find experimentally? They have described their work in a rather long paper,[20] and there are a number of tables in this paper. There is one table, Table 3, which gives the following information about "certain" and "probable" events of the type (IV. 6) and (IV. 7):

	$np\pi^+\pi^-$	$pp\pi^-\pi^0$
certain events	86	5
probable events	12	22

You can see for yourself that because they knew that Fermi's statistical theory gives the ratio 3.3:1 for the processes (IV. 6) and (IV. 7), they have been tempted to interpret most of the probable events as belonging to the category (IV. 7). This is like some theoretical physicists who try to adjust the theory to get the experimental results. Anyway, it is quite obvious that the ratio of the events of the type (IV. 6) and (IV. 7) is much larger than 3.3:1. If we have this ρ^0 meson then it will contribute fo (IV. 6) but it won't affect the mode of production (IV. 7).

Thus the ρ^0 meson will increase the theoretical ratio of the above processes. Shutt and his co-workers also find some angular correlation between the pion pairs in the process (IV. 6), which they are unable to explain by postulating a meson-meson interaction or some such thing. The ρ^0 meson will help in explaining the angular correlation between some of the pion pairs.

Of course, there are a large number of papers, but I have mentioned only one because it is more detailed than any other paper. There are many subsequent papers, and there is also the work of the California group on proton-antiproton annihilation where again they find that the multiplicity is very high. One can also investigate other consequences of the ρ^0 meson. I have investigated its effect on anomalous magnetic moments of the nucleons, and this particle does help in shifting the theoretical result toward the observed values. This work is also in the course of publication in the Physical Review.

Another problem which one of my graduate students is investigating is the effect of the strange particles and the ρ^0 meson on electron-neutron interaction. In fact, I might say a few words about this problem because considerable experimental work on electron-neutron interaction has been done here at Argonne. If we use the pion theory to calculate the interaction between electron and neutron, then we can show in general that what the pion field does is to produce an effective charge around the neutron and it also changes the magnetic moment of the neutron. Now it has been pointed out by Foldy that if we just consider the effect on electron-neutron interaction due to the static anomalous

magnetic moment of the neutron, which we know with very great accuracy, then the effective potential between the electron and the neutron is quite close to the experimentally observed electron-neutron potential at low energies. This means that the extended charge of the neutron is negligible, at least for low-energy electron-neutron scattering. But from the theory one finds that the extended charge of the neutron is quite large — far larger than any experimental error. So this is a problem which one does not know what to do about. There is also some uncertainty in the experimental work on this problem. At least I hope that at Argonne they will determine the experimental electron-neutron potential more accurately. I think from an experimental point of view this is a very important result, and if someone could determine it with very great accuracy I am sure it would become a standard result in nuclear physics. In this experiment the lower the energy of the neutron the better, and thus here one does not need high energies. Of course, you know that high-energy experiments of this type have been done at Stanford and they also find the same thing — that is, the extended charge of the neutron is unexpectedly small. We have been looking into this to see whether the ρ^0 meson and the strange particles might reduce the theoretical result for the extended charge of the neutron. But, so far whatever preliminary work has been done is certainly not very encouraging. I do not think I shall say any more about these strange particles. But, if there are any questions I shall be glad to answer them.

Hamermesh: Would you say something about the mechanism by which the ρ^0 meson and the strange particles might reduce the extended

charge of the neutron?

Gupta: The diagrams are quite similar to those for π mesons, and I calculated it very crudely and it didn't reduce the extended charge. I suppose it's a nice exercise to work out the thing in complete detail. But I have very little hope that it will work. So this is a problem for theoreticians as well as for experimental physicists because one should know at least the sign of what little extended charge the neutron has, whether it is positive or negative. I have been completely confused by the various values given in the experimental papers. I think more work should be done on this problem.

V. GENERAL PRINCIPLES OF QUANTUM FIELD THEORY

Now I shall say a few words about the general field theory. There is a problem in the formulation of quantum field theory on which I have been working for three or four years. In fact, I have never been satisfied with what is usually done in textbooks, and I have always been very worried about this problem concerning the basic Lagrangian formulation of quantum field theory. What is usually done is to follow the work of Heisenberg and Pauli. In their treatment, which was given around 1930, the Lagrangian density was treated as a classical quantity. Then they carried out various variations and obtained various results, and at a later stage they passed over from c-number quantities to q-number quantities. First of all, it is obvious that this is a very artificial

procedure in which first one treats something as a classical quantity and later on one says that it is an operator. So the really rigorous formulation of field theory should be based on a Lagrangian density which is a function of operators, that is, the field quantities should be treated as operators from the very beginning. The Heisenberg-Pauli treatment is not only artificial but when we pass over from the c-number quantities to the q-number quantities the question arises, "In which order should we arrange those quantities?" Since 1930, of course, many papers have been written on this subject, and probably you are familiar with the series of papers by Schwinger – there are about half a dozen of them. He starts with a Lagrangian density which is a function of operators, and essentially what he does is to manipulate various things, arrange various orders of operators and work out the variations, and then the whole treatment becomes extremely complicated. I have been working on this problem for several years. In fact, since I am writing a book on the quantum theory of fields, this problem was really a headache for me because unless this thing is cleared up one cannot write a systematic account of field theory. I have published a paper in the Physical Review [21] on this subject in which I believe that I have solved this problem. The resulting formalism is not very difficult, and there are a number of things which come out of it. In fact, there are a number of things which are so beautiful that I definitely feel I am on the right track. I won't describe the mathematical details but I shall just mention what I have done.

I started from a Lagrangian density which is an ordered product, that is,

$$: L : = : L(u^{(r)}, \partial u^{(r)}/\partial x_{\mu}) : . \qquad (V.1)$$

If there were no interaction it would be easy to define an ordered product as Wick has done. But we are starting from a general Lagrangian density which contains interaction, and there one cannot split in a Lorentz covariant way all the field operators into positive and negative frequency parts. Therefore, there is no simple way in which one could define the ordered product in (V.1) because one does not even know the commutation relations in the very beginning of the formalism. The way I avoided this difficulty is described in the paper. What I did was that first I postulated a number of properties of the ordered product and with the help of those properties it is possible to work out the whole formalism. Then one passes over from the Heisenberg representation to the interaction representation, and in the interaction representation one identifies these ordered products with those defined by Wick. When one has done all this, the resulting formalism is completely free from any ambiguity, and one never uses any c-numbers anywhere. Moreover, in this formalism the zero-point energy of the fields automatically vanishes, and the zero-point charge also vanishes. Thus, in this formalism everything becomes simpler, and there is no need to introduce the charge-conjugate operators because charge conjugation is ensured automatically in a more compact form. In fact, in the usual charge conjugation procedure the zero-point charge of the original field

is destroyed by the zero-point charge of the charge-conjugate field. But this does not change the zero-point energy because even when we reverse the roles of electrons and positrons the zero-point energy remains the same. Therefore, charge conjugation can remove only the zero-point charge, but the present formalism removes the zero-point charge and also the zero-point energy. Another thing which comes out is that in this formalism all the interaction terms are ordered products, and it follows from a theorem due to Wick that we must always ignore all those diagrams in which lines from the same vertex are joined to each other. Some people call them "tadpole"-type diagrams. This treatment shows that whatever the field may be, we must ignore the tadpole-type diagrams. This information is particularly useful for non-linear interactions. I must say I am very proud of this work because I think this thing was never done properly before.

Another topic about which I might make some comments are particles of spin higher than 1. One question which has often been asked is, "Can particles of spin higher than 1 exist in nature?" As you probably know, there are various theories of these particles of higher spins. In some the rest mass has more than one value, and some contain particles of both negative and positive energies. But, I think the most sensible work on this subject is due to Fierz and Pauli. When I was looking into these theories of particles of large spin, I was told that Fierz believed that these fields of large spin probably could not be quantized in a physically meaningful way. In fact, Fierz and Pauli have given the Lagrangian formalism and they have given the field equations, but they did not quantize the field. Therefore, partly because there has been so much talk that

some of these strange particles in cosmic rays might have large spins, and partly because of mathematical curiosity I spent some time on the quantization of a field of spin 3/2 according to the Fierz-Pauli work. In this paper, which I published in the Physical Review,[22] what I find is this: If we develop the theory of spin 3/2, we can do everything starting with the Lagrangian formalism. We can quantize the field, obtain the commutation relations, and write down the contributions of various diagrams. The only difference is that the matrices involved are very complicated compared with the Dirac matrices. They are 16×16 matrices for spin 3/2, but there is no difficulty of negative energy, and it is not necessary to use any supplementary conditions. The theory is, of course, tougher than that for particles of spin 1/2, but that does not mean that such particles should not exist. On the other hand, I investigated whether the electromagnetic interaction of particles of spin 3/2 can be renormalized, and I found that the divergencies are very, very bad in this case. So, as one goes to higher spins the divergencies become worse and worse, and if one very strongly believes in the renormalization theory then one could rule out these particles. But then we know that according to Einstein's theory of gravitation the gravitons have spin 2. Therefore, what I would say finally is that one cannot give any clear argument to show whether these particles should or should not exist in nature. But one thing is certain — if somebody discovers a particle of spin 3/2. then it will be a most unpleasant task to carry out calculations for such a particle. I shall certainly feel very unhappy because these matrices which

appear in the theory of spin 3/2 are extremely difficult to handle. Maybe then people will have to write theoretical papers in which there will be 18 names or so just as they do in experimental papers from California.

VI. FUTURE OUTLOOK

Finally I want to say a few things about the future outlook. It is very difficult to say, of course, what we can expect in the future in this field, but I think there are a few things which seem fairly clear.

Some people have compared the present situation to the situation which existed when quantum mechanics was being developed. I do not think it is a fair comparison because at the moment we do understand most things in a qualitative way. But when quantum mechanics was being formulated one just could not explain even in a qualitative way how, for instance, one could have a stable hydrogen atom within the framework of classical mechanics. At present I don't think there are many mysteries which are completely obscure and where one does not even know what is happening. I think we have a reasonable idea of what is happening in various processes, but it is the quantitative treatment that is still lacking. This trouble is largely caused by strong interactions, and I have very little hope that anybody can really give some method of calculation which can be applied reasonably well to strong interactions. To me it seems that one can expect some more real progress in quantum electrodynamics or weak interactions. But in strong interactions I have the feeling that people will resort more and more to various models, which is, of course, not very

satisfying from an intellectual point of view.

One trouble these days is that there are various laboratories which have very expensive apparatus and have to carry out some observations, and in fact one can learn about unknown things only in this way. Then they want theoreticians to do something on what they are doing. So the situation seems to be that many experimental people who have tremendous amounts of money at their disposal expect the theoretical people to give some theory whether it makes any sense or not from a logical point of view. They want something to compare their results with — statistical model, isobar model, static model, or some other model. This tendency is bound to increase more and more, and fewer and fewer physicists will work on the fundamental problems. The universities and research institutes will become like industrial establishments in which more and more papers will describe the joint works of a large number of authors. Another thing which is definitely happening in the United States is that there is a growing feeling in the minds of the general public that they must try their best to maintain their supremacy in the scientific field. So I suppose people will be prepared to spend more and more money for scientific research, and I have no doubt that the salaries of scientists are bound to increase all over the United States. Therefore, perhaps one can say that in the future the life of a physicist will be very comfortable and very boring.

Hamermesh: I would like to thank Professor Gupta for a very interesting series of lectures.

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Naive Inquiries into Field Theory

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Lecture 1.

Since the most successful field theory is undoubtedly quantum electrodynamics, it seems sensible to study the successes and failures of that theory before attempting any wholesale extrapolation of the theory into a different domain. The procedure in quantum electrodynamics is well known and only a very brief sketch will be given, just enough to phrase some questions which might be pertinent.

a) INTRODUCTION

One always starts out from field equations derived from a Lagrangian or Hamiltonian. One may separate $L = L_1 + L_2 + L_{int.}$, where L_1 and L_2 are free (non-interacting fields), and L_{int} is the interaction term. Objectionable as this procedure may be from a mathematical standpoint, it should be stressed that nonetheless the existence and form of the coupling term is definitely demanded by experiment; for instance, $(\vec{j} \cdot \vec{A})$.

The free fields are described in terms of field operators. These operators satisfy certain field equations, the Dirac and Maxwell equations. Since these field operators are operators, they are to act on certain objects. The state of the system also must be expressed in a certain manner. In nonrelativistic quantum theory, the states correspond to vectors in a Hilbert space. This

Hilbert space has a (countable) base $\{\phi_k\}$. Any physical state can be expressed as a combination of ϕ's by the equation

$$|\psi\rangle = \sum_k c_k |\phi_k\rangle ,$$

where $|\psi\rangle$ is an element of $\mathcal{H}$.

Now when dealing with a field theory, one deals with a system with an infinite number of degrees of freedom, e.g., a free radiation field may be thought of as a collection of an infinite set of harmonic oscillators. In quantum theory, the states of each one of these oscillators would be described in a Hilbert space. Take the direct product of all these Hilbert spaces. This again is a Hilbert space. The state of the system is now determined by a vector in that Hilbert space, called D. Physically D is very reasonable; the basic states indicate the number of particles in state 1, state s, ... Hence a state is described in terms of an infinite sequence $\{0, 1, \ldots 0, s \ldots\}$. The number of such infinite sequences is non-denumerable (it would include all real numbers, for instance) — this is a non-separable Hilbert space, many frequently used mathematical theorems are not true. It is possible for the case of non-interacting fields to describe the state in a separable Hilbert space F, the Fock space. Let $\mathcal{H}$ as before be a Hilbert space. Now consider a state $\psi(x)$ in $\mathcal{H}$. Now construct $\mathcal{H} \otimes \mathcal{H}$, the tensor product of $\mathcal{H}$ and $\mathcal{H}$. This is a space whose elements are two-particle states, the operators acting in $\mathcal{H}^{(2)}$ are $H_1 + H_2$. In this manner construct a space $\mathcal{H}^{(n)} = \mathcal{H} \otimes \mathcal{H} \otimes \ldots \otimes \mathcal{H}$.

If $\{\phi_k\}$ are a base in $\mathcal{H}$, then the sequences $\{\phi_{k_1}\}$, $\{\phi_{k_1}, \phi_{k_2}\}, \cdots \{\phi_{k_1} \cdots \phi_{k_n}\}$ (finite n) are a base in $\mathcal{H}^{(n)}$.

Now form the sum space ($\oplus$ symbol, not the symbol $\otimes$ that is used for a product) by

$$F = \mathcal{H}^{(1)} \oplus \mathcal{H}^{(2)} \oplus \cdots \oplus \mathcal{H}^{n} \oplus \cdots .$$

The spaces in the direct sum are orthogonal to one another, an element in $\mathcal{H}^{(2)}$ has nothing to do with an element in $\mathcal{H}^{(n)}$. An element of F consists of a collection; an element in $\mathcal{H}^{(1)}$, one in $\mathcal{H}^{(2)}$, $\cdots$, etc., or written symbolically an element of F is

$$\begin{pmatrix} 1 \\ \psi(x_1) \\ \psi(x_1)\ \psi(x_2) \\ \cdot \\ \cdot \\ \cdot \end{pmatrix} .$$

The basis vectors would be

$$\begin{pmatrix} 1 \\ 0 \\ \cdot \\ \cdot \\ \cdot \\ \cdot \end{pmatrix} \qquad \begin{pmatrix} 0 \\ \psi\ (x) \\ \cdot \\ \cdot \\ \cdot \end{pmatrix} \qquad \begin{pmatrix} 0 \\ \cdot \\ 0 \\ \cdot \\ \psi_n(x_1,\ x_2 \cdots x_n) \end{pmatrix}$$

The Fock space F has a countable basis. Unfortunately, there appears to be no simple way to utilize the description of the Fock space to describe interacting systems. For point interactions one can not maintain a description in the Fock spaces. Specifically as shown by Cook[1] if the free fields are represented by elements in Hilbert space F_1 and F_2 respectively, the interaction operator H_{int} is an operator on $F_1 \otimes F_2$. One now proves that if the equation

$$H_\Lambda = \int f_\Lambda(x_1\ x_2)\, H(x_1, x_2)\, dx_1\ dx_2$$

(where f is a form factor and Λ^{-1} indicates the range of the non-local interaction) has a limit, say H_∞, then there is in fact no interaction so $H_\infty = 0$. The proof indicates 1) either the space $F_1 \otimes F_2$ is too small for the description of interacting fields (it is included in D), or 2) the limit H_∞ does not exist, or 3) one should not take the limit in the first place. (This type of result also has been obtained by Landau et al.[2]) In spite of this questionable start, let us now consider formal quantum electrodynamics, and later return to the problematic aspects of the subject.

b) FORMAL THEORY[3]

One starts from an interaction Hamiltonian $(j_\mu\ A_\mu) = H$. The temporal development of a system of electrons and photons is described by an operator S which describes the behavior from $\tau = -\infty$ to $\tau = +\infty$. Actually the precise mathematical theory of the S operator is also not known. It must be defined in a limiting sense, because a transition operator $U(\tau_1, \tau_2)$ for finite times

may not exist; this adds to the difficulties. U satisfies the equation of motion

$$i \frac{dU}{d\tau} = H(\tau_1\ \tau_0)\ U(\tau_1\ \tau_0) \tag{1}$$

or

$$U(\tau_1\ \tau_0) = 1 - i \int_{\tau_0}^{\tau} d\tau'\ H(\tau_1' \tau_0)\ U(\tau_1'\ \tau_0) \tag{1a}$$

Put $\tau_0 = -\infty$, then $U(\tau) \equiv U(\tau_1\ -\infty)$ so

$$U = \sum U_n\ ;\ U_n = -i \int_{-\infty}^{\tau} H(\tau')\ U_{n-}\ (\tau')\ d\tau' .$$

Define $S = \lim_{\tau \to -\infty} U(\tau)$ so

$$S = \sum_{n=0}^{\infty} S^{(n)} \tag{2}$$

where

$$S^{(n)} = \frac{(-i)^n}{n!} \int_{-\infty}^{+\infty} \cdots \int d\tau_n \cdots d\tau_1\ P\{H(\tau_n) \cdots H(\tau_1)\}$$

$$H = i e \bar{\psi}\ \gamma^{\mu}\ \psi\ A_{\mu}(x) . \tag{3}$$

The time-ordered product P can now be expressed in terms of Wick normal products and commutators. The commutators always depend on (in fact are) the Green's functions of the free-field equations. We will need the Fourier transforms of these functions (usually called propagators):

Photon field: $$D(k) = \frac{1}{k^2} \qquad (4a)$$

Electron field: $$S(p) = \frac{i\not{p} - m}{p^2 + m^2} \qquad \not{p} \equiv p_\mu \gamma^\mu \ . \qquad (4b)$$

The diagrams below represent the iteration series in a graphical form. [Recall one integrates over an internal line $\int d^4 k$ or $\int d^4 p$; and each such line contributes an integrand D(k) or S(p).] The point is now that in the iteration solution one has several kinds of divergencies. Some can be seen by inspection. As an example, consider the vacuum fluctuation diagram shown in Fig. 1.

Fig. 1. Vacuum fluctuation.

For this situation,

$$\sim \int d^4 p \int d^4 p' \int d^4 k S(p) S(p') D(k) = \int dp \int dp' \int dk \, (pp')^3 k^3 \frac{1}{pp'k^2} \ .$$

This expression diverges; such disconnected diagrams are always left out.

There are other divergencies in the iteration solution. One of these is represented graphically by the "closed loop diagrams". An appeal to invariance considerations (gauge invariance, for example) allows one to omit these graphs. Another type divergence is the infrared divergence. If an electron is scattered

in an outside field (see Fig. 2), the same process but with a photon emitted (Fig. 3) diverges as $\omega \to 0$.

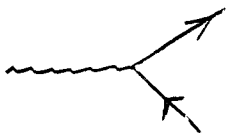


Fig. 2. Electron scattered in an outside field.

Fig. 3. Same as in Fig. 2 except that a photon is emitted.

This type of divergence, analyzed in great detail by Jauch & Rohrlich,[3] and also by Brown & Feynman[4] will be discussed a little later.

None of these omissions appear to have observable consequences. However there are experimental consequences resulting from other divergent diagrams, namely the electron self energy, the vacuum polarization, and the vertex parts (Fig. 4).

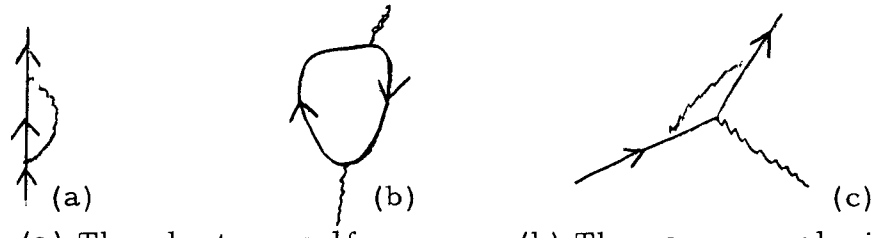


Fig. 4. (a) The electron self energy. (b) The vacuum polarization. (c) The vertex parts.

The way in which one treats these divergencies is also well known. One first formally calculates the effect of a self-energy insertion in an internal line.

This replaces S(p) by S'(p), where

$$S'(p) = S(p) + S(p)\,\Sigma(p)\,S(p) \tag{5a}$$

$$\Sigma(p) = \frac{i e^2}{(2\pi)^4} \int d^4 k \gamma_\nu \frac{S(p-k)}{k^2} \gamma^\nu \,. \tag{5b}$$

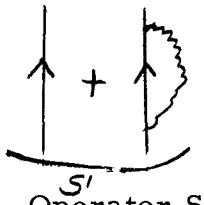


Fig. 5. Operator S' is the sum of two terms represented by diagrams.

The S-operator is altered by $S^{(2)} = -i \int \phi(p)\,\Sigma(p)\,\phi(p)\,d^4 p$. The insertions in external lines may be computed similarly. Now the point is simply that $\Sigma(p)$ [given by Eq. (5b)] is divergent. Divergent integrals also occur when one inserts a vacuum polarization term in a photon line. In the latter case

$$D'(k) = D(k) + D(k)\,\Pi(k)\,D(k).$$

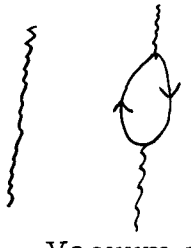


Fig. 6. Vacuum polarization term inserted in a photon line.

These integrals always diverge as k, $p \to \infty$ (sometimes also at $k \to 0$).

Now we formally manipulate these expressions to obtain them in the form

$$\Sigma(p) = A + (i\not{p} + m)\, B + (i\not{p} + m)^2\, \Sigma_f \tag{6}$$

$$\Pi(k^2) = k^2 (-C + k^2\, \Pi_f(k^2)). \tag{7}$$

Here A and B and C are divergent entities of the form

$$A = \frac{3\alpha}{2\pi}\, m \left(\log \frac{M}{m} + \frac{1}{4}\right) \qquad M \to \infty$$

$$B = \frac{\alpha}{4\pi} \left(D - 4 \int_0^1 \frac{dx}{x} + \frac{11}{2}\right) \qquad D \sim 2 \log \frac{M}{m} - 1 .$$

An infrared divergence appears here but Σ_f and Π_f are finite.

The remarkable feature is now that these divergent constants always occur in such a manner as to combine with the mass m, the charge e, or the normalization constant of the wave functions. "They know their proper place in the formalism." One sees from Eq. (6) for example that $S^{(2)}$ has (for free particles) the form

$$S^{(2)} = -iA \int \bar{\phi}(p)\, \phi(p)\, d^4 p = -iA \int \bar{\psi}(x)\, \psi(x)\, d^4 x .$$

Since A is of order α, this is exactly the correction to S (in first order) of adding a term $\delta m \int \psi(x)\, \psi(x)$ to the Hamiltonian. Hence the diagram of Fig. 4(a) only alters the mass of a free electron. The infinite constant is combined with m in the form $m + A = m_{exp}$, the experimental mass (which is finite). The symbol m as used before was thus meaningless, giving the whole development a somewhat ethereal character, although the final answer contains

the experimental mass and charge only and no arbitrary constants. The consistency of the whole procedure — it works in all orders, it maintains the unitary character of S, it retains the invariance properties of S — all have been formally checked.

Suppose one has a diagram with n corners, E_i internal electron lines, P_i internal photon lines, E_{ext} electron lines, and P_{ext} photon lines. Then in the integration one has n δ-functions giving (n-1) relations between the integration variables. The number ρ of independent 4-vectors is

$$\rho = E_i + P_i - N + 1 . \tag{8}$$

There are thus 4ρ integrations to be performed. A primitively divergent graph is one which is divergent but becomes convergent if one integration over a 4-vector is omitted (or it is convergent if one 4-vector is kept fixed).

By counting the number of integrations, using the explicit form of S and D, one can easily decide whether an integral diverges or converges. Define the degree of divergence by

$$k = E_i + 2 P_i - 4n + 4 . \tag{9}$$

If $k = 0, 1, 2, \cdots$, one has a logarithmic, etc., divergence. If k is negative, one has a convergent integral. The crucial point is now that k is independent of n, because

$$k = 4 - \frac{3}{2} (E_e + P_e) . \tag{10}$$

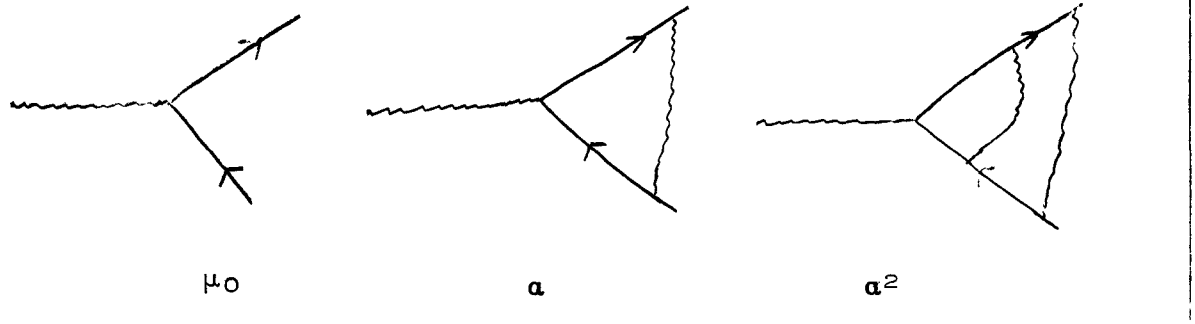
Thus, since k is to be positive for a divergent graph, the number of divergent

graphs is finite. This means the theory is renormalizable. One must properly separate out all infinite terms, identify e + B and m + A with the observed mass and charge; then the remaining finite terms should be observable.

c) COMPARISON WITH EXPERIMENTS

It is clear from the graphs that there should be corrections of order α and also α^2, etc., to the magnetic moment of the electron. After initially not quite accurate experiments --- and (fortunately) equally inaccurate calculations – the theory and experiment now appear to check to order α^2.

1. Contribution to the anomalous magnetic moment of the electron. –



μ_0 α α^2

Fig. 7. The first three contributions to the magnetic moment.

$$\mu_{th} = (1 + \frac{\alpha}{2\pi} - 2.973\ \frac{\alpha^2}{\pi^2})\,\mu_0 = 1.0011454\,\mu_0 \qquad \text{Karpus \& Kroll}$$

$$\mu_{exp} = (1.001146 \pm 0.0000\ 12) \qquad \text{Koenig-Prodall \& Kusch}$$

$$\mu_{exp} = (1.001167 \pm 0.000005) \qquad \text{Harvard}$$

$$\mu_{th} = (1.0011596) \qquad \text{Summerfield}$$

2. Lamb shift; observed $^2P_{1/2} - {}^2S_{1/2}$ level separation in H and D. - The diagrams are of 2 kinds. The electrons are now of course bound electrons.

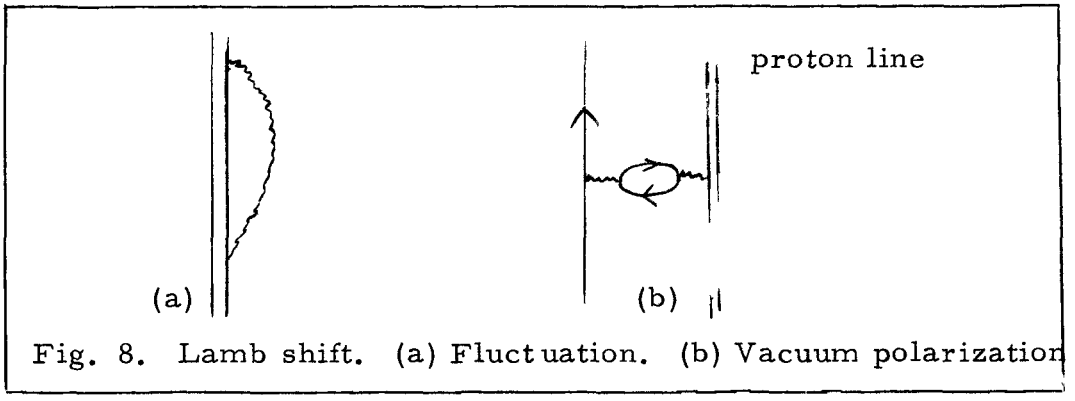


Fig. 8. Lamb shift. (a) Fluctuation. (b) Vacuum polarization

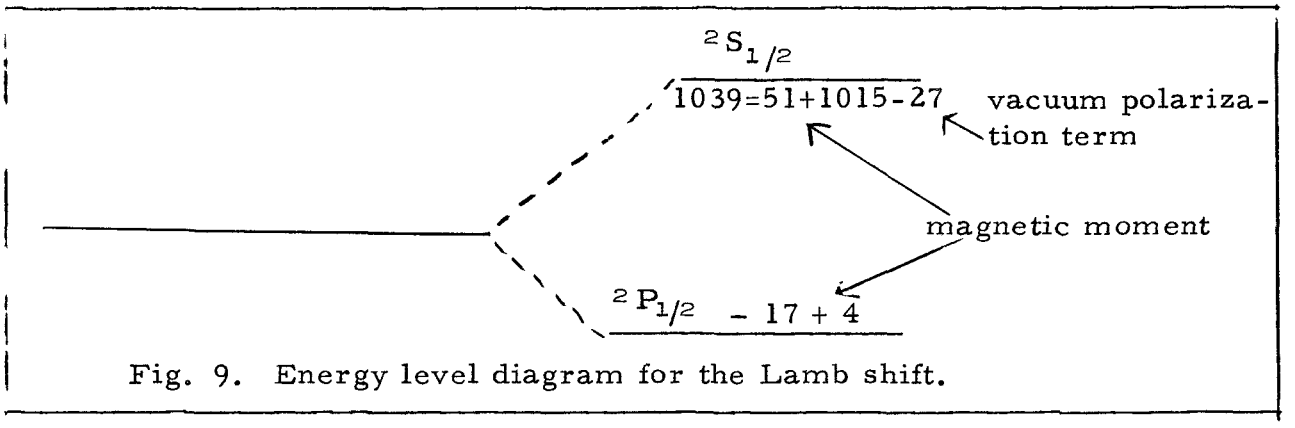


Fig. 9. Energy level diagram for the Lamb shift.

$$\Delta E_{th} = 1052.14 \pm 0.08 \quad (Mc/sec) \qquad (\alpha)$$

$$= 1057.19 \pm 0.2 \quad (Mc/sec) \qquad \alpha^2$$

$$\Delta E_{exp} = 1057.77 \pm 0.1 \quad (Mc/sec)$$

The actual calculation of the Lamb shift is quite nasty, there are several delicate computational questions to be analyzed, such as the kind of intermediate states to be used in the perturbation calculation. Still the best calculation differs

from the experimental value by only 0.5 of a megacycle. This difference has not yet been explained.

3. Radiative correction to the hyperfine structure separation . — One finds radiative corrections to the energy levels of the electron in the field of nuclear spin. The expression is

$$\delta E = -\frac{4}{3}\,\mu_e\,(\mu_p \cdot S)\,\frac{m^2}{\pi}\,(Z\alpha)^3\,\Big[\,1+\frac{\alpha}{2\pi}-\frac{32\,z\alpha^2}{\pi^2}-Z\alpha^2\,(\frac{5}{2}-\log 2)\Big]\,\Big[\,1+\frac{3}{2}Z\Big] \tag{11}$$

proton moment — magnetic moment correction

One actually uses Eq. (11) to calculate α. One also may calculate the ratio R_{exp} of these corrections for 2s and 1s levels. One finds

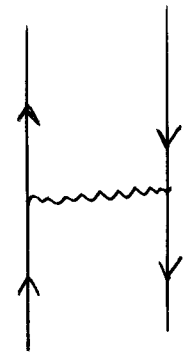
$$R_{exp} = \frac{\delta E(2s)}{\delta E(1s)} = \begin{cases} \frac{1}{8}\,[\,1.000\;\;034\;6 \pm 0.000\;000\;3] & \text{for H} \\ \frac{1}{8}\,[\,1.000\;\;034\;2 \pm 0.000\;000\;3] & \text{for D} \end{cases}$$

Within experimental error, the values are the same for H and D. On the other hand

$$R_{th} = \begin{cases} \frac{1}{8}\,(1.000\;033\;3) & \text{to } \alpha^2 \\ \frac{1}{8}\,(1.000\;035\;4) & \text{to } \alpha^3 \end{cases}$$

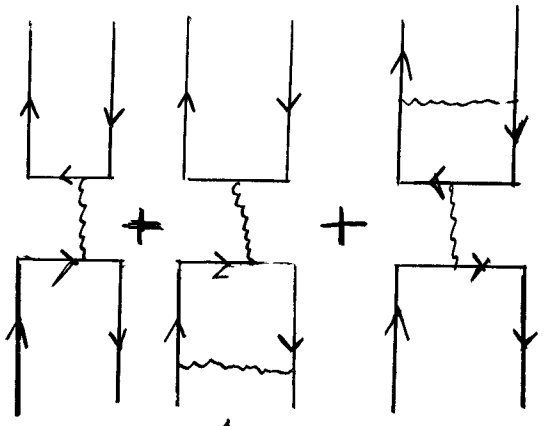
This question is not yet settled. Again there is a small unexplained difference between theory and experiment.

4. Positronium.[5] — Here one wants to calculate the radiative effects on the ^3S-^1S level splitting; so one needs the radiative corrections which affect the



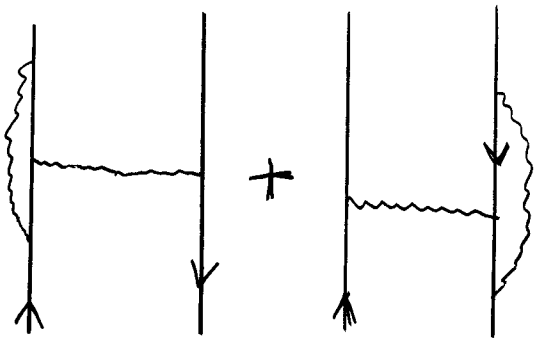
$\Delta W_1 = \alpha^4 \frac{1}{3} mc^2$

(a) One-quantum exchange



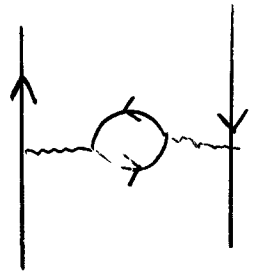
$\Delta W_2 = \alpha^4 mc^2 \left(\frac{1}{4} - \frac{\alpha}{\pi}\right)$

(b) One-quantum annihilation.



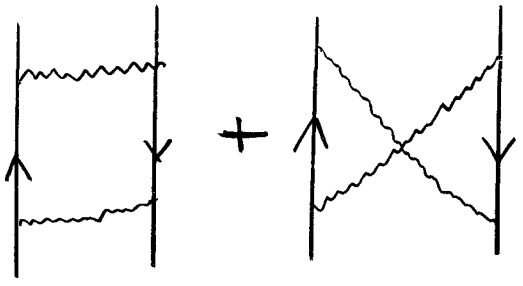
$\Delta W_3 = \alpha^4 mc^2 \frac{\alpha}{3\pi}$

(c) Self energy and magnetic moment.



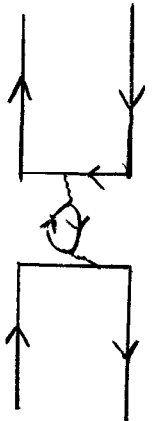
$\Delta W_4 = 0$

(d) Vacuum polarization



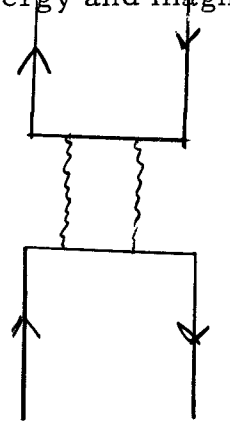
$\Delta W_5 = \alpha^4 mc^2 \frac{\alpha}{2\pi}$

(e) Two-photon exchange.



$\Delta W_6 = \alpha^4 mc^2 \left(\frac{-2\alpha}{9\pi}\right)$

(f) One-quantum annihilation and vacuum polarization.



(lifetime)

$\Delta W_7 = \alpha^5 mc^2 \left(1 - \log 2 + \frac{i\pi}{2}\right)$

(g) Two-quantum annihilation.

Fig. 10. Processes contributing to the $^3S - {}^1S$ level splitting. The net effect is

$$\Delta W_{th} = \begin{matrix} 2.0 & 44 \times 10^5 \text{ Mc/sec} & \alpha^4 \\ 2.0 & 337 \times 10^5 \text{ Mc/sec} & \alpha^5 \end{matrix}$$

$$\Delta W_{exp} = (2.0\ \ 335 \pm 0.0\ \ 003) \times 10^5 \text{ Mc/sec}$$

^1S state differently from the ^3S state. The diagrams in Fig. 10 all give such contributions. The energy differences are what is written down. (One should remember that a factor α^2 comes from the Rydberg constant in the energy expression.) The agreement between theory (including many terms, all of comparable magnitude) and experiment, is certainly impressive.

5. The p-p scattering. — The scattering cross section $\sigma(\theta)$ for p-p scattering has been measured with accuracy up to 0.2% in a range from 0.8 to 2 Mev. These data have been compared with theoretical calculations. Using a) nuclear s wave scattering, b) the Coulomb phase shifts (all), and c)[6] the vacuum-polarization potential given by V_p in the expression

$$V(r) = \frac{e_1 e_2}{r}\left[1 + \frac{2\alpha}{3\pi}\int_1^\infty d\xi\; e^{-2k\xi r}\left(1+\frac{1}{2\xi^2}\right)\left(\frac{(\xi^2-1)^{1/2}}{\xi^2}\right)\right] = V_{coulomb} + V_p.$$

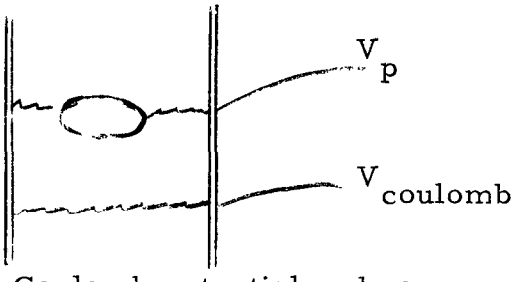


Fig. 11. The Coulomb potential and vacuum-polarization potential.

The potential V_p produces effects of 0.7 — 0.5% in $\sigma(\theta)$. This now fits the data within 0.2%, without this correction there are definite, angle-dependent deviations between the theory and experiment. Thus the use of V_p from the vacuum polarization certainly improves the fit. This again argues strongly for the reality of radiative effects.

Lecture 2.

d) QUESTIONS ABOUT QUANTUM ELECTRODYNAMICS

Today I would like to formulate some questions and perhaps suggest some possible courses of action which one might take in connection with problems of field theory. I reviewed last time some of the features of quantum electrodynamics and I will not do that again, except to remind you of the remarkable agreement which exists between theory and experiment. Once one has somehow gotten used to the fact that in the theory certain infinite entities appear which have to be treated in a particular way, they no longer cause trouble.

Let me now state a few questions. They are not in any particular order, but are more or less in the order in which I thought of them. I should perhaps say this, that I have attempted to collect all the questions which are of mathematical character, so they will be coming in a little while. Also I would like to point out that if you have any questions, naive or not, not only I but Hans and everybody would be pleased to have these incorporated so that we get here a representative cross section of what at least some naive people think of quantum electrodynamics.

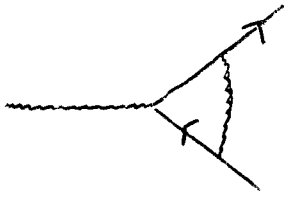


Fig. 12. Scattering of an electron in an outside field.

The first question, a naive one, is the following: If you look at integrals which diverge in quantum electrodynamics, you will find universally that what we call the the "serious divergencies" always occur for large values of the integration variable.

Let me remind you that these integrations always occur because one performs integrations over the momentum variables of the internal lines. So if I have a particular kind of a diagram (say a scattering) of an electron in an outside field, and I put a radiative reaction in there, I have to integrate over the momentum of this photon, and it is this integration which diverges at the upper end of the limits of integration. This is fairly universally the case. There are divergencies which occur at zero. (I will say something about them later.) Now, you might in fact say: Well look, if these divergencies occur for large values of the integration variable (for large momenta which is the same as large energies), why do we not just say that quantum electrodynamics does not apply when dealing with high energies? After all it is in principle possible to create electromagnetically a mu-meson if I have an energetic enough photon. Near a nucleus, the law of conservation energy allows the creation of a meson pair. This clearly cannot be a consequence of the theory so far developed. Consequently, what's all the fuss about? Why don't I just cut off at a value which corresponds to an energy which is the rest energy of the mu-meson, and call it a day? So let me just say "cut-off".

I would like to hear some discussion about this. You can, of course, make such a cut-off in a relativistically invariant way. You have your choice of form factors so that integrals look nice and invariant. The renormalization scheme is then only necessary because quantum electrodynamics is not a closed theory. Other particles have electrical charge, they will be created, and that is outside quantum electrodynamics. All the divergent integrals will be finite

and all you have to worry about are certain integrals which diverge at the zero value of the integration variables, but these are always typically different in character and can normally be removed. Then you accept that quantum electrodynamics takes into account part of the electrodynamic phenomena, but not all. The mesons are outside of it and we have to do as well as we can with meson theory starting from scratch. I wonder if any of you have any objections in principle or in practice on this general matter.

Moldauer: Is it so difficult to do electrodynamics with mu-mesons – a theory with two types of particles? Asymptotically you only have electrons?

Dresden: No, I believe not. But the point is that I do not get it out of the original postulate of the theory. If I write down quantum electrodynamics the way we have done it, the existence of the mu-meson does not follow.

Sunakawa: I think that if one puts in a cut-off the result is not gauge invariant. This was demonstrated by us to fourth order using a perturbation calculation.

Dresden: I can certainly introduce quantum electrodynamics with a finite mass for the photon, in which case it is not gauge invariant anyhow – I can make a cut-off here. Now I will define the limiting procedure, in which I have two limits to take. I have to let the photon mass go to zero, I have to let the cut-off go to infinity, and I can do it in such a way that I end up with what we normally have.

May I add one of my objections? I want to point out that even with a cut-off it must be remembered that one still must renormalize because the renormalization tells you something about the fact that masses with which you deal are not the masses which you originally thought you had. What a cut-off will do is not to remove renormalization, but only to remove an infinite renormalization. And this particular feature of quantum electrodynamics (which to me is one of the ugliest) is that the entities initially occurring in your equations, say in the free-particle equation, are not the same entities which occur once you have coupled this equation. This I think is a very crucial feature, that in any theory in which you have fields interacting with other fields one has to renormalize. To give you a very simple example, if I have an electron which interacts not with electromagnetic waves but with lattice waves, I have to renormalize the electron mass in that case also. That happens to be a finite renormalization rather than an infinite one. So I would say that the cut-off theory, although not manifestly terrible, is still not satisfactory. Suppose I want to construct a field theory, not of electrons and photons but a field theory of π mesons. We pattern that after the field theory we have of electrons or photons. Now if a cut-off is an essential feature of that theory then we would have to cut the π meson theory off somewhere also, say at the mass of the K meson. I think that is a very unsatisfactory situation — that each theory would have to be cut off in this particular manner. I want to stress this point

particularly — that even though field theory, so to say, has been born by imitating electrodynamics, this may very well have been a completely misdirected guess. I don't think it is likely, but it can be. It is not at all evident a priori that other field theories must be formally or physically similar to electrodynamics.

Ekstein: I believe the most obvious objection against this as a physical theory is that it is not deductive. If one wants to make up a number of recipes as far as they are successful, there are no objections against the recipe with or without infinite cut-off. When you write down a cut-off are you talking about a deductive procedure or a set of recipes?

Dresden: Quantum electrodynamics does not predict the existence of mu-mesons — there the theory is false or incomplete. So this is my argument for saying that one might cut off at the mu-meson mass.

Sunakawa: If we calculate the S matrix with cut-off in 4th order, one can not keep both an invariant form factor and gauge invariance.

Dresden: This is a different point although related to something I will discuss later, namely one can not construct a non-local theory satisfying these requirements: a) relativistic invariance, b) gauge invariance, c) causality. I think this is what you are saying — you have verified this impossibility for this fourth order calculation.

Ekstein: Is not a general answer to this and all following questions that any recipe is good if it leads to the desired result?

Dresden: No. Not all recipes are possible. In fact the prescriptions in quantum electrodynamics are quite unique. So it is ceratinly sensible to investigate reformulations and modifications to see whether one may arrive at a deeper understanding of these rules. It is surprising how small a freedom one has in altering the set of rules, while keeping agreement with experiment. This is the reason, I believe, for having some faith that these arbitrary rules have some sense — certainly much beyond their derivation.

Kaplan: Why are you worried about this cut-off? You are not worried about this for quantum electrodynamics, are you? I mean, when one works with quantum electrodynamics and calculates all the things you have listed, one has at the end stage these cut-off terms which somehow completely disappear, is this true in all calculations?

Dresden: Yes. The cut-off terms are perfectly apparent.

Kaplan: Is there not some logarithmic term?

Dresden: The logarithm you talk about is, I believe, an infra red divergence.

Peshkin: It is a perfectly finite term and one which is presumably small. It depends on the square of the electron mass divided by the square of the cut-off.

Kaplan: I was under the impression that in the Lamb shift there is a term which is cut-off dependent and which does not go to zero in the limit as the cut-off goes to infinity.

Dresden: No. There are two terms you must think of. I know now where the confusion comes from. If you do the Lamb shift non-relativistically there is a cut-off at the lower limit. That is because there you do not take into account the fact that the electron is bound. If you do it relativistically this term does not occur; but if you do it relativistically there is still a cut-off dependent term which is, however, of the type m/k where m is the electron mass and k is the cut-off. If k goes to infinity that term gives no trouble. So the one which I originally thought you mentioned is this non-relativistic term which comes from the fact that we have not taken the fact of the binding into account properly.

Let me talk about the infra red divergence. Once in a while you get divergencies which stem from the fact that the lower limit of the integration is zero. These terms arise in a very curious fashion. Suppose I have an electron which is scattered in an outside field (Fig. 13). You can calculate process (a).

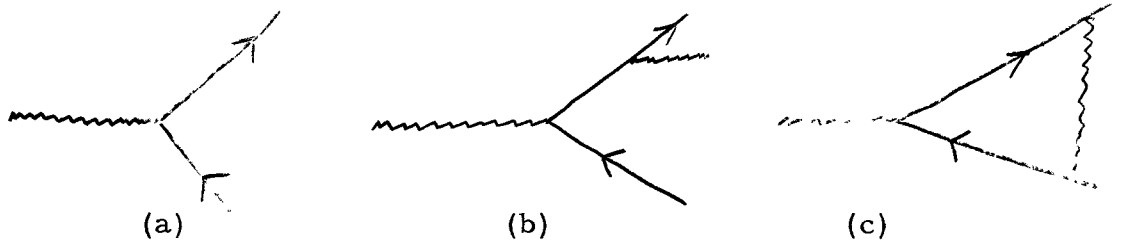


Fig. 13. Electron scattered in an outside field. (a) or (c) without radiation; (b) with emission of a soft photon.

Suppose now that I calculate a process (b), one in which also a soft photon is emitted. Now of course these two are physically different processes. I can ask

for the cross section of this particular process (for the scattering of an electron in an outside field with the emission of a photon). If I calculate this matrix element the result is divergent — it is logarithmically divergent. Integrals of the type $\int d\omega/\omega$ occur, and at 0 frequency they diverge. These divergencies are normally classed as "not serious" for a very amusing reason. The order of process (a) is α. The radiative reaction (c) is always added to it. By the way, if I calculate this particular process the contribution due to the photon line also diverges. Therefore, (b) diverges as does (c). Now it has actually been shown by numerous people, first by Brown and Feynman and then in a bit more official way by Jauch and Rohrlich, that the divergence of order α^4 in the cross section given by the square of the absolute value of the sum of the contributions from diagrams (a) and (c) of Fig. 13 cancels the α^4 terms in the cross section from (b). This cancellation occurs in all orders. I am quoting here what everybody else says; everybody demonstrates this for the second order and says it clearly goes the same in all orders. I have not checked it so I want it understood that this is only a quotation.

Now the reason I think this is an amusing point lies in the fact that here you have an example of a divergence caused by the iteration procedure. The total result if I take the whole series into account is not divergent at all. The divergence arises from the very clumsy and unphysical way in which I separated off the physical processes and the radiative effects.

I cannot really talk about the single process — I must talk about all of them. Doing that I get a perfectly finite result. So the moral I want to draw is

essentially "down with the iteration solution". It's a bad way of describing a physical process, but I want to contrast this statement immediately with what I talked to you about last time, namely vacuum polarization. The vacuum polarization as you know was suggested by the form of the iteration solutions; it is in fact a consequence of it. Now vacuum polarization is a perfectly possible effect; and as I pointed out to you before, it contributes a certain amount to the Lamb shift, it is perhaps directly observable in proton-proton scattering, and it is an effect of significance in positron annihilation. The problem which I now raise is really this: It appears to me that I am to draw two different morals. The one is that the processes obtained from the iteration solution appear to have a certain amount of objective validity, vacuum polarization, for example. On the other hand, if I make the separation in terms of radiative effects I appear to arrive at very unphysical and unrealistic divergencies as in the infrared problem. You might say, "So what?" Part of the discussion is right and the other part is not very good. I just want to mention it because it personally gives me an uncomfortable feeling that the same argument which I like, the diagramatic analysis which yields the classification of processes, works so well and gives me such a good physical understanding in one situation and in another it messes up the works in giving you a divergence. I have nothing more to say about this except that I know that traditionally whenever I mention infrared divergencies — Murray wants to say something, so go ahead and say it, Murray.

Peshkin: Well, I am somewhat embarrassed because I agree with what you said. Maybe I should be the devil's disciple anyway and say that the thing which you have referred to here, perhaps is not so much the fault of the iteration solution as of the way in which one interprets the time-dependent perturbation theory. Here you have two phenomena which are in a certain sense physically distinguishable from each other. On the other hand, you can really only distinguish them in principle by an experiment in which you wait a real long time. Somehow in the perturbation theory you always have to assume that you don't wait too long a time.

Dresden: Yes, this is quite correct. Let me add a third point. This particular scheme — this set of rules — has a restricted validity. It works in quantum electrodynamics. It works also in certain meson theories. It does not work in others and this is really the basis of my question: Is there any deep significance in the circumstance that renormalization works for pseudo-scalar meson theory, and for quantum electrodynamics, but does not work for vector coupling, for derivative coupling, and (perhaps most interesting) it does not work at all for any coupling when one deals with particles of higher spin? Whether one takes this seriously or not depends very much, it seems to me, on the kind of validity one ascribes to these arbitrary rules we just described. There is a very well known book by Schweber and Hoffman where this

is stated explicitly — it is fortunate that most theories are not renormalizable because that means you can throw them away. There the principle of renormalizability is taken as a major principle in the description of nature.

Let me now talk about a few mathematical matters. The first question which, I think, is an important one, is this: Is there any compelling reason to demand that the states of the system can be put in one-to-one correspondence with the vectors of a Hilbert space? This condition, to my knowledge, is never relaxed.

Ekstein: Is this supposed to be a one-to-one correspondence? Each vector in Hilbert space corresponds to a single state?

Dresden: Presumably so, this is a much discussed question (of Einstein, Podolsky, Rosen). I would just like to mention that all the mathematical discussions say that the underlying space in which the physical state is to be represented is a Hilbert space.

There is some relation between the vectors of the Hilbert space and the states of the system. This, of course, is always done in non-relativistic quantum mechanics. It is also always done, to the best of my knowledge, in field theory. Why this is done I am not so sure. Asking the question is, of course, very cheap because immediately you can say that if you do not assume that then you must make some kind of assumption about the manifold of physical states. You must say something about it and if it is not Hilbert space then what is it?

It is possible to give a distribution theoretic formulation of certain aspects of quantum electrodynamics. In this formulation the basic entities are operator valued distributions. A distribution is a continuous linear functional on a space D of test functions ϕ; or, stated in less fancy form, a distribution associates in some manner a number with each function ϕ. The test functions ϕ are infinitely differentiable, they vanish outside a compact set. As an example,

$$T_f(\phi) = f(x)\,\phi(x)\,dx$$

is a distribution.

Derivatives of distributions, defined by $T_f' = -T_f\,(\frac{\partial \phi}{\partial x})$ always exist. It is not possible in general to multiply distributions. (This corresponds to the fact that the product of two integrable functions is not necessarily integrable). By using distributions, one may give a more precise formulation of field theory. The statement that a field operator $\psi(\phi)$ is an "operator valued distribution" means that $(\Psi, \psi(\phi)\Psi)$ is a distribution where Ψ is a vector in the underlying Hilbert space. The fact that products of distributions are not necessarily distributions, can be seen from the fact that $(\Psi_0, j_\mu(\phi) j_\nu(\phi)\Psi_0)$ for scalar fields [$(j_\mu(\phi))$ is the current operator $j_\mu(\phi) = \int d^3x\, j_\mu(x)\,\phi(x)$] still diverges. At one time I thought that the test functions were related to the non-relativistic wave functions of the measuring instruments. The measurement, so to say, produces a non-locality in the fields. Even though this is a beautiful formalism, I could never make any progress with the distribution formulation of interacting fields if I insisted on the usual formal structure where one can use Hamiltonians,

coupling terms, etc.

Ekstein: May I ask what is the trouble? As I understand it in the modern attempts, one just assumes that the products of such distributions are proper operators.

Dresden: I have not seen any discussion of interacting fields. I have seen a lot of discussions of non-interacting fields. In Wightman and also in Bauman and Schmitt, they always talk about non-interacting fields.

Ekstein: Perhaps we can localize the point of this disagreement. Whether one can formulate a consistent relativistic theory of interacting fields is an open question. This I admit. However, as I understand it everybody nowadays believes that the starting point of it will be the assumption that products of the distribution valued operators are proper operators — not necessarily bounded, but proper.

Dresden: In fact they are necessarily not bounded. When I looked at this I could not get any understanding of what was meant by the product of distribution to make sure this was a sufficiently regular structure to allow the use of this particular kind of formalism.

Ekstein: What do you mean by "making sure"? As I understand it this is just assumed.

Dresden: Recall the definition of a field operator; this is that $(\Psi, \psi(\phi)\Psi)$ is a distribution. When dealing with interacting fields, one

deals in the customary formulation with coupling terms of the type $\psi(x)\, A(x)\, \psi(x)$. In order to transcribe this to distribution theory one needs a product $j(\phi)\, A(\phi)$ which generally is not defined. A definable direct product $j(\phi) \times A(\phi')$ does not yield any interaction. (This is reminiscent of the result of Cook, stated in Lecture 1). Consequently if one wants to retain the usual expressions, it appears that the distribution formalism is not suitable for interacting fields.

Sunakawa: One must be careful to insure that the test functions are relativistically invariant.

Dresden: That is correct. The original attempts were made where one integrated over three-dimensional regions; there you are in trouble with relativistic considerations. If you integrate over four-dimensional regions, the light cone must be excluded and the test functions are not defined in a compact set.

Peshkin: You can't get away with the usual business of space-like surfaces?

Dresden: I don't believe so.

There is another comment which I consider more interesting. Assume that states correspond to vectors in a Hilbert space, and in addition assume that the operators possess local commutativity, that is

$$[\psi(x), \psi(y)] = 0 \qquad (x - y^2) > 0 \quad .$$

Finally assume invariance under Lorentz transformations. On the basis of these three assumptions you may prove (as was first proven by Wightman) that the operators ψ are necessarily unbounded. Now I don't know whether you care much — this is the kind of statement which traditionally physicists care little about. By the way, there is nothing strange about unbounded operators. We always work with unbounded operators, the p's and x's in ordinary quantum theory are also unbounded. By the way, "unbounded" means that if I have an element f in the Hilbert space and I apply the operator ψ to it, I no longer know if this has a bound. To state it more precisely I cannot just apply the operator to any element in the Hilbert space. Now the ψ's are unbounded, and this means that an operator in such a space is really not specified if I give you only the operator, but I must also give you the domain in which it can actually operate. It can no longer act on every state vector. So the operators only act on a sub-set of vectors and that sub-set is somehow characteristic of the operator. If ψ is unbounded the question of the domain of the operator is quite important.

I would like to suggest that perhaps the domain of an operator has some kind of physical significance. I don't have the vaguest notion what it is. We identify the observables with characteristics of the operator. It is conceivable, although admittedly far-fetched, that the domain as a characteristic of an operator has some physical couterpart.

Ekstein: What makes you think that ψ is an observable?

Dresden: I would say that I can certainly insist that I can observe

such entities as the vacuum expectation value of ψ because this is the way in which the physical content is always introduced into the theory.

Ekstein: That is not so. Not by Wightman, Haag, Schmidt and Bauman.

Dresden: Schmidt and Bauman, to the best of my knowledge, do not introduce any physical content into the theory. How else does one introduce physical content into the theory?

Ekstein: Of course you are perfectly entitled to formulate your own axiomatics. I only wish to point out that in the so-called modern axiomatic work the only physical interpretation is introduced through the asymptotic quantities which are not at all identical to the quantities $\psi(x)$.

Dresden: I quite agree; but then you have already gone a long way toward a restriction within your theoretical framework. As I understand it, at least from reading Wightman, he is perfectly willing to talk about entities like $(\Psi, \psi(x)\Psi)$. He talks about expectation values and certainly the inference is very strong that these are observable. May I just say this — if you do not think there is something observable about $\psi(x)$ why do you insist on local commutativity? There doesn't appear to me to be a bit of reason for it. The reason I think it is there is that if two points in space-time are separated by a space-like interval, a measurement of one cannot interfere with a measurement of the other, and consequently by the general principles of quantum theory these entities are

to commute. But if you say that these $\psi(x)$ and $\psi(y)$ have nothing to do with any observability, then I have no earthly reason to believe this. If these $\psi(x)$ and $\psi(y)$ are unrelated to observations, why write down local commutativity?

Ekstein: There are various views on that. Wightman nowhere says, and I don't think he believes, that $\psi(x)$ is an observable at all. Haag believes that it is, in a very loose manner, connected with observables. He has set up this idea of quasi-local observables. They are, in a somewhat shadowy manner, connected with it; but the strongest reason (which I believe is mostly given by Lehman, Symanzik and Zimmerman) for writing down this equation is indirect. In order to obtain statements on observables which are asymptotic quantities and to make sure that those statements have the so-called macro-causal nature, one can start out with these non-physical quantities ψ and impose the condition of local commutativity. Then one has fairly good plausibility arguments for believing that asymptotic and observable quantities will indeed have this macro-causal character.

Dresden: What you're telling me is that there exists a particular way, starting from this local commutativity, in which I may in an arbitrary way arrive at a system of equations which itself may have no content. It is a completely open question whether the equations of Lehman and Zimmerman (in fact I hope to tell you something about that tomorrow) have any content whatsoever. I find it very hard to believe

that one can do away with any physical significance of ψ. If you want to say that the ψ are just symbols you choose to utilize to obtain calculable results, that, of course, is legitimate.

I know you and Haag and Wightman all start out with certain asymptotic conditions. In my estimation, I would say that one does that because one does not know what else to do. I don't know what else to do either, but I think the fact you don't know what to do is different from stating a law of nature.

Tanaka: I would like to make one comment. Starting from that commutator relation, one could use equations of motion and derive a similar commutator for the current. The current is observable.

Dresden: Are you talking about electrodynamics now?

Tanaka: I am talking about electrodynamics as well as any field theory. When you give me an equation of motion for ψ, I could relate that to the commutator between two currents. This is a physical statement.

Ekstein: Are you sure that what you are talking about is a total current — not really a part of it? That quantity which is constructed bilinearly from these bare fields is only a part of the current, I believe.

Tanaka: That is the total current.

Dresden: I'm not sure that that is right. Suppose I have a lot of particles (π mesons and K mesons) in interaction; I don't know what current you have in that case any more.

Tanaka: If you are going to include K particles and others, then I would not say it is a total current of the universe.

Ekstein: May I ask you to write down the current density at a point in pseudo-scalar meson-nucleon theory?

Tanaka: I don't think I remember all the terms.

Ekstein: Well, I think you will agree that there is a contribution from the mesons and the nucleons. Therefore, your expression will not be equal to the current density at a point.

Dresden: I think this is right. In general the current will have contributions from all parts of the field.

There are a few final comments I should like to make.

1) I want to remind you of a result of Haag's that for quantized fields interacting with one another, one needs the equivalent representations of the canonical commutation rules. With other words one has always a "myriotic case.

2) The largest progress has been made in the discussion of asymptotic mechanics (Ekstein), the use of specific asymptotic conditions (Lehman, Symanzik and Zimmerman), and the general discussion of asymptotic conditions (Nishijima, Haag, Wightman). Even though here one has advanced furthest in the precise mathematical formulation, it should be emphasized that the connection with physics has become much less clear. This is not true in scattering theory, where probably these general formalisms can lead to

results of direct observational significance. However, in the bound state problems (where after all quantum electrodynamics is very well verified) the rigorous formulations have not as yet been applied — and it is not clear that the "asymptotic viewpoint" will allow such applications. (One should stress that in quantum electrodynamics the success of the Lamb shift, say, depends precisely on the procedure of the separation of the Hamiltonian — which from the "abstract" viewpoint is most objectionable. In this separated form one uses all the information about Coulomb, spin and magnetic-moment interactions, really in a one-particle theory form, then adds the radiative terms in perturbation approximation.)

3) It still seems very worth while to study the use of asymptotic conditions, but rather with the view of constructing a scheme which is such that other physical information may be fitted into it.

Lecture 3.

e) PROPOSED "PRINCIPLE OF UNDECIDABILITY"

I would like to start out by suggesting that every person who has a question should ask it with vigor and persistence. I feel that Hans thinks I will try to put something over on you, but that is not my intention. So if I say anything either in the way of physics, mathematics, methodology, or for that matter anything else, be sure to tell me.

An attempt was made a number of years ago, (it always impressed me very much) connected with divergence and divergence difficulties which nobody has done very much with. The attempt I am talking about was originally under-

taken by Wheeler and Feynman[7] although the problem involved is much older. They actually demonstrated that it was in principle possible to write down classical electrodynamics, not as a field theory but as a theory having only direct particle interactions. Therefore, it was an action-at-a-distance theory rather than a field theory. That was always a result which impressed me because in it one retains what one wants from the field, (one has a finite radiation reaction), and one eliminates essentially all the divergencies. In classical electrodynamics one has an example where there exist two independent and equally valid prescriptions; one prescription uses the Maxwell equations and the field notions, and the other employs only the direct particle interaction. I can now ask myself a silly question: Which one of these formulations is right, the Wheeler-Feynman formulation of the formulation a la Maxwell? Now everybody would immediately say that this is a silly question because you cannot decide. They are both prescriptions and they are both in harmony with one another and the observed facts as far as classical electrodynamics goes. Therefore, the question just doesn't come up. Of course, you could perhaps discuss this question in another framework where the two theories would no longer be equivalent, say in the quantum realm. It is clear that from an a priori viewpoint there is no way of deciding whether the action-at-a-distance theory is better or worse than field theory. Maybe one ought to impose this as a general condition of invariance, and the condition of invariance which I would like to suggest very tentatively is to demand that as physical theories we accept only those which can equally well be formulated in terms of direct particle interactions (in terms of action-at-a-distance) as in terms of field description. So essentially you might say that this

is the principle of undecidability of whether one has an action-at-a-distance theory or field theory. This principle somehow appeals to me and I can give no further justification of it other than that it appeals to me. I can only tell you that it is not true that every field theory can necessarily be written as a direct particle theory or conversely that the direct interaction can be written as a field theory. So it is a principle which may be wrong, but it is not vacuous — it will reject certain theories and allow others. Now whether or not this is right only further discussion can decide, and I have not been able to make very much progress with it, especially if one makes the additional demands of quantum theory or relativity. I just thought I would mention it to you.

Havas: If you insist they are the same then you would have to reject all meson theory.

Dresden: Somehow this doesn't break my heart.

Havas: Yes, but what would be worse is if you would have to reject all theories which correspond to a field where the rest mass of the field is zero so that won't leave you very much.

Dresden: I am not sure just how serious this is. If I think of masses exclusively as self energies, then I am quite willing to start out with a basic theory which has zero rest mass. The mass is then a derived entity which is a manifestation of a self-energy.

Havas: I won't argue about this — I just wanted to make a statement.

Dresden: This, if nothing else, lends support to the statement that the principle is maybe nonsense but it is not empty, and it does

restrict the possible theories severely.

Havas: What I wanted to say is that I feel that the Wheeler-Feynman theory as such has nothing to say about action-at-a-distance vs field. It can be formulated from both points of view because you can get the same equations of motion as you get from field theory.

Dresden: Yes, I agree with that, except that there are theories which do not have this characteristic. There are theories where you will get different results from an action-at-a-distance than you do from a field-theory approach. I think one might consider a principle which would reject such theories.

f) THE ASYMPTOTIC APPROACH

The next point which I want to make, and the last one before I come to a more formal discussion, concerns the modern trend as exemplified by the work of Haag, Wightman, Ekstein and Lehman. You know Hans has talked about this a year or so ago. He gave a series of lectures which were properly entitled "Asymptotic Mechanics". There were no longer discussions about the details of the interactions but rather one attempts to learn what one can from the change from initial to final states. One hopes to find operators which somehow connect the initial and final states. Other characteristics of the system must then be expressed in terms of these operators. The S-matrix theory in its original form was this kind of theory. Most of the progress which has been made in the last 3 or 4 years has been made in this direction. To the best of my knowledge,

however, nobody has made any progress in studying the region of strong interaction in the same mathematical detail. It is of course an open question whether such a detailed description is either desirable or possible. It is not known whether the S matrix contains all the physical information. These more precise formulations have not helped, and in fact have at this point not yet achieved the power of the set of recipes or rules which I have given you previously in quantum electrodynamics. Specifically, it is a somewhat embarrassing point that the confirmations of quantum electrodynamics lie in part in a discussion of bound state problem. The Lamb shift is a bound state problem, and so is positronium; the magnetic-moment correction, of course, is not. Unfortunately the more precise mathematical discussions are quite incapable of treating these problems at this point. This is a thing to be kept in mind, and this is in part a justification of what I want to talk to you about later. What I consider to be a very significant question is to see whether in any way one can fit information about the bound states into these more general schemes. It is, I believe, correct that within the framework of the axiomatic approach, one does not have the computational power to deal with the problems which have been handled using the renormalization technique. This suggests this question: Is it possible, starting from a more precise mathematical viewpoint to imitate or, in any sense, obtain procedures which would justify what has been done previously (in a haphazard fashion)? As I understand it, there is no hint of that within the axiomatic formalism.

Ekstein: No, that is not quite so. This more precise method cannot deal with the question of the metastable state because we don't

understand what a metastable state means. However, the so-called metastable state of the hydrogen atom, that is an excited state, is really not necessary if you ask the question, "What is the scattering amplitude of a photon on a hydrogen atom?" and there is in principle a perfectly precise answer to that. What we cannot answer is: Given at some initial state an excited hydrogen atom, what will happen later?"

Dresden: Yes, but it seems to me that a perfectly definite question is: "What is the energy spectrum of a hydrogen atom in vacuum?" In terms of the conventional physical picture you have an interaction with the vacuum fluctuations. This gives rise to an observable level separation between the $^2P_{1/2}$ and the $^2S_{1/2}$ states. I know of nothing within the abstract formalism which would allow me in principle to calculate this.

Ekstein: You can ask very simply the question, "What is the spectral density of the total Hamiltonian?" and then at least in principle you would obtain certain peaks of the spectral density which to be sure are continuous functions and not delta-functions, and those will correspond to the so-called stationary spectrum of the hydrogen.

Dresden: May I ask, where do you get the total Hamiltonian?

Ekstein: The total Hamiltonian is presumably the one that you always write down.

Dresden: With point interactions?

Ekstein: Yes.

Dresden: With what commutation rules?

Ekstein: With the canonical commutation rules.

Dresden: Why am I not in trouble? This formalism gives all the divergencies!

Ekstein: Oh, you are in trouble. I did not want to say that the more precise methods are able to actually deal with the present local field theories.

Dresden: In principle then how do I get answers? I don't care if you give me wrong answers, but what I want to know is if I start out with the formulation a la Wightman how then can I calculate the hydrogen spectrum?

Ekstein: Perhaps I can explain that this more precise approach is still very far removed from those Hamiltonians which have any plausibility of describing actual nature. In fact, we are pretty certain that none of those Hamiltonians actually fit into the axiomatic scheme as either of us has formulated it. This is a general critique of the state of the art, so to speak, but it is not a specific critique of the ability of this art to deal with these questions such as the spectrum of the hydrogen.

Dresden: Maybe I ought to make one final point in this same connection. The reason the renormalization works so well is precisely the same feature which makes it mathematically so objectionable, namely you can split up the Hamiltonian into an unperturbed part with corrections added to that unperturbed part. The perturbation calculations used in the Lamb shift are all performed by separating a

Hamiltonian. It is well known that in the more precise formulation such a splitting of a Hamiltonian cannot be done. It is ambiguous and gives rise to nothing but difficulty. Nonetheless, it seems to me that it is a curious feature of the present situation that mathematically the most objectionable feature turns out to be just the one which makes the calculation possible.

Let me tell you a little bit about some of the attempts of Gordon Feldman and myself, in connection with these questions. There exists an abstract formulation of field theory which is due to Lehman, Symanzik and Zimmerman,[8] and it, among all the abstract field theories, is the least abstract. Let me tell you a little about it and also how we attempted to introduce into this theory a certain number of observable notions.

Let me make the physical assumptions clear. First of all, one deals with a field operator A(x), an ordinary field operator. One also makes the assumption that the state vectors are elements of a Hilbert space. You make the assumption of local commutativity, $[A(x), A(y)] = 0$ for $(x-y)^2 > 0$. You also assume Lorentz invariance and this I believe is all, except for one new condition, the so-called asymptotic condition. I want to say something about it later. Now let me make this comment immediately. They do not assume equations of motion for the A field, nor commutation relations for the A field. The reason for that is that the assumption of either commutation relations or equations of motion will immediately lead back to a divergent theory. The

theory they contemplate is already renormalized and no infinities ought to occur in it whatsoever. This presumably one can arrange, at the expense of making the theory so broad that the real question will be whether the theory indeed has any content whatsoever.

Define two fields, described by operators A_{in} and A_{out} satisfying

$$(\Box - m^2)\, A_{in}(x) = 0 \qquad (\Box - m^2)\, A_{out}(x) = 0 \,. \tag{12}$$

I will also need functions f_α, ordinary c-number functions. They will be normalized and they will be solutions of the Gordon-Klein equations,

$$(\Box - m^2)\, f_\alpha(x) = 0$$

$$f_\alpha = \frac{1}{\sqrt{2k_o (2\pi)^3}}\; e^{i(\vec{k}\vec{x}) - ik_o t} \qquad k_o > 0 \,. \tag{13}$$

This is a set of solutions of the Gordon-Klein equation, all belonging to positive energies. I will only use positive energies, and the functions f_α satisfy orthogonality condition of the type

$$-i \int d^3x \left(f_\alpha \frac{\partial f_\beta^*}{\partial x_o} - f_\beta^* \frac{\partial f_\alpha}{\partial x_o} \right) = \delta_{\alpha\beta} \,. \tag{14}$$

The completeness relation is

$$\sum_\alpha f_\alpha(x)\, f_\alpha^*(x') = i\Delta^+(x-x') \,. \tag{15}$$

The Δ^+ is one of the usual singular functions. Introduce the operators A^a defined by

$$A^a(t) = i \int d^3x \left(A(x) \frac{\partial f^3_a}{\partial x_0} - f^*_a(x) \frac{\partial A}{\partial x_0} \right).$$

(You can formulate this in a number of ways. You could use distribution theory.) This is integrated at a constant time and over all space. (If one uses space-like surfaces, nothing is changed.) No more is known about the A^a than about the A. If the operators A exist the A^a are somewhat more regular; in fact, that's really all they are — they are smoothed out operators. The f_a really play the role of invariant form factors.

The following asymptotic condition is imposed. I take a state Ψ of the system, and also a state Φ, and suppose I calculate the matrix element of the operator A^a. Quite clearly this matrix element $(\Phi, A^a\Psi)$ will depend on the time and now you would like to say something about the limiting behavior of this matrix element as $t \to +\infty$ and $t \to -\infty$, namely

$$\lim_{t \to -\infty} (\Phi, A^a\Psi) = (\Phi, A^a_{in}\Psi). \tag{17}$$

Now A^a_{in} is an operator about which one does know something, namely it satisfies the Gordon-Klein equation. This condition therefore states more precisely that a very long time ago the particles behaved essentially as free particles which satisfy the Gordon-Klein equation. You will not be surprised that in the limit as $t \to +\infty$, A^a becomes A^a_{out}.

I will assume that a vacuum state exists which corresponds to the lowest energy. These are all the physical assumptions made in the theory. This is a very general framework and one sees that there is no Hamiltonian in it. There is no question of a splitting into a perturbed and an unperturbed Hamiltonian because you don't have either. So the question which now comes up is: Can one do anything with a theory of this kind? Precisely what can be done with a theory of this kind is still an open question, but certain formal results can be obtained. The states with which one deals are the following ones: First one has a vacuum state which is assumed; call it Ω. The next state which you can have is a state $A_{in}^{a}\,\Omega$. An A_{in}^{a} is an operator about which I know a good deal because that is an operator which satisfies the Gordon-Klein equation. That means it may be expanded using functions f_a in terms of creation and annihilation operators a_a, a_a^+, operators which destroy or create a particle with wave function f_a. So $A_{in}^{a}\,\Omega$ is a state which contains asymptotically a single meson of wave function f_a. You can then also introduce a state

$$A_{in}^{a_1} A_{in}^{a_2} \Omega \, .$$

In general I will write such a state as

$$\Phi_{in}^{(a)} = A_{in}^{a_1} \cdots A_{in}^{a_k} \quad \Omega \, . \tag{18}$$

You can of course do exactly the same thing for the out-field and there the $\Phi_{out}^{(a)}$ is described by $\Phi_{out}^{(a)} = A_{in\ out}^{a_1} \cdots A_{out}^{a_\ell}\,\Omega$. These are the basic states with which the theory operates. One now defines (and this is a perfectly general and reasonable definition) a matrix element of the S-matrix as the overlap

between an incoming and outgoing state, i.e.,

$$S_{(\alpha)(\beta)} = (\Phi^{(\alpha)}_{out} , \Phi^{(\beta)}_{in}) . \tag{19}$$

This is the (α, β) matrix element of the S-matrix. The (α) and (β) run over all numbers of incoming and outgoing particles. This S-matrix is defined in terms of A^{α}_{in} and A^{α}_{out}. Now one hasn't learned anything. There are two results which are somewhat surprising and which I would like to state for you. I don't think I will prove them for you although the proof is not very hard.

I want to give one more definition: Define τ-functions (sometimes called Wightman functions) as the vacuum expectation value of time-ordered T-products

$$\tau (x_1 \cdots x_n) = (\Omega, T\{ A(x_1) \cdots A(x_n) \} \Omega) . \tag{20}$$

(For many considerations the R-products, retarded products, are much more suitable.)

I might point our parenthetically that one of Wightman's major contributions to this whole type of endeavor is the proof that the theory is essentially uniquely characterized by these Wightman functions, or τ-functions. Now the contribution of Lehman, Szymanzik and Zimmerman consists of two main results. First, the S-matrix can be expressed in terms of the τ-functions. This in a sense is a special case of the general result of Wightman that the physical content is completely determined by the τ-functions. Since the S-matrix is presumably one of the physical results, it must be expressible

in terms of these τ -functions. In a sense this is a systematic advance, but I still have not learned anything inasmuch as these τ -products are vacuum expectation values of operators about which one knows nothing. One only knows the asymptotic condition. So this is in the nature of an organizational advance, but nothing more.

The second statement is perhaps more surprising, it is more meaningful but also much more difficult to discuss: There exists a set of equations for the τ -functions alone. What I mean by alone is that there is a set of equations in which only the τ -functions and known entities occur. These known entities are all expressible in terms of the f_a, but they no longer contain any reference to the operators A. It is not known whether this sytem of equations determines the τ -functions. If that were so, one would be in business. If you know that the system of equations determines the τ -functions and if then in turn all the physical content is contained in these functions, then I'm through. If in addition everything would come out finite, then we could all take a holiday.

I can prove one thing about this system of equations, namely it is not vacuous — it has some content. How much I don't know. There are only two physical ideas which go into the derivation of this: The first idea is that the S-matrix is unitary. The second idea, the second physical idea which goes in there, is the completeness of the set $\Phi_{in}^{(a)}$ which I have written down. These are the two physical ideas which, of course, are both assumed. One assumes to completeness — in this formalism one can prove the unitarity of S quite easily.

Tanaka: It appears that there are enough of these equations so that there is at least a hope that they may be solved.

Dresden: Correct. This is also all there is — hope. There is not yet despair. You have a large number of these equations and in fact G. Feldman and I have tried to say something about this system of equations.

Let me indicate to you how one obtains these results because the derivations are really extraordinarily simple and quite pretty. What one does is the following. First prove the result that

$$(\Omega,\ T(x_1 \cdots x_n)\ \Phi^{(a)}_{in}\ ) = -i \int d^4\ y\ K_y\ \tau\ (x_1 \cdots x_n, y)\ f_a(y), \tag{21}$$

where

$$K_y = \Box_y - m^2\ .$$

The next thing you obtain is an expression for $(\Omega,\ T(x_1 \cdots x_n)\ \Phi^{a_1 \cdots a_k}_{in}\ )$. This is the transition matrix element between Ω and an arbitrary state and, using the unitarity, you throw it around and eventually you get the expression for the S-matrix which I will write down in a moment. Let me just sketch part of it. Make the observation that

$$(\Omega,\ T(1 \cdots n)\ \Phi^{a}_{in}\ ) = (\ T^*\ \Omega,\ A^{a}_{in}\ \Omega\ ). \tag{22}$$

At this point one applies the asymptotic condition in a very cute way. Since $T^*\Omega$ is a state vector, Ω is also a state vector, so one has

$$(\Omega,\ T(1 \cdots n)\ \Phi^{a}_{in}\ ) = \lim_{t \to -\infty} (T^*\Omega,\ A^{a}\Omega\ ). \tag{23a}$$

What I have done, as a matter of fact, is just to run the asymptotic condition backwards. So this is a straightforward application, and now you just play around. One of the things one does is to make use of another observation, namely

$$\lim_{y_0 \to -\infty} \iiint d^3\, y\, I = -\int_{-\infty}^{+\infty} d^4\, y\, \frac{\partial}{\partial y_0} I + \lim_{y_0 \to +\infty} \iiint d^3\, y\, I\,. \tag{23b}$$

The final thing which is amusing is that you know how to handle this T-product. The T-product, as you know, is time-ordered; but in the limit when t goes to $+\infty$, you know precisely where to put the particular operator A(t) since it is necessarily last in time and as such has to be put way to the left. One ends up in fact with this four-dimensional integral which I wrote down here. The reason you keep a four-dimensional integral even though you have a derivative here is because you make a partial integration on the f-functions and then you make use of the equation which they satisfy. Then you end up with an expression for the S-matrix which is

$$S_{(\alpha)(\beta)} = (-1)^{k+\ell} \int\cdots\int d^4\,\xi_1 \cdots d^4\,\eta_\ell\; K_{\xi_1}\cdots K_{\eta_\ell}\; \tau\,(\xi_1 \cdots \eta_\ell)\, f^{*}_{\alpha_1}(\xi_1)\cdots f_{\beta_\ell}(\eta_\ell)\,. \tag{24}$$

Next consider the question of the system of equations for the τ-functions. The derivation of that is also quite trivial. Let me show you how one does it. Suppose I write down the T-product

$$T\{A(x_1)A(x_2)\} = \theta\,(x_1-x_2)\; T\{A(x_1)\}\; A(x_2) + \theta\,(x_2-x_1)\, T\{A(x_2)\}\; A(x_1) \tag{25a}$$

where

$$\theta(x) = 1 \qquad x > 0$$
$$= 0 \qquad x < 0 \ .$$

The T here, of course, one doesn't need, because there is only one A-operator. I am only writing that down for two operators but you can do it for any number. Let me write down in general

$$T\{A(x_1) \cdots A(x_n)\} = \sum_p \theta(x_1 - x_n) \cdots \theta(x_{n-1} - x_n)\, T\{1 \cdots n-1\}\, A(x_n) \ . \tag{25b}$$

Now from here on the derivation between the relations of the τ-functions is quite simple because if I take the vacuum expectation value of the left side, I will just get a τ-function which now will be expressed in terms of what? Well, if you take the vacuum expectation value of the right side, you see the kind of entity I get will be the matrix element

$$(\Omega,\ T(1 \cdots n-1)\, A(x_n)\Omega) \ .$$

This is not yet a τ-function. Now you make the observation that you may write this, using the completeness relation, as

$$(\Omega,\ T(1 \cdots n-1)\ \Phi_{in}^{(\alpha)})\ (\Phi_{in}^{(\alpha)},\ A(x_n)\ \Omega) \ .$$

By the way, summation over α is really over all α's, i.e., over singles, doubles, triples, etc.

Sunakawa: There is an assumption that there is no bound state?

Dresden: Yes, that is right, that is assumed.

Now at this point I will no longer pursue the derivation but I want to point out that now you can see for yourself that this indeed can be expressed in terms of τ-functions because

$$(\Omega,\ T(1\cdots n-1)\ \Phi^{(a)}_{in}) \qquad \text{and} \qquad (\Phi^{(a)}_{in},\ A(x_n)\ \Omega)$$

may be so expressed. If you put it all in, then you end up with an expression for one τ-function in terms of all others. And that expression I will write down for you because it is very miserable.

$$\cdots\tau(1\cdots n) = \sum_p \sum_k \theta(1-n)\ \theta(2-n)\cdots\theta(n-1-n)\ i^k \int\cdots\int d^4\xi_1\cdots d^4\xi_k\ d^4\eta_1\cdots d^4\eta_k\ Y$$

where

$$Y = K_{\xi_1}\cdots K_{\xi_k}\tau(x\cdots x_{n-1},\ \xi_1\cdots\xi_k)\ K_{\eta_1}\cdots K_{\eta_k}\tau^*(x_n)\ \eta_1\cdots\eta_k\Delta^+(\xi_1-\eta_1)$$

$$\cdots\ \Delta^+/\xi_k-\eta_k)\ . \qquad (26)$$

By the way, this relation contains what I said it would — the functions, some combinatorial factors, integrations, operations on the τ, etc. I can now phrase the major question very simply, namely: Does this system have any content whatsoever, or is it a pure tautology? All this is contained in the first paper by Lehman, Szymanzik and Zimmerman. Let me make the following observation as a side remark. This system of equations has nothing to do with

invariance properties, in fact it only depends on the unitarity of the S-matrix and on the completeness. If you make assumptions about symmetry or about invariance, you have special restrictions on these τ -functions. That means then that this is a general result for an already renormalized theory. This is a very complicated non-linear system, and one might despair at obtaining any results at all. This is the kind of question Gordon Feldman and I investigated. We started by seeing whether everything automatically satisfies these equations. That is not true. Let me mention one result which I consider the most interesting one. The advantage of this particular formulation is that it should be directly applicable to anything; nothing has been assumed about invariance, about point sources, about interaction — it is perfectly general. This means that this system of equations as it stands should be directly applicable to such models as the Lee model. One can in fact calculate what these τ -functions are for the Lee model and you can just verify the validity of this system of equations. You find something quite amusing; you find that indeed they do satisfy the Lehman relations. But, as you all know, in the Lee model there is a certain critical coupling constant; if the coupling gets larger than a certain amount, then the Lee model is not renormalizable in the ordinary sense of the word. Now the thing which is interesting about this is how this information is indeed contained in the system of equations which I have written down here.

Tanaka: How can you verify this? What exactly was done?

Dresden: I took the τ -functions from the Lee model.

Tanaka: How many are there? Can you write a general form for this?

Dresden: You can calculate the τ -functions for the Lee model; actually all the integrals separate in a very simple form. Essentially you can write this as a product of integrals. Putting this into momentum space simplifies matters. Then what you get are folding type integrals. But therefore I can make the following statement: If the coupling constant comes out to be less than the critical value, then the Lee τ -function satisfies the Lehman relations. If it is larger than the critical value, then they are not satisfied. The result I think is interesting because it settles for me one question which I had not known before — whether this system of equations has any content. For a long time I thought that all functions satisfy these results. This is clearly not true because I can at least give you one example, namely the τ -functions of the Lee model whenever the coupling constant is larger than the critical coupling constant, for which they are not satisfied.

A much harder question is the following: Is it possible (and this happens to be the problem I am looking at at this particular moment) to look at this system of equations again, and knowing now that the Lee model satisfies it, invert the problem. I can ask for necessary and sufficient restrictions on the τ -function so that the Lee model will result. One question which always haunts me in all these considerations is where in these formalisms is the coupling constant; where is the interaction? In some sense both the magnitude and the form of the coupling must be contained in the expressions for τ. The

reason for studying these models is precisely so that one may obtain relations between characteristics of the τ-functions and the structure of the coupling terms. One finds relations between the poles and zero's of the τ-functions and the nature of the potential in simple cases. In this way one may learn to transcribe physical information directly into the τ-functions, which usually are quite unintuitive structures. Perhaps one may hope that one may learn to put physical information, previously put in the Hamiltonian, directly into the τ- or R-functions. It is interesting to observe that in the Lee model the τ functions just have a single pole and a single zero. Any function of a complex variable defines a transformation; these functions in particular define a simple class of transformations. It is interesting to observe that the Hamiltonian of the Lee model is in fact invariant under a transformation of a very similar form. So one might perhaps conjecture, that there is a relationship between the group of transformations which express a certain symmetry character of the Hamiltonian, and the structure of the τ-functions. In the models it appears as if the τ-functions are like the generating functions of the transformations which leave the Hamiltonian invariant. This is all conjecture. However, I would like to mention that this seems like a possible way of feeding physical information into this scheme.

There are a number of other questions which one can ask: Suppose I do the same formulation for a vector field and for a spinor field. Then of course I get very similar equations. Suppose I now impose the conditions of relativistic

invariance and gauge invariance (which is, by the way, not so trivial to do). The asymptotic conditions are quite nasty to deal with when you deal with gauge invariance. I can now ask the question: Is the theory which I obtain isomorphic with renormalized quantum electrodynamics? If I have a theory which is gauge invariant and relativistically invariant, and which couples a spinor field and vector field, is that renormalized electrodynamics? I don't know the answer to the question; I cannot handle the gauge invariance with a great deal of dispatch. I would like to know the answer. We have looked at that some with not very exciting results.

This is essentially all I have to say in this connection. I am sorry that I don't have more definite answers.

Peshkin: May I ask a question? What is a ghost state?

Dresden: The ghost states were first noticed by Pauli in the Lee model. There, after renormalization, one still has trouble because then other requirements of the theory, such as the unitarity of the S-matrix, are no longer satisfied.

Miyazawa: You say that the equations plus gauge invariance and Lorentz invariance gives all of electrodynamics?

Dresden: That's the question. Do these equations plus Lorentz invariance plus gauge invariance give renormalized electrodynamics? Nishijima has a paper which I have not understood, unfortunately, in which he claims that if you do certain things you obtain something equivalent to the perturbation expansion in quantum electrodynamics already

renormalized. That I think would be somewhat of a feat if it were right because then it would mean that, at least on the basis of more or less well-defined mathematics and well-defined assumptions, one obtains this series development. But I have not understood the paper so I am not sure that it is right.

Miyazawa: My statement is that if you want to use invariance (Lorentz invariance, gauge invariance, causality) one obtains renormalized electrodynamics.

Dresden: But nobody has yet shown that; is that right?

Miyazawa: Yes.

Ekstein: I want to thank you very much for these three lectures. You were certainly inspiring.

Dresden: Thanks to your discussions. You are a gentleman.

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Classical Relativistic Theory of Elementary Particles

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Lecture 1.

I. INTRODUCTION

There is a twofold interest in the part of the classical theory of elementary particles which I am planning to discuss here: First, an intrinsic interest as a part of the scheme of classical physics; and second, its possible relevance for the quantum theory of elementary particles. I shall first discuss the scope of the classical problem.

Within the conceptual framework of Newtonian mechanics there is a sharp division between equations of motion and force laws. The equations of motion ($F = ma$) are supposed to be the same for all matter, while the force laws (like Newton's law of gravitation or Coulomb's law, etc.) are different for different types of particles; thus, they alone are the object of experimental investigation.

This apparent independence of the equations of motion and the force laws was maintained at first, also after it was realized that the laws of physics take a simpler form if one does not consider force laws, but rather field laws. The prototype of this approach is, of course, Maxwell's electrodynamics. The independence of the two sets of laws seemed to be firmly established when it was slowly realized that it was not possible to find a mechanical model for Maxwell's equations, and when it was found that there was one phenomenon, namely electromagnetic radiation, which allowed a simple description in terms of the field equations alone.

On the other hand, if radiation was to carry away energy, it was clear that the equations of motion had to be modified if one would want to maintain

conservation of energy for the entire system. The main contribution at this point is due to Lorentz, who approached the problem in two different ways. First, he simply modified the equations of motion to maintain conservation of energy on the average for almost periodic motions. Second, he tried to calculate the change in the equations of motion, if an electric charge was considered to be of finite extension, the retarded effects of the different parts of the charge on each other were taken into account and an additional condition was imposed, namely that the total force on the particle should vanish.

As you all know, Lorentz's calculations led to a dimemma. If the charge was taken to be of finite extension, a term proportional to the second derivative of position appeared. It seemed to offer possibilities of an electromagnetic theory of mass. On the other hand, the equation of motion obtained was of infinite order and all the terms beyond the third derivative of position were structure-dependent. If the charge was to be taken as a mathematical point, all the structure-dependent terms disappeared, the term in the third derivative (correctly describing radiation damping) was unchanged, but unfortunately the term proportional to the acceleration became infinite.

I shall not pursue the line of research which tried to get a consistent field theory of mass. On the whole, I believe it did not bear fruit. However, Lorentz's realization that the imposition of certain conditions in addition to the field equations could lead to, or at least modify, the equation of motion did indeed lead to important results which we shall discuss later. Most of this

work is based on a paper by Dirac written twenty years ago, which presented a new derivation of Lorentz's equations of motion, based on the requirement of conservation of energy and momentum.

The entire problem appeared in a new light in the general theory of relativity. Initially, Einstein had thought that again the equations of motion had to be postulated independently of the field equations; for a test particle he took the motion to be determined by a geodesic. However, it was soon realized that actually the field equations do not allow solutions for arbitrary motions and that the equations of motion are implicitly contained in the field equations. Basically, this is due to the fact that the field equations are so constructed that the conservation of the total energy is already implied. The equations of motion obtained from the general theory in the approximation of the special theory of relativity are the same as one can obtain directly by special relativistic methods on the additional assumption of conservation laws, the line of approach proposed by Dirac.

In this sense, a complete unification of the laws of motion and of the laws of force has been obtained. There is, however, another approach which also leads to complete unification. This is the approach from the point of view of action at a distance. There, fields are not considered to be basic independent variables. This approach was, of course, taken by Newton, but it was abandoned in the second half of the 19th century when it was realized that physical effects are not transmitted instantaneously. Nevertheless it was realized, even then, for example by Gauss, that such a description should be possible, and a formulation of such effects in terms of particle variables alone was first given in the case

of electrodynamics by Schwarzschild and Fokker, and it has since been extended to other fields. Their theory can be summarized in a single variational principle in terms of particle variables alone, from which one can not only obtain the equations of motion, but one can also define "fields" in terms of the particle variables and then show that the new quantities so defined satisfy the appropriate field equations.

In this approach the main difficulty is in the description of radiation and this is why the field approach was pursued almost exclusively for a long time. However, it had been suggested already half a century ago by Einstein that possibly radiation was a statistical effect. The first attempt to give a quantitative description following this idea is due to Wheeler and Feynman who, with certain assumptions, showed the equivalence of this formulation and that of the usual theory in electrodynamics. Their considerations can be extended to mesodynamics as well. However, there are some significant differences between the two formulations there. It can also -- at least for the linearized theory -- be extended to gravitation.

This is roughly the scope of the topics I want to discuss in the classical theory. I believe that this is also of interest for the quantum theory of interacting elementary particles, mainly because the quantum theory has been plagued by infinities and mathematical inconsistencies. A considerable part of these difficulties seems to be due to the fact that the usual theory is built up by first constructing a theory of free fields, and then attempting to couple

these. The classical theory has had similar difficulties with infinities, but it has been able to overcome them in two different ways. In field theory no concept of "bare" particle is necessary. The particle is introduced as a singularity of the fields and mass renormalization can be accomplished exactly for all fields and all types of singularities in one step. In action-at-a-distance theory no infinities appear at all as the field is not an independent quantity, and again no "bare ' particles need to be considered. The existence of these alternative classical formulations suggests the possibility of alternative approaches to quantization.

Furthermore, there exist classical restrictions on the types of particles and fields allowed. The most stringent one concerns fields of spin two or greater. Although it is possible to develop a theory of pure fields of this type, it can be shown that (at least within the framework of linear theory) it is not possible to have singularities interacting by means of these fields at all. Also there exists a very general equivalence theorem for different couplings. All of these results suggest similar restrictions in quantum theory. Considering the present state of the theory I believe that some alternative approaches are badly needed, and that some of the recent advances of the classical theory may provide some fruitful new ways of looking at old problems.

II. NEUTRAL FIELDS OF INTEGRAL SPIN WITHOUT SOURCES

Today I will mostly go through some mathematical preliminaries, and first of all notations. In general I shall use a metric

$$\eta_{\mu\nu} = 0, \quad \mu \neq \nu, \qquad \eta_{00} = -\eta_{11} = -\eta_{22} = -\eta_{33} = 1$$

and a four-space x^0, x^1, x^2, x^3 ($\equiv$ t, x, y, z). The velocity of light is taken as unity. We shall denote the coordinates of a particle by z^μ, and its proper time by τ where $d\tau^2 = dz_\mu \; dz_\mu$. The four-velocity is

$$v^\mu = \frac{dz^\mu}{d\tau} \equiv \dot{z}^\mu$$

and therefore

$$v_\mu \, v^\mu = 1, \; v_\mu \, \dot{v}^\mu = 0.$$

Furthermore

$$\partial_\mu \equiv \frac{\partial}{\partial x_\mu}, \; \partial_{\alpha\beta\cdots} \equiv \partial_\alpha \, \partial_\beta \cdots, \; \Box \equiv \partial^\mu \, \partial_\mu .$$

The notation I use is essentially the one which has been used in a series of papers by Bhabha and Harish-Chandra.

Now I shall outline the theory of fields of integral spin. This has first been given by Dirac [1] and then was taken up by Fierz; [2] the energy-momentum tensors I am going to discuss later have, according to Fierz' paper, first been given by Jauch. The fields by both Dirac and Fierz were given without interaction, the interactions were introduced later, mainly by Harish-Chandra. Fierz considered charged fields and described them by complex tensors. I

shall write first the equations for neutral fields and then generalize directly to charge-symmetric fields. This requires essentially no extra work, and then one can always fall back to charged fields alone by just taking two components of the charge-symmetric field. (I will discuss what I mean by charge-symmetric fields later.) Therefore, I will describe fields by a real symmetric tensor U of rank f, which obeys the following equations.

$$(\Box + \chi^2)\, U_{\ldots} = 0, \tag{1}$$

$$\partial^{a} U_{a\ldots} = 0, \tag{2}$$

$$U^{a}_{a\ldots} = 0. \tag{3}$$

Here χ is a constant of dimensions of reciprocal length which after quantization corresponds to quanta of mass $m = \frac{\hbar \chi}{c}$. The reason for Eqs. (2) and (3) is that you want to get a theory which is irreducible and which after quantization describes quanta of spin $f\hbar$ without also describing quanta of smaller spin. The conditions are just enough to allow only (2f + 1) linearly independent plane wave solutions if $\chi \neq 0$. If $\chi = 0$, then the number of independent components is two just as in electrodynamics, independent of f.

Hamermesh: For spin one there is no condition except on the divergence (Eq. 2)?

Havas: Yes. For spin one this is just the usual electrodynamics with the Lorentz condition.

Equations (1) to (3) correspond to the description of electrodynamics

with the use of potentials alone, but one can also describe it instead by fields and potentials. One needs both if one has $\chi \neq 0$, and one can use just the fields if $\chi = 0$, as in electrodynamics.

One can introduce

$$U^{(1)}_{[\alpha\beta]\ldots} \equiv \partial_\alpha U_{\beta\ldots} - \partial_\beta U_{\alpha\ldots} \quad . \tag{4}$$

The square bracket implies that the tensor is now antisymmetric in the bracketed indices. Then from Eqs. (1), (2) and (4) one gets

$$\partial^\alpha U^{(1)}_{[\alpha\beta]\ldots} + \chi^2 U_\beta = 0. \tag{5}$$

From these equations we also have

$$U^{(1)\beta}_{[\alpha\beta]\ldots} = 0,$$

$$U^{(1)}_{[\alpha\beta]\gamma\ldots} + U^{(1)}_{[\gamma\alpha]\beta\ldots} + U^{(1)}_{[\beta\gamma]\alpha\ldots} = 0 \ ,$$

$$\partial_\alpha U^{(1)}_{[\beta\gamma]\ldots} + \partial_\gamma U^{(1)}_{[\alpha\beta]\ldots} + \partial_\beta U^{(1)}_{[\gamma\alpha]\ldots} = 0,$$

$$\partial^\gamma U^{(1)}_{[\alpha\beta]\gamma\ldots} = 0.$$

One can take this set as basic and then can regain the others because, for example, you can contract Eq. (5) and regain the Lorentz condition (2) provided $\chi \neq 0$. If $\chi = 0$ you have to postulate it separately just as in electrodynamics, where the Lorentz condition does not follow from the field equations.

Following Fierz, we can also define quantities

$$U^{(q)}_{[\alpha\beta]\ldots} = \partial_\alpha U^{(q-1)}_{\beta\ldots} - \partial_\beta U^{(q-1)}_{\alpha\ldots} \tag{6}$$

for all q f, which again satisfy the wave equation, and then have

$$\partial^\alpha U^{(q)}_{..[\alpha\beta]..} + \chi^2 U^{(q-1)}_{..\beta..} = 0, \tag{7}$$

$$U^{(q)\beta}_{..[\alpha\beta]..} = 0 \tag{8}$$

$$U^{(q)}_{..[\alpha\beta]\gamma} + U^{(q)}_{..[\gamma\alpha]\beta} + U^{(q)}_{..[\beta\gamma]\alpha..} = 0, \tag{9}$$

$$\partial_\alpha U^{(q)}_{..[\beta\gamma]..} + \partial_\gamma U^{(q)}_{..[\alpha\beta]..} + \partial_\beta U^{(q)}_{..[\gamma\alpha]..} = 0, \tag{10}$$

$$\partial^\gamma U^{(q)}_{..[\alpha\beta]\gamma..} = 0; \tag{11}$$

$$(\Box + \chi^2)\, U^{(q)}_{[\alpha\beta]..} = 0. \tag{12}$$

Equations (7), (10) and (12) are analogous to those of electrodynamics. The others don't have a counterpart in electrodynamics because one does not have that many indices.

Actually all these descriptions for different q's are fully equivalent in two respects. First, if one defines energy-momentum tensors for the different q's, then the total energy is the same for all descriptions, although the

energy density is not the same. Similarly for complex fields, the total charge is the same, the charge density is not. They are also equivalent in another sense, as I am going to show later, namely that they all lead to the same equations of motion as was proved by Harish-Chandra. I mentioned, however, that actually for these equations of motion it is not possible to satisfy the conditions which are implied in all this for $f \geq 2$.

I define the energy-momentum tensor following Jauch and Fierz for $q \geq 1$, and therefore $f \geq 1$. I shall come back to spin zero later; the energy-momentum tensor for this is not a special case of the tensor for higher spin. We have

$$4\pi\, T_{\mu}^{(q)\nu} = (2\gamma^2)^{(1-q)} \left\{ - U^{(q)}_{\ldots[\sigma\mu]\ldots}\, U^{(q)\ldots[\sigma\nu]\ldots} \right.$$

$$+ \frac{1}{4}\, \delta_{\mu}^{\nu}\, U^{(q)}\, U^{(q)}$$

$$\left. + \gamma^2 \left(U^{(q-1)}_{\ldots\mu}\, U^{(q-1)\ldots\nu} - \frac{1}{2}\, \delta_{\mu}^{\nu}\, U^{(q-1)}_{\ldots\ldots}\, U^{(q-1)\ldots\ldots} \right) \right\}, \tag{13}$$

where contraction of the indices is implied by dots in the corresponding co- and contravariant positions. You will recognize that for $f = 1$ and $\gamma = 0$ this is just the Maxwell tensor.

Now I take the divergence and go through this in detail once, because we shall take divergences many times later, and the procedure is always the same. For the purposes of seeing what is required if I want to introduce interactions,

I will first take the divergence without making use of the field equations, but only using the definition (6) and the relations (10) following from the definition. We then have

$$4\pi\,\partial^{\mu}\,T^{(q)\nu}_{\mu} = (2\chi^{2})^{1-q}\Big\{-U^{(q)}_{\cdot\cdot[\sigma\mu]\cdot\cdot}\,\partial^{\mu}\,U^{(q)\cdot\cdot[\sigma\nu]\cdot\cdot}$$

$$-\partial^{\mu}\,U^{(q)}_{\cdot\cdot[\sigma\mu]}\,U^{(q)\cdot\cdot[\sigma\nu]} + \frac{1}{2}\,U^{(q)}_{\cdot\cdot}\,\partial^{\nu}\,U^{(q)\cdot\cdot}$$

$$+\chi^{2}\,\partial^{\mu}\,U^{(q-1)}_{\cdot\cdot\mu}\,U^{(q-1)\cdot\cdot\nu} + \chi^{2}\,U^{(q-1)}_{\cdot\cdot\mu}\,\partial^{\mu}\,U^{(q-1)\cdot\cdot\nu} -$$

$$-\,U^{(q-1)}_{\cdot\cdot}\,\partial^{\nu}\,U^{(q-1)\cdot\cdot}\Big\}.$$

Ekstein: Why does the q stand there (in Eq. 13)?

Havas: Because I have different energy-momentum tensors, depending on what field I am using. If I have a field with sufficiently many indices, I can describe this by different $U^{(q)}_{\cdot\cdot\cdot}$.

Ekstein: This is just a matter of definition?

Havas: An energy-momentum tensor is something which is a quadratic function of field quantities, and the divergence of which vanishes.

Hamermesh: Do you have a choice for the electromagnetic field?

Havas: No. For the electromagnetic field you have no choice. q is 1, there is only one $T^{\mu\nu}$ - you can't construct $T^{\mu\nu}$ (2) because there are not enough indices.

Teng: What does this mean physically?

Ekstein: This is really the question;according to Wentzel $T^{\mu\nu}$ are the only observables.

Havas: What I said was that for all these the integrated $T_{oo}^{(q)}$ is the same.

Ekstein: I still don't understand. Let us take one q, say q = 3. Then it pleases you to rewrite the $T^{\mu\nu\,(3)}$ in terms of $U^{(2)}$. This would be identical, but presumably this is not what you are doing.

Havas: No, the $T^{\mu\nu\,(3)}$ (formed with $U^{(3)}$ and $U^{(2)}$) and the $T^{\mu\nu\,(2)}$ (formed with $U^{(2)}$ and $U^{(1)}$ are not identical.

Ekstein: But could you not, by means of the definitions, express the $U^{(3)}$ and $U^{(2)}$ in terms of $U^{(2)}$ and $U^{(1)}$?

Havas: That I could. But if I get rid of $U^{(3)}$ in $T^{\mu\nu\,(3)}$, I get a tensor different from $T^{\mu\nu\,(2)}$, so I would have two different energy-momentum tensors.

Teng: So in this case you have another quantity which satisfies all the conditions?

Havas: Yes. Now you might ask, physically should not the energy density be unique? But for example in general relativity, which is certainly a legitimate field theory, the energy densities don't mean a thing. It is only the integrated energy density which means something. So this is not unique in field theory, that there are fields which allow density distributions which are different.

Ekstein: How about symmetry?

Havas: They are all symmetric

Ekstein: So there are infinitely many?

Havas: No, not infinitely many. The q goes only as high as you can go without running out of indices. So, for example, in the case of spin 1, there is only one. In case of spin 2 there are two.

Ekstein: So you would say that in the case of vector mesons $T^{\mu\nu}$ is not an observable?

Havas: In the case of vector mesons there is only one $T^{\mu\nu}$, so there is no difficulty.

Hamermesh: For spin f, you have f different energy-momentum tensors?

Havas: Yes.

Hamermesh: Except, of course, for spin 0?

Havas: Yes. For spin 0 you have one.

Ekstein: Is there any uniqueness statement implied in what you said? For instance, for vector mesons, do you say that it is impossible to construct any other tensor which is symmetric and whose divergence vanishes?

Havas: No, that is certainly not true, because you can always construct new energy-momentum tensors from a Lagrangian when you add a divergence to the Lagrangian. In that sense an energy-momentum tensor is never unique.

Ekstein: There is this matter of symmetrization. Wentzel discusses the possibility of adding something to the Lagrangian in order to symmetrize.

Havas: Yes, but even then you can still add a quantity with zero divergence.

Hack: You said that only the integral T_{oo} is unique, but this is a tensor. If you integrate T_{io}, should this not be the same?

Havas: Yes, it should.

Hack: So the total momentum is also the same?

Havas: Yes.

Concerning the ambiguities, I said before that all these energy-momentum tensors lead to the same equations of motion and the ambiguity disappears there. But as I said, it turns out that in the end you can't get any singularity introduced consistently at all. I think this impossibility of having point sources of fields of higher spin is one of the interesting results one can draw out of the classical theory.

But to go back to the divergence, the procedure is just as in the case of electrodynamics. By antisymmetrizing the first term with respect to σ and μ we can write for the first and third term

$$\frac{1}{2}\left(-\partial^{\mu}\, U^{(q)}{}_{..[\sigma\nu]..} + \partial^{\sigma}\, U^{(q)}{}_{..[\mu\nu]..} + \partial^{\nu}\, U^{(q)}{}_{..[\sigma\mu]..}\right) U^{(q)}{}_{..[\sigma\mu]..},$$

which vanishes because of Eq. (10). Making use of Eq. (6) we can write the last term

$$\chi^2 \left(U^{(q-1)}_{\ldots\sigma\ldots} \; U^{(q)\ldots[\sigma\nu]\ldots} - U^{(q-1)}_{\ldots\sigma\ldots} \; \partial^\sigma \; U^{(q-1)\ldots\nu\ldots}\right.$$

and the second term

$$\Box \, U^{(q-1)}_{\ldots\sigma\ldots} \; U^{(q)\ldots[\sigma\nu]} - \partial^\mu \; \partial_\sigma \; U^{(q)}_{\ldots\mu\ldots} \; U^{(q)\ldots[\sigma\nu]} \; .$$

Therefore, we finally have

$$4\pi \; \partial^\mu \; T_\mu^{(q)\nu} = (2\chi^2)^{1-q} \left\{\left[\Box \, U^{(q-1)}_{\ldots\sigma\ldots} + \chi^2 \, U^{(q-1)}_{\ldots\sigma\ldots}\right.\right.$$

$$\left.\left. - \partial^\mu \; \partial_\sigma \; U^{(q-1)}_{\ldots\mu\ldots}\right] U^{(q-1)\ldots[\sigma\nu]} + \chi^2 \, \partial^\mu \; U^{(q-1)}_{\ldots\mu\ldots} \; U^{(q-1)\ldots\nu}\right\} . \tag{14}$$

Clearly this is zero because of the Lorentz condition (11) and the wave equation (12).

I just want to make another remark about the energy-momentum tensor when the rest mass is zero. With the rest mass zero you would expect difficulties because of the factor $(2\chi^2)^{1-q}$ in Eq. (13). Actually then only $T_\mu^{(1)\nu}$ exists. However, then you have another non-uniqueness due to the gauge transformations; one can add a tensor of the form

$$N_{\alpha\beta\ldots\gamma_\delta} = \partial_\alpha \, C_{\beta\ldots\gamma_\delta} + \partial_\beta \, C_{\alpha\ldots\gamma_\delta} + \ldots + \partial_\delta \, C_{\alpha\beta\ldots\gamma}$$

with

$$\Box C_{\ldots} = 0,\ \partial^{\alpha} C_{\alpha\beta\ldots} = 0,\ C^{\beta}_{\beta\ldots} = 0,\ C \text{ symmetric},$$

to the potential without changing the field or the energy of the field.

For spin zero the energy-momentum tensor is not of the general form (13). In this case we have the wave equation

$$(\Box + \chi^2)\, U = 0; \tag{15}$$

we can define

$$\partial_{\mu} U = U_{\mu}$$

and then can write Eq. (15) as

$$\partial^{\mu} U_{\mu} + \chi^2 U = 0.$$

Then the energy-momentum tensor is

$$4\pi\, T^{\nu}_{\mu} = U_{\mu} U^{\nu} - \frac{1}{2}\, \delta^{\nu}_{\mu}\, (U_{\sigma} U^{\sigma} - \chi^2 U^2), \tag{16}$$

and for its divergence we get

$$4\pi\, \partial^{\mu} T^{\nu}_{\mu} = (\Box U + \chi^2 U)\, U^{\nu}, \tag{17}$$

which again is zero if the wave equation is satisfied.

III. NEUTRAL FIELDS WITH SOURCES

I want to talk about spin 0 first because in this case it is quite easy to see what you should do if you want to introduce interactions; as usual you want

the divergence of the energy-momentum tensor to become an expression which is bilinear in the source density ρ and the field U. For this you just have to take

$$(\Box + \chi^2)\, U = 4\pi \rho\,. \tag{18}$$

Then Eq. (17) becomes

$$\partial^{\mu}\, T_{\mu}^{\ \nu} = \rho\, U^{\nu}_{\cdot} \tag{19}$$

This seems to be the only way in which one can sensibly introduce an interaction in the scalar case. In the other cases, the situation is a little bit more complicated. We have before us some equations such as (9) and (10) which only depend on the definition of (6) of $U^{(q)}$ and therefore can not be changed. For our present purposes the easiest thing to do is to introduce the source density in Eq. (7). We define in analogy to Eq. (6) for all $q \leq f$

$$\rho^{(q)}_{\ldots[\alpha\beta]\ldots} = \partial_{\alpha}\, \rho^{(q-1)}_{\ldots\beta\ldots} - \partial_{\beta}\, \rho^{(q-1)}_{\ldots\alpha\ldots} \tag{20}$$

and replace Eq. (7) by

$$\partial^{\alpha}\, U^{(q)}_{\ldots[\alpha\beta]\ldots} + \chi^2 U^{(q-1)}_{\ldots\beta\ldots} = 4\pi\, \rho^{(q-1)}_{\ldots\beta\ldots}\,. \tag{21}$$

Contracting this equation with ∂^{β} I get

$$\chi^2\, \partial^{\beta}\, U^{(q-1)}_{\ldots\beta\ldots} = 4\pi\, \partial^{\beta}\, \rho^{(q-1)}_{\ldots\beta\ldots}\,. \tag{22}$$

This expression is zero outside the singularities and so the Lorentz condition, which you need so that you have only quanta of definite spin in the field, is

maintained outside the singularities. We also need a similar relation for the traces.

However, I want to make two remarks: First of all, if $\chi = 0$, you get from Eq. (20)

$$\partial^{\beta} \rho^{(q-1)}_{..\beta..} = 0, \tag{23}$$

which means that for zero rest mass of the field you must have a conservation law for the source density. Then, it looks here as if you don't need Eq. (23) for $\chi \neq 0$. I can't show the details [3] because it would take too long, but it turns out that actually you have to assume Eq. (23) for $f \geq 2$, at least for point sources. But for spin 1 you definitely can leave Eq. (22) unchanged and, as I shall discuss later (in Sec. VII), this does not impose any restriction. So actually instead of distinguishing in the equations that for spin ≥ 2, $\partial^{\beta} \rho_{..\beta..}$ equals 0, and for spin 1 it does not. I shall write the same formulas all the time and just imply that if you have more than one index Eq. (23) must hold.

From Eqs. (21) and (22) we get

$$(\Box + \chi^2)\, U^{(q-1)}_{..\beta..} = 4\pi \left(\rho^{(q-1)}_{..\beta..} + \frac{1}{\chi^2}\, \partial_{\beta} \partial^{\alpha} \rho^{(q-1)}_{..\alpha..} \right) \tag{24}$$

and then the divergence of the energy-momentum tensor (14) becomes

$$\partial^{\mu}\, T^{(q)\nu}_{\mu} = (2\chi^2)^{1-q} \left\{ \rho^{(q-1)}_{..\sigma..}\, U^{(q)..[\sigma\nu]} + \partial^{\mu}\, \rho^{(q-1)}_{..\mu..}\, U^{(q-1)..\nu} \right\}. \tag{25}$$

IV. CHARGE-SYMMETRIC FIELDS

The only other topic I want to discuss today is the question of charge-symmetric fields. As you know, charge-symmetric fields have been introduced first in quantum theory by Kemmer.[4] An analogous classical theory was investigated later by LeCouteur.[5] One considers a "charge space" and takes all quantities as three-component vectors in this charge space. The third component is supposed to represent neutral fields, and the first and second together the charged field. We can regain the neutral theory by only considering the third component of all our equations, or we can regain the charged theory by setting the third component equal to zero. Fierz wrote the charged fields in terms of complex quantities, which amounts to the same thing. Actually I have nothing to rewrite in what I did here. All I have to do is to put little arrows under all field quantities and under all the source densities to imply that I have a three-component vector in charge space. Every time I have a product I replace it by a scalar product in charge space.

The new feature, of course, comes in because you can define a new quantity which is conserved.

Ekstein: Two new quantities which are conserved.

Havas: Namely?

Ekstein: The isotopic spin and the charge.

Havas: We have no particles as yet -- just the pure field.

Ekstein: Even then, in any charge theory, charge-symmetric or not, you must have current conservation. In addition, if you have charge

symmetry you must have isotopic spin invariance.

Havas: This is implied because I really get a conservation law which is too wide. The strictly charge-conserving equation would be a statement on the third component in charge space of the charge-current density vector, but what one gets in this approximation is a statement on all three components; so you are right.

Now let us consider spin zero first and just write down the appropriate definitions. You can define the "charge-current density" which is a four-vector in four space, and also a three-vector in charge space. We take

$$4\pi \underset{\rightarrow}{j}^{\nu} = e \underset{\rightarrow}{U} \wedge \underset{\rightarrow}{U}^{\nu} \tag{26}$$

(and just the third component of this in charged theory). Here $\wedge$ means vector product in charge space, and e is a constant. For the divergence we get, using Eq. (18)

$$\partial_{\nu} \underset{\rightarrow}{j}^{\nu} = -e \underset{\rightarrow}{\rho} \wedge \underset{\rightarrow}{U} \ . \tag{27}$$

One can define analogous vectors for spin $f > 0$ by

$$4\pi \underset{\rightarrow}{j}^{(q)\nu} = e(2\chi^2)^{1-q} \left\{ \underset{\rightarrow}{U}^{(q-1)}_{\ldots\sigma\ldots} \wedge \underset{\rightarrow}{U}^{(q)\ldots[\sigma\nu]\ldots} \right\}, \tag{28}$$

where, as remarked before, all different $j^{(q)}$ give the same total charge.

Then

$$\partial_\nu \underset{\rightarrow}{j}^{(q)\nu} = e\,(2\chi^2)^{1-q}\ \underset{\rightarrow}{\rho}_{\ldots\sigma\ldots}^{(q-1)} \wedge \underset{\rightarrow}{U}^{(q-1)\ldots\sigma\ldots} \qquad (29)$$

Teng: Does physics specify which q to take?

Havas: At this stage, no. In the end physics will tell us.

Kaplan: What is the point then?

Havas: People have been inventing all kinds of theories of elementary particles. They found out after a while that the conditions they applied in either classical or quantum theory are not stringent enough. You can build up theories of all kinds of spin. I am going through it because here is a case where one can get rid of them again. In most other cases one is just building up and then does not seem to have any selection and then one doesn't know why one does not find a particle of spin 260.

Teng: I was thinking of the electromagnetic case.

Havas: In the electromagnetic case, there is just one case anyhow, q = 1.

Teng: What do you call the potentials in that case?

Havas: The potentials are the thing with one index. From the potentials you define $U^{(1)}$, which is the electromagnetic field. Then you can't do anything anymore because there are no free indices.

Teng: The interactions can never be through the potentials? They always have to be through the fields?

Havas: You mean in the electromagnetic case?

Teng: In any case.

Havas: It is really through both. I introduced it through Eq. (21), which involves both of them. Actually, except for fields of rest mass zero, the potentials and the fields enter more or less symmetrically, as Kemmer has stated already in his original article on meson theory. It is just a matter of prejudice from electrodynamics that you think of the fields as more fundamental, because once you have rest mass non-zero, the fields and the potentials are related symmetrically, if you want to write the theory as first-order differential equations. The field is related to the first derivative of the potential through Eq. (4), and the potential is related to the first derivative of the field through Eq. (5).

Lecture 2.

V. PROPERTIES OF THE SOLUTIONS OF THE GENERALIZED WAVE EQUATION

The essential result obtained last time was that we had to deal in the end with some generalized inhomogeneous wave equations. Now the next thing to do before we can obtain equations of motion is to discuss the general properties of solutions of this equation. The important feature is that one can make statements on which particular solutions (including their derivatives) are finite and which are not.[6] One needs this later for the equations of motion. Let us consider the equation

$$(\Box + \chi^2)\, 0_{\ldots} = 4\pi\, \rho_{\ldots} \,. \tag{30}$$

As you all know the Green's functions for this from quantum theory of wave fields, I won't spend any time on deriving them. There are several different possible ways of choosing Green's functions, and the ones I shall need are the retarded one, the advanced one, and the symmetric one. The retarded one is

$$\Delta_r = \begin{cases} \frac{1}{2\pi}\,\delta(u^2) - \frac{\chi}{4\pi u}\, J_1(\chi u) & u_o > u_s \\ 0 & |u_o| < u_s \\ 0 & u_o < -u_s \end{cases} \tag{31}$$

The advanced one is

$$\Delta_a = \begin{cases} 0 & u_o > u_s \\ 0 & |u_o| < u_s \\ \frac{1}{2\pi}\,\delta(u^2) - \frac{\chi}{4\pi u}\, J_1(\chi u) & u_o < u_s \end{cases} \tag{32}$$

The symmetric one is

$$\Delta = \frac{1}{2} (\Delta_r + \Delta_a). \tag{33}$$

In the limit of $\chi \to 0$ these functions reduce to the ordinary Green's functions D_r, D_a and D of quantum electrodynamics. In the above

$$u_\mu = x_\mu - x_\mu', \qquad u = (u_\mu u^\mu)^{1/2}$$

$$u_s = (u_k u^k)^{\frac{1}{2}} \qquad (k = 1, 2, 3).$$

In the inhomogeneous equations, the inhomogeneities consist in general of a sum of terms, but for our present purposes we assume that there is just a single inhomogeneity of the form

$$\rho \ldots(x) = \int_{-\infty}^{\infty} S\ldots(\tau)\ \delta(s^0)\,\delta(s^1)\,\delta(s^2)\,\delta(s^3)\,d\tau$$

with

$$s^\mu = x^\mu - z^\mu(\tau), \tag{34}$$

where τ is the proper time as we defined it last time, $S_{\ldots}$ is a tensor which is again symmetric just as the field tensor was, and the delta is the Dirac delta function. From now on I shall denote this four-fold delta function as $\delta^4(s)$. Then we can obtain immediately the "fundamental retarded solution"

$$O^r_{\ldots} = 4\pi \int \Delta_r(x, x')\,\rho(x')\,d^4x', \tag{35}$$

where d^4x' is the four- dimensional volume element for the x' system.

This we can immediately reduce to a simpler form. First of all there is a delta function in $\rho(x')$, which on integration changes $\Delta_r(x,x')$ to $\Delta_r(x,z)$. Therefore the first term in Eq. (35) reduced to

$$\frac{1}{2}\int_{-\infty}^{\tau_r} \delta(s^2)\, S_{\ldots}(\tau)\, d\tau \ . \tag{36}$$

For the second term in Eq. (33) we can again integrate over the delta function and get

$$-\chi \int_{-\infty}^{\tau_r} S_{\ldots}(\tau)\, \frac{J_1(\chi s)}{s}\, d\tau \ ,$$

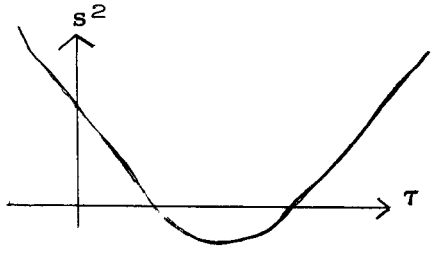
where

$$s = (s_\mu\, s^\mu)^{1/2}$$

and the retarded point τ_r and the advanced point τ_a are defined by

$$s = 0, \quad s_o > 0 \ \text{ or } \ s_o < 0, \tag{37}$$

respectively. Now Eq. (36) can be reduced further by a trick due to Dirac. Everything would be simple here if the variable of integration would be the same as the variable in the delta function, so we will just make the switch to that; we want to replace $d\tau$ by ds^2, so we have to multiply and divide by $ds^2/d\tau$. Now s^2 as a function of τ is as shown in the figure,



so the derivative in the region of interest is negative. Therefore, if I want to keep the lower limit, corresponding to the smaller value of the variable, I have to introduce a minus sign. Then I have

$$-\frac{1}{2}\int \frac{\delta(s^2)\, S_{\ldots}(\tau)}{ds^2/d\tau}\, ds^2 .$$

The denominator depends on τ through z, the position of the particle, and therefore

$$\frac{ds^2}{d\tau} = -2 s_\mu \frac{dz^\mu}{d\tau} = -2 s_\mu v^\mu .$$

We define

$$K = s_\mu v^\mu$$

and write expression (36) as

$$\int \frac{\delta(s^2)\, S_{\ldots}(\tau)}{K}\, ds^2 ,$$

which equals

$$\frac{S}{K} ,$$

to be evaluated at $s^2 = 0$. Since I used the retarded delta function (31), the zero corresponding to the retarded time has to be picked. Therefore, we get

$$O^r_{\ldots}(x) = \left(\frac{S_{\ldots}}{K}\right)_r - \int_{-\infty}^{\tau_r} S_{\ldots}(\tau)\; J_1\; \frac{(\chi s)}{s}\, d\tau \qquad (38)$$

for the fundamental retarded solution.

In the same way we could work out the advanced potential; the difference here, first of all, is that you get a minus sign, that is $-S/k$, evaluated at the advanced point, minus the integral going from the advanced time to plus infinity, i.e.,

$$O^a_{\ldots}(x) = -\left(\frac{S_{\ldots}}{K}\right)_a - \chi \int_{\tau a}^{\infty} S_{..}(\tau)\ \frac{J_1(\chi s)}{S}\, d\tau \ . \tag{39}$$

This is the "fundamental advanced solution".

In the case of electrodynamics you would only get the first term in Eqs. (38) and (39) and this then is just the usual retarded potential and advanced potential in four-dimensional notation.

I just considered the fundamental solutions, but in general we can have some more complicated expressions since we started out from a linear equation. Obviously when I differentiate on both sides I still get solutions of the inhomogeneous wave equation; I can contract over indices and still get solutions, and I can add solutions. So I'll denote the differential operator which does all that by $\mathcal{D}$ and we have

$$U^r_{..} = \mathcal{D}(O^r_{.}) \ , \ U^a_{\ldots} = \mathcal{D}(O^a_{..}) \ , \tag{40}$$

for the general retarded and advanced potentials of a point particle.

For these differentiations I'll note a few relations for future use. For differentiation within an integral, τ is just a variable of integration. In that case we get from the definitions

$$\partial_\nu\ s_\mu\ (\tau) = \eta_{\mu\nu} \ , \quad \partial_\nu\ s(\tau) = \frac{s_\nu}{s} \ . \tag{41}$$

Outside an integral you have to worry about dealing with a retarded point or an advanced point. Let's do it first for the retarded point. We have then

$$\partial_\nu s_\mu (\tau_r) = \eta_{\mu\nu} - v_\mu (\tau_r) \partial_\nu \tau_r ,$$

and I have to evaluate the last term. Contracting with s^μ, I get

$$s^\mu \partial_\nu s_\mu (\tau_r) = s_\nu - K \partial_\nu \tau_r .$$

On the other hand, for the retarded point I have

$$s^\mu s_\mu = 0$$

and if I differentiate this I get just the left-hand side of the previous equation. Therefore

$$\partial_\nu \tau_r = \frac{s_\nu}{K} \tag{42}$$

and I get finally

$$\partial_\nu s_\mu (\tau_r) = \eta_{\mu\nu} - v_\mu \frac{s_\nu}{K} , \tag{43}$$

all of which has to be evaluated at the retarded time. In the same way I get

$$\partial_\nu K (\tau_r) = v_\nu - (1 - K') \frac{s_\nu}{K} , \tag{44}$$

where

$$K' \equiv s_\mu \dot{v}^\mu .$$

For any function of τ alone, I get from Eq. (42)

$$\partial_\nu f(\tau_r) = \dot{f}\, \partial_\nu \tau_r = \dot{f}\, \frac{s_\nu}{K} \quad . \tag{45}$$

Now it is important to note for what is coming later that in the differentiation I never made use of the fact that I was dealing with a retarded point, as I never had to worry about the sign of s_o. Therefore if I would do the same for the advanced point I would end up with final answers of the same form as Eqs. (42)-(45), except that they would have to be evaluated at the advanced point.

The essential point for the theory is that one can make quite general statements on the behavior of the solutions as one approaches the world line and tries to evaluate them on the world line. This I can't go through in detail; it involves some not very difficult, but sometimes lengthy, calculations. One can prove the following statements: If I define the "fundamental radiation solution" by

$$O^{rad}_{\ldots} = \frac{1}{2}\,(O^r_{\ldots} - O^a_{\ldots}) = \frac{1}{2}\left(\frac{S_{\ldots}}{K}\right)_r + \frac{1}{2}\left(\frac{S_{\ldots}}{K}\right)_a$$

$$- \frac{1}{2}\,\chi\left[\int_{-\infty}^{\tau_r} - \int_{\tau_a}^{\infty}\right] S_{\ldots}\, \frac{J_1(\chi s)}{s}\, d\tau \ , \tag{46}$$

then it can be shown that this quantity and also all its derivatives are finite as we approach the world line. Later I will give explicit formulas for the first few expressions. I also want to point out, to avoid confusion, that what I call

the radiation solution is half of what Dirac and Bhabha call the radiation solution, but actually it is the quantity (46) which enters the equations of motion as a radiation damping term.

Ekstein: How can one make a general statement about finiteness without specifying what $S_{...}$ is?

Havas: It has nothing whatsoever to do with $S_{...}$, except that S as a function of τ and all its derivatives should be finite. It is not the S in (46) which causes the trouble, but the K because it vanishes as one approaches the world line. So one has to go through some expansion powers of K and show that everything stays finite. Actually, this goes for the separate parts too. The part involving the integrals stays finite and the other part, by itself, also stays finite.

Tanaka: Does $S_{...}$ become infinite on the world line?

Havas: $S_{...}$ is only defined on the world line to begin with -- it is a function of the proper time.

Ekstein: How can you make statements about the differentiability of an integral without knowing what is in the integral?

Havas: The argument really goes the other way around. $S_{...}$ as a function of τ should be determined once one has equations of motion. I could only say something about how $S_{...}$ will behave at $\tau = +\infty$ or $-\infty$ if I had the equations of motion solved. So at this point I can make all the assumptions I want about reasonable behavior and if I then came up

with a set of equations of motion which lead to unreasonable behavior I would be in trouble. Therefore this is really a question which can not be answered here.

Now if instead of the difference I take the sum of the potentials, I get what is called the "fundamental symmetric solution"

$$O^{sym}_{\dots} = \frac{1}{2}\,(O^{r}_{\dots} + O^{a}_{\dots}) = \frac{1}{2}\left(\frac{S_{\dots}}{\kappa}\right)_r - \frac{1}{2}\left(\frac{S_{\dots}}{\kappa}\right)_a$$

$$- \frac{\chi}{2}\left[\int_{-\infty}^{\tau_r} + \int_{\tau_a}^{\infty}\right] S_{\dots}\,\frac{J_1(\chi s)}{s}\,d\tau\,. \qquad (47)$$

This is singular on the world line.

The integral

$$\int_{-\infty}^{\infty} S_{\dots}\,\frac{J_1(\chi s)}{s}\,d\tau \qquad (48)$$

by itself is a solution of the homogeneous equation because

$$(\Box + \chi^2)\int_{-\infty}^{\infty} S_{\dots}\,\frac{J_1(\chi s)}{s}\,d\tau = \int_{-\infty}^{\infty} S_{\dots}(\Box + \chi^2)\,\frac{J_1(\chi s)}{s}\,d\tau\,,$$

and this is 0 since the term involving the Bessel function just satisfies the required differential equation. But as the integral (48) by itself is a solution of the homogeneous equation, I can add or subtract it from any solution of the inhomogeneous equation without changing the character of this solution.

I shall now define two new quantities: The "modified radiation solution"

$$O'^{\mathrm{rad}}_{\ldots} = O^{\mathrm{rad}}_{\ldots} - \frac{\chi}{2} \int_{-\infty}^{\infty} \ldots = \frac{1}{2}\left(\frac{S_{\ldots}}{K}\right)_r + \frac{1}{2}\left(\frac{S_{\ldots}}{K}\right)_a$$

$$- \frac{\chi}{2}\left[\int_{-\infty}^{\tau_r} + \int_{-\infty}^{\tau_a}\right] S_{\ldots} \frac{J_1(\chi s)}{s}\, d\tau\,, \tag{49}$$

and the "modified symmetric solution"

$$O'^{\mathrm{sym}}_{\ldots} = O^{\mathrm{sym}}_{\ldots} + \frac{\chi}{2} \int_{-\infty}^{\infty} \ldots = \frac{1}{2}\left(\frac{S_{\ldots}}{K}\right)_r - \frac{1}{2}\left(\frac{S_{\ldots}}{K}\right)_a$$

$$+ \frac{\chi}{2} \int_{\tau_r}^{\tau_a} S_{\ldots} \frac{J_1(\chi s)}{s}\, d\tau\,. \tag{50}$$

Of course, the sum of Eqs. (49) and (50) is the same as the sum of Eqs. (46) and (47); namely the retarded potential. Now the reason for these definitions is the following: Eq. (49) is symmetric in r and a, and Eq. (50) is antisymmetric in r and a, while Eqs. (46) and (47) have no such simple properties. One can prove the statement about the energy-momentum tensor which I am going to make later, just on the basis of this symmetry.

As I said before, the radiation solution is finite on the world line; I shall not go through the evaluation of it. There are essentially two ways in which one can do it. One is due to Harish-Chandra [7] who developed the method originated by Dirac. [8] He uses an expansion in powers of K and then goes to the limit

$K \to 0$. It is a rather long calculation if one wants to go beyond the first derivative. The other is the method of Riesz,[9] developed first for electrodynamics. He treats the differential equations one really wants to consider by analytic continuation from a different set, which behaves decently. Either way, you get the same answers, at least as far as investigated. One can't say much in general about the integrals in Eqs. (46) or (49), but for the other part you get the following results.

$$\frac{1}{2}\left(\left(\frac{S}{K}\right)_r + \left(\frac{S}{K}\right)_a\right)_o = -\dot{S}_o \ ,$$

$$\frac{1}{2}\left(\partial_\rho \left(\frac{S}{K}\right)_r + \partial_\rho \left(\frac{S}{K}\right)_a\right)_o =$$

$$-\left\{\frac{1}{3} S(\ddot{v}_\rho - v_\rho v_\sigma \dddot{v}^\sigma) + 2\dot{v}_\rho \dot{S} + 2 v_\rho \ddot{S}\right\}_o . \qquad (51)$$

One can continue for higher derivatives, but the process gets extremely cumbersome. Also, one may have to evaluate derivatives of the integrals, which involve derivatives of the limits. One also needs expressions like

$$\frac{1}{2}\left(\partial_\rho\left(\frac{S s_\sigma}{K}\right)_r + \partial_\rho\left(\frac{S s_\sigma}{K}\right)_a\right)_o = \left\{-\eta_{\rho\sigma}\dot{S} + \dot{v}_\rho v_\sigma S + \dot{v}_\sigma v_\rho S + 2 v_\rho v_\sigma \dot{S}\right\}_o .$$

(52)

All these expressions are needed because the equations of motion involve the modified radiation solution evaluated at the position of the particle.

VI. DIRAC'S METHOD FOR OBTAINING EQUATIONS OF MOTION

The next question is how to get equations of motion. It might be thought that it is sufficient to equate mass times acceleration to certain finite interaction terms. Although this works in some cases, it does not in general. This is the procedure used by Riesz[9] in electrodynamics, where it happens to be right. He didn't do the case of a scalar field where it would lead to inconsistencies. Thus he didn't do anything wrong, but this isn't a general method -- just something that happens to work in this simple form in some cases.

Dirac,[8] on the other hand, started from the law of conservation of energy and momentum, which is

$$\partial_\nu T_\mu^{\ \nu} = 0$$

when there are no singularities present, and wanted to maintain such a conservation law for the entire system, even in the presence of singularities.

I shall first describe Dirac's method and then return to some criticisms. Dirac's idea is to take the world line of the particle and surround it by a thin tube, the radius of which one ultimately makes to tend to zero, and then one calculates the flow of energy and momentum into the tube from the outside, which can be done just by using the $T_{\mu\nu}$ for the field alone. Then one finds the equations of motion from the requirement of conservation, which implies that the integral of the flow into the tube between two times τ_1 and τ_2 should be expressible as a difference of two terms at the beginning and at the

end. This integral equals

$$\int_{\tau_2}^{\tau_1} T^{\mu\nu} \, S_\nu \ ,$$

because you start out with the divergence of $T^{\mu\nu}$ integrated over some volume element, and this you can transform into a surface integral.

Now Dirac stipulates that the limit of the flow into the four-dimensional region should just be the difference of two quantities $A^\mu(\tau_2) - A^\mu(\tau_1)$. Another way of saying this is that

$$\frac{d}{d\tau} \lim \int_{\tau_1}^{\tau} T^{\mu\nu} \, dS_\nu = \dot{A}^\mu(\tau) \ , \tag{53}$$

i.e., a certain quantity should be a perfect differential.

Dirac himself applied that to the energy-momentum tensor alone. The mathematics is exactly the same for any tensor, just as long as there is a conservation law. The method was extended to conservation of angular momentum by Bhabha,[11] and then Bhabha and Harish-Chandra[12] generalized the argument mathematically to all tensors for which one has conservation laws. The results obtained also follow from a mathematically different way of proceeding, which is also based on the existence of a conservation law, which id due to Mathisson;[10] the methods are entirely equivalent.

It is quite easy to see that the information you get, just as long as you finally let the radius go to zero, does not depend on what tubes you started out with, so that the difference of the expressions you calculated by having two

different tubes, can at most be a perfect differential. Thus, you get unique information out of this method. This information by itself, of course, does not help you because the trouble is not in getting equations of motion, the trouble is in getting finite equations of motion; if you calculate (53) explicitly, of course, you will find that the derivative of this limit is infinite. To get finite equations of motion one has to show that whatever becomes singular as you go to the limit is by itself also a perfect differential. Once you have established that, then you can say that you didn't know what A^{μ} should be in the first place, and so whatever singular perfect differential you have on the left in (53) can be compensated by an appropriate choice of A^{μ}, so that the difference is finite. It can be shown in general that whatever terms you get in (53) which depend on the radius of the tube and become singular as you go to zero with the radius of the tube, are identically perfect differentials;[12] therefore one does not have to consider them anymore. The procedure may be thought of as a "renormalization" of A^{μ} although the "unrenormalized" A^{μ} need never be considered.

The next question is, "What is left over?" The answer depends on the form of the energy-momentum tensor. First of all, when I wrote down the energy-momentum tensors (13) and (16) I wrote them as functions of the field but didn't say what the field was. There is a choice there because there are several Green's functions for the wave equations. What you should put in is the total field, but you can either take it as the retarded field plus an external field, or as the symmetric field plus the external field, or you could take any

combination of retarded and advanced fields for which the coefficients add up to one, since it would still be a solution of the inhomogeneous equation. I shall just consider the first two cases. The calculation is the same for the different fields, but the final equations of motion depend on what you assumed the total field to consist of in the beginning.

In the retarded case I can write identically

$$U^{\text{total}} = U^{\text{ret}} + U^{\text{ext}} = U^{\text{sym}} + U^{\text{rad}} + U^{\text{ext}}$$

from the definitions, and this I can also write as

$$U^{\text{total}} = U'^{\text{sym}} + U'^{\text{rad}} + U^{\text{ext}} \tag{54}$$

because of the way the modified fields are defined. In the symmetric case I can write, using Eq. (50),

$$U^{\text{total}} = U^{\text{sym}} + U^{\text{ext}} = U'^{\text{sym}} + \int\ldots + U^{\text{ext}}. \tag{55}$$

Now you see that both expressions contain U'^{sym}, which is singular. The other quantities on the right-hand side of Eqs. (54) and (55) are all solutions of the homogeneous equation. So in both cases I put

$$U^{\text{total}} = U'^{\text{sym}} + U'^{\text{mean}}, \tag{56}$$

where I define the "modified mean field" by

$$U'^{\text{mean}} \equiv \begin{cases} U'^{\text{rad}} + U^{\text{ext}} \text{ in the retarded case} \\ \int\ldots + U^{\text{ext}} \text{ in the symmetric case.} \end{cases}$$

I shall just talk about U'^{mean} from now on and won't have to distinguish the two cases.

The argument we shall use depends on the fact that all the energy-momentum tensors $T_{\mu\nu}$ which we considered are homogeneous quadratic functions of potentials and field strengths U. I shall now introduce the notation $T_{\mu\nu}(U^A, U^B)$ to denote an expression obtained from $T_{\mu\nu}$ by replacing one U in each term by U^A and the other by U^B and then symmetrizing. So instead of having an expression which is quadratic in the U's, I get an expression which is bilinear in U^A and U^B and symmetric in U^A and U^B. With this notation I could write my original $T_{\mu\nu}$ as

$$T_{\mu\nu}(U^{total}, U^{total}).$$

This is just an awkward way of writing the usual energy-momentum tensor, but now I can substitute Eq. (56) and obtain

$$T_{\mu\nu} = T_{\mu\nu}(U'^{sym} + U'^{mean}, U'^{sym} + U'^{mean}) =$$

$$= T_{\mu\nu}(U'^{sym}, U'^{sym}) + 2T_{\mu\nu}(U'^{sym}, U'^{mean}) + T_{\mu\nu}(U'^{mean}, U'^{mean}). \tag{57}$$

What I have to do according to the Dirac method is to find the flow of energy and momentum in Eq. (53) by using these tensors and then to go to the limit of shrinking the radius to zero and taking the derivative. The surface integral in (53) came, of course, from the divergence, but as the last term in

Eq. (57) is just made up of solutions of the homogeneous equations, its divergence is zero and thus it won't contribute. The first term $T_{\mu\nu}(U^{sym}, U'^{sym})$ is singular -- very much so -- but it can be shown to give a flow which is identically a perfect differential.[6] This can be shown simply from the fact that U'^{sym}, as I discussed before, is antisymmetric in the retarded and advanced points. That is all which enters into the proof, but it is just a little too long to write out. Being a perfect differential it can be taken care of in A^{μ} as discussed before. Therefore only $T_{\mu\nu}(U'^{sym}, U'^{mean})$ is left. If one calculates its divergence, and takes into account that U'^{mean} satisfies the homogeneous equations, one finally gets an expression of the form of an inhomogeneity multiplied by some function of the modified mean field. But from what I said in Sec. V, the modified mean field is finite and all its derivatives are finite. This is as far as one can get in general, and now one has to consider the specific forms of the energy-momentum tensors which we had in Sec. II.

When I start out from the energy-momentum tensor in Eq. (53) it will give me the translational equations of motion. When I do the corresponding calculations with the angular momentum tensor, I get the rotational equations of motion. The question which arises is, Which other tensors should be considered?" Here I think the Dirac method is ambiguous. The Dirac method, as generalized by Bhabha and Harish-Chandra,[12] starts out as if it were a mathematical prescription. It says, "Take whatever conservation laws you have for the free fields and then require them to persist in the presence of singularities." This, however, is too much, because often there are more

conservation laws for free fields than you are able to satisfy when you have singularities present, and also more than you would physically expect to mean something.[13] Therefore one has to make a selection somewhere on what one really wants to consider; so this turns out not to be just a mathematical recipe to get equations of motion, but one must pick out what one considers to be physically significant in the laws for the free fields and then proceed from these to get equations of motion.

Of course, it would be preferable to have a method in which one does not have to impose arbitrarily what one wants to keep and what one does not want to keep. Actually this kind of selection is automatic if, instead of proceeding Dirac's way, one proceeds from the linearized equations of general relativity.[14] Essentially this selection is due to the fact that in the general theory or relativity the over-all conservation of energy-momentum and the conservation of angular momentum are built in. So you can take the basic equations and obtain from them the equations of motion without any ambiguity and without any arbitrary imposition of conditions. One can show in general that one gets the same equations of motion this way as from the Dirac method, provided that one restricts oneself in the latter to conservation of energy-momentum of angular momentum, and of charge.

I shall now indicate very briefly, following Harish-Chandra[7] (compare also reference 3), the calculations leading to the equations of motion for the specific form (13) of the energy-momentum tensors. I'm just going to consider

the first term in the divergence (14) for lack of time. There is another term which involves the divergence of the source density, but for the fields of spin two or higher that doesn't enter anyhow, as I discussed before, so I am not going to evaluate it. For spin one it does matter, but there I shall just give you the final answer. The important feature I do want to show is that you can work your way through back from the q you started out from, down to 1 and end up with the same equations of motion, regardless of q. From what I said before, in the end the only field which matters is the U'^{mean} in Eq. (56); this being understood, I shall just write U again.

To evaluate Eq. (53) we take our tube surrounding the world line to be bounded by a mantle Q and by two space-like surfaces A and B, with A later than B. Then, if I take the outward normal for Q and the future normals for A and B, the flow into our region is given by

$$\int_V \partial_\nu \; T^{\mu\nu} \; d^4 x \; = \; - \int_Q T^{\mu\nu} \; dS_\nu \; + \int_A T^{\mu\nu} \, dS_\nu \; - \int_B T^{\mu\nu} \; dS_\nu \, . \tag{58}$$

I won't have to worry about the integrals over A and B because in the limit when you shrink the tube down to zero, these surface integrals all reduce to expressions which only depend on the time τ at the points where A and B intersect the world line and thus correspond to perfect differentials. Therefore we can forget about them and only have to evaluate the negative of the integral over Q. If I just substitute into it the first term from Eq. (14) I get from

Eq. (58)

$$-(2\gamma^2)^{1-q} \int_V \rho^{(q-1)}_{\ldots\sigma} U^{(q)\ldots[\sigma\mu]} d^4x . \tag{59}$$

Now the general idea is the following: $\rho^{(q-1)}$ and $U^{(q)}$ are defined in terms of lower order fields by certain derivatives. I can substitute these definitions and integrate by parts until I am at q = 1. For the integrated terms, the limits are the surfaces Q, A or B. At Q one is outside the singularity and therefore whatever has been integrated out won't contribute, because the source density vanishes on Q. Whatever one gets at A and B are integrals like those in Eq. (58) which we just discussed and which again can be discarded. So I can forget about the integrated terms. I won't go through the remaining details, but if you put in the definitions (6) and (20) and make use of the generalized wave equation (12) it turns out that at each step you go simultaneously from $\rho^{(q-1)}$ to $\rho^{(q-2)}$, and also from $U^{(q)}$ to $U^{(q-1)}$, and also get a factor of $2\gamma^2$. Continuing like this you end up with

$$-\int \rho_{\ldots} U^{(1)\ldots[\sigma\mu]} d^4x . \tag{60}$$

Therefore, whatever q you start out from, you end up with the same translational equations of motion. The same thing happens when you consider the angular momentum tensor -- you define the angular momentum tensor $M^{(q)}_{\mu\nu\lambda}$ in terms of the $T^{(q)}_{\mu\nu}$ and go through the same procedure, reduce it

down all the way to $M^{(1)}_{\mu\nu\lambda}$ and end up with the same rotational equations of motion.

Now we assume for the source densities the form

$$\rho_{\mu\nu\ldots} = \sum_n \partial^{\alpha\beta\ldots} \mathcal{S}_{\alpha\beta\ldots,\,\mu\nu} \,, \tag{61}$$

which is equivalent to a multipole expansion, and then, since $\partial^{\alpha\beta\ldots}$ is symmetric, I can take $\mathcal{S}_{\alpha\beta,\,\mu\nu\ldots}$ to be symmetric in $\alpha\beta\ldots$ by themselves; it also has to be symmetric in $\mu\nu\ldots$ separately from the symmetry character of the tensor fields, but between the two sets of indices there is no connection. The $\mathcal{S}$ is taken to be of the form

$$\mathcal{S}_{\alpha\beta\ldots,\mu\nu\ldots} = \int_{-\infty}^{\infty} S_{\alpha\beta\ldots,\mu\nu\ldots}\, \delta^4(s)\, d\tau \,. \tag{62}$$

We substitute Eq. (61) with (62) into (60) and then we again go through integrations by parts, getting rid of the n derivatives in (61) and transferring them over to the $U^{(1)}$. Then, using (53), we finally get

$$\dot{A}^{\mu} = (-1)^{n+1}\, S_{\alpha\beta\ldots,\,\ldots\sigma\ldots}\, \partial^{\alpha\beta\ldots} U^{(1)}{}_{\ldots[\sigma\mu]} \,. \tag{63}$$

If I had carried along all the terms of Eq. (14) I would have obtained an additional term

$$(-1)^n\, S_{\alpha\beta\ldots,\,\ldots\sigma\ldots}\, \partial^{\sigma\alpha\beta\ldots} U^{\mu} \,, \tag{64}$$

which is of importance only for f = 1. A^{μ} is the momentum of the particle.

This was for neutral fields, and for charge-symmetric fields we would just have scalar products of the corresponding vectors in charge space in these expressions. This is the translational equation of motion; for the rotational equations of motion you get some complicated expression equal to a certain perfect differential $\dot{\mathcal{B}}_{\mu\nu}$ which is antisymmetric in μ and ν. One can simplify the expression by subtracting out the rate of charge of the orbital angular momentum, i.e., by introducing

$$\dot{B}_{\mu\nu} = \dot{\mathcal{E}}_{\mu\nu} - \frac{d}{d\tau}\,(z_\mu A_\nu - z_\nu A_\mu).$$

$B_{\mu\nu}$ can be interpreted as the intrinsic angular momentum of the singularity. Then we have

$$\begin{aligned}\dot{B}_{\mu\nu} = (-1)^{n+1} \Big[& n\big(S_{\mu\beta\ldots,}{}^{\ldots\sigma\ldots}\,\partial^{\beta\ldots}\,U^{(1)}_{\ldots[\sigma\nu]\ldots} - S_{\nu\beta\ldots,}{}^{\ldots\sigma\ldots}\,\partial^{\beta\ldots}\,U^{(1)}_{\ldots[\sigma\mu]\ldots}\big) \\ & - n\big(S_{\mu\beta\ldots,}{}^{\ldots\sigma\ldots}\,\partial^{\beta\ldots}\,{}_{\sigma}U_{\ldots\nu} - S_{\nu\beta\ldots,}{}^{\ldots\sigma\ldots}\,\partial^{\beta\ldots}\,{}_{\sigma}U_{\ldots\mu}\big) \\ & - \big(S_{\alpha\beta\ldots,}{}^{\ldots\ldots}{}_{\mu}\,\partial^{\alpha\beta\ldots}\,U_{\ldots\nu\ldots} - S_{\alpha\beta\ldots,}{}^{\ldots\ldots}{}_{\nu}\,\partial^{\alpha\beta\ldots}\,U_{\ldots\mu\ldots}\big)\Big] \\ & - (v_\mu A_\nu - v_\nu A_\mu)\,, \end{aligned} \tag{65}$$

where again the terms involving U should be omitted except for f = 1, and scalar products should be substituted in the charge-symmetric theory.

In the charge-symmetric theory we also get another equation from the requirement of conservation of electric charge.

Lecture 3.

VII. THE CONDITIONS OF THE SINGULARITIES AND THE FORM OF THE MULTIPOLE MOMENTS

Last time we found the general form of the equations of motion for spin 1 or greater. I didn't go through the same calculation for spin 0. It is completely analogous and I shall just write down the results later. I also did not obtain the other conditions you get from conservation of charge; I shall not talk about the charged theory or charge-symmetric theory from now on because there isn't enough time. The calculations are completely analogous and nothing startlingly different is obtained.

The next question is what these equations really mean, because in the form in which we left them we had, first of all, all kinds of multipole moments introduced without any further statements about them; and second, we had introduced the quantities A^{μ} and $B_{\mu\nu}$, one of which was supposed to have some connection with the momentum, and the other one with the intrinsic angular momentum. The question is what further can one say, or what further conditions should one impose so that one should be able to say something specific and get theories which are physically meaningful.

First let us see what we can say in general about the conditions which the equations impose on the quantities characteristic of the singularity. Now we obviously did take care of the wave equation because we got all the fields from integrals of the wave equation. But then we also had conditions on the divergence of the source density and on its trace. So let's look first at the

divergence condition (23). For spin 2 or greater it can be shown that you have to take the divergence to be 0, but not for spin 1. The solution of the wave equation for spin 1 is

$$U_\alpha = 4\pi \int \Delta(x, x')\, \rho_\alpha(x')\, d^4 x' ,$$

where Δ is any one of the Green's functions we introduced, and it was required by Eq. (22) that

$$\chi^2 \partial^\alpha U_\alpha = 4\pi \partial^\alpha \rho_\alpha .$$

This does not impose any restrictions provided $\chi \neq 0$ since

$$\partial^\alpha U_\alpha = 4\pi \int \partial^\alpha \Delta(x,x') \left[\rho_\alpha(x') + \frac{1}{\chi^2} \partial'_\alpha \partial'^\beta \rho_\beta(x') \right] d^4 x'$$

$$= -4\pi \int \partial'^\alpha \Delta [\cdots]\, d^4 x' = 4\pi \int \Delta\, \partial'^\alpha [\cdots]\, d^4 x'$$

$$= \frac{4\pi}{\chi^2} \int \Delta (\Box' + \chi^2)\, \partial'^\alpha \rho_\alpha(x')\, d^4 x'$$

$$= \frac{4\pi}{\chi^2} \int (\Box + \chi^2)\, \Delta\, \partial'^\alpha \rho_\alpha(x')\, d^4 x' = \frac{4\pi}{\chi^2} \int \delta^4 (x-x')\, \partial'^\alpha \rho_\alpha(x')\, d^4 x'$$

$$= \frac{4\pi}{\chi^2} \partial^\alpha \rho_\alpha(x).$$

If $\chi = 0$, one has to require explicitly that the divergence of ρ should vanish, however. Similarly it should vanish for fields of higher spin. This implies conditions which one can investigate by a method which is a generalization of the method of Lubanski.[15] He introduced it when he was dealing with

the linearized equations of general relativity. Lubanski did not make use of δ-functions, but what his method amounts to is the following: Consider the expression

$$\int S_{\alpha\ldots,\nu,\ldots}\, \partial'^{\alpha\ldots,\nu}\, \delta^4\,(x' - z(\tau))\, d\tau = 0. \tag{66}$$

We want to know what conclusions one can draw from this about S. We multiply Eq. (66) by $\delta(u^2)\, d^4 x'$ and integrate over all space. Then we get

$$0 = \int\int S_{\alpha\ldots\nu,\ldots}\, \partial'^{\alpha\ldots\nu}\, \delta^4\,(x' - z)\, \delta\,(u^2)\, d\tau\, d^4 x'$$

$$= (-1)^n \int\int S_{\alpha\ldots\nu,\ldots} \delta^4\,(x' - z)\, \partial'^{\alpha\ldots\nu}\, \delta\,(u^2)\, d\tau\, d^4 x'$$

$$= \partial^{\alpha\ldots\nu} \int\int S_{\alpha\ldots\nu,\ldots}\, \delta^4(x' - z)\, \delta\,(u^2)\, d\tau\, dx^4$$

$$= \partial^{\alpha\ldots\nu}\, \frac{S_{\alpha\ldots\nu,\ldots}}{K}\,. \tag{67}$$

The last step follows as in the calculations leading to Eq. (38). Here we can carry out the differentiations, using Eqs. (43-45). Now Lubanski's argument is that as one approaches the singularity different terms will become singular with different powers of K. Therefore, if you want expression (67) to vanish, the coefficient of each separate power of K has to vanish separately, which furnishes a number of conditions. I'll just go through the simplest case (in which you know the answer), namely the electromagnetic field;

there, as you know, the charge density is proportional to the four-velocity. One usually takes this for granted, but I shall prove it to show how Lubanski's method works. In this case we have spin 1, and therefore the charge-current density involves a four-vector S_ν. We split this up in a component parallel to the four-velocity and one component perpendicular to the four-velocity:

$$S_\nu = g v_\nu + B_\nu \ , \quad B_\nu v^\nu = 0 \ . \tag{68}$$

In general, if we have several indices we just split the multipole moments appropriately in components parallel and perpendicular to the four-velocity; this simplifies the calculation.

We must now evaluate $\partial^\nu (S_\nu / K)$. From Eq. (68) and the formulas for differentiation (44) and (45) we get

$$(g\dot{v}_\nu + \dot{g} v_\nu + \dot{B}_\nu) \frac{s_\nu}{K} - (g v_\nu + B_\nu)\left[v^\nu - \frac{(1-K')s^\nu}{K}\right]\frac{1}{K^2}$$

$$= \frac{\dot{g}}{K} + \frac{B_\nu s^\nu}{K^2} + B_\nu s^\nu \frac{1-K'}{K^3} \ .$$

The most singular term diverges as $\frac{1}{K^2}$. The coefficient is $B_\nu s^\nu / K$. This must vanish for arbitrary s^ν since you can approach the singularity from any direction. Therefore we must have

$$B_\nu = 0 \ ;$$

but then all you have left is

$$\dot{g} / K = 0.$$

Therefore we must have $\dot{g} = 0$, and so the electromagnetic current must be of the form $g v_\nu$, with constant g. One can generalize this procedure and get a lot of information out of this; it can be used to derive the equations of motion of general relativity.

For spin 2, where I won't go through the calculations, the divergence will equal 0 only if $S_{\mu\nu}$ is of the form

$$S_{\mu\nu} = g v_\mu v_\nu . \tag{69}$$

This will satisfy the divergence condition, but you also have the condition on the trace

$$S_\mu{}^\mu = 0.$$

But Eq. (69) has a non-vanishing trace except if $g = 0$; this implies that one can't have a simple pole in a field of spin 2. Similarly one can show that the conditions which one has to impose on particles in fields of spin 2 or higher are just too much. One can't satisfy all of them. Although in Section VI it looked as if one had solved the general form of the problem of the equations of motion by obtaining Eqs. (63) and (65), there still are further conditions to be satisfied and things don't work out right.

Now again before I go to the specific equations, I want to show something else about multipole moments in general, which amounts to a very general equivalence theorem about different couplings.[3] What I want to show

is that one can assume

$$v^{\alpha} S_{\alpha\beta\ldots\nu,\ldots} = 0 \tag{70}$$

for the first n indices. If Eq. (70) does not hold, i.e., if the 2^n-pole moment has components which are parallel to v_{α}, it is not actually a pure 2^n-pole, but it is equivalent to a 2^{n-1}-pole.

Let us assume that Eq. (70) is not satisfied for a part of the 2^n-pole moment which I write

$$v_{\alpha} T_{\beta\ldots\nu,\ldots} . \tag{71}$$

From Eq. (38) the retarded potential is

$$O^{r}_{\ldots} = \partial^{\alpha\beta\ldots\nu} \left[\left(\frac{v_{\alpha} T_{\beta\ldots\nu,\ldots}}{K} \right)_r - \chi \int_{-\infty}^{T_r} v_{\alpha} T_{\beta\ldots\nu,} \frac{J_1(\chi s)}{s} d\tau \right]. \tag{72}$$

I shall show that it is actually not the potential of a 2^n-pole but that of a 2^{n-1}-pole. When the α-differentiation is carried out we get, using Eqs. (44) and (45)

$$O^{r}_{\ldots} = \partial^{\beta\ldots} \left[\left(\frac{v_{\alpha} \dot{T}_{\beta\ldots\nu,\ldots} + \dot{v}_{\alpha} T_{\beta\ldots\nu,\ldots}}{K} s^{\alpha} - \frac{v_{\alpha} v^{\alpha} T_{\beta\ldots\nu,\ldots}}{K^2} \right.\right.$$

$$\left.\left. + \frac{v_{\alpha} T_{\beta\ldots\nu,\ldots}}{K^3} s^{\alpha} (1 - s_{\mu} \dot{v}^{\mu}) - \chi\, v_{\alpha} T_{\beta\ldots\nu,\ldots} \frac{J_1(\chi s)}{s} \frac{s^{\alpha}}{K} \right)_r \right.$$

$$\left. - \chi \int_{-\infty}^{T_r} v_{\alpha} T_{\beta\ldots\nu,\ldots} \partial^{\alpha} \frac{J_1(\chi s)}{s} d\tau \right] .$$

Now

$$v_a \partial^a \frac{J_l(\chi s)}{s} = -\frac{d}{d\tau} \frac{J_l(\chi s)}{s} .$$

Therefore we can make an integration by parts in the integral. The integrated part equals

$$\chi T_{\beta\ldots\nu,\ldots} \frac{J_l(\chi s)}{s}$$

and if you then simplify, you obtain

$$O^r_{\ldots} = \partial^{\beta\ldots\nu} \left[\left(\frac{T_{\beta\ldots\nu,\ldots}}{\kappa} \right)_r - \int_{-\infty}^{\tau_r} T_{\beta\ldots\nu,\ldots} \frac{J_l(\chi s)}{s} \, d\tau \right] . \tag{73}$$

This is of exactly the same form as Eq. (72), but with one differentiation less and one index less, or just the retarded potential of a 2^{n-1}-pole. The advanced potential works out the same way.

The first realization of any such equivalence was again by Lubanski[15] who found that if in a gravitational dipole you have a component parallel to v_a, this is equivalent to a monopole. Then a similar equivalence was found by Harish-Chandra for the scalar dipole;[7] but actually this is quite general, the proof is essentially the same in all cases. Therefore, from now on we can assume Eq. (70) outright for any multipole. This is no restriction at all.

The next question is how much more must you require about the singularities. Let me first do something where one doesn't have to require

anything. Suppose I just want to get the results for a simple pole. In the case of spin 1 fields, of course, if I assume zero rest mass I should just get the ordinary Lorentz-Dirac equation. If you don't have rest mass 0 there will be more even for spin 1 because, as I showed you, then you have no condition on the divergence, and then you have, even for a simple pole, two kinds of current, one parallel to the four-velocity and one perpendicular to it. That complicates the calculations a little, but you can then carry through without essential difficulty.

Thus, we do first spin 1 and for the moment stick to the condition that $\partial^{\mu}\rho_{\mu} = 0$. Then Eqs. (63) and (65) reduce to

$$\dot{A}_{\nu} = -e v^{\sigma} U^{(1)}_{[\sigma\nu]} . \tag{74}$$

$$\dot{B}_{\mu\nu} = -(v_{\mu} A_{\nu} - v_{\nu} A_{\mu}) . \tag{75}$$

For simplicity let us assume that we have no intrinsic angular momentum. Then Eq. (75) must be 0, which is satisfied provided that

$$A_{\mu} = A v_{\mu} . \tag{76}$$

We can substitute this in Eq. (74) and get

$$\dot{A} v_{\nu} + \dot{A} v_{\nu} = -e v^{\sigma} U^{(1)}_{[\sigma\nu]} .$$

This I can evaluate immediately. I contract with v^ν and get

$$\dot{A} = 0, \tag{77}$$

and so A has to be equal to a constant -- let me call it m. I shall come back to this little "change in notation" in a minute. Then Eq. (74) becomes

$$m \dot{v}_\nu = e v^\sigma U^{(1)}_{[\nu\sigma]} . \tag{78}$$

In the case of $\chi = 0$, these are just the Lorentz equations as I shall show you, and if you have $\chi \neq 0$, you have a little bit more. I just want to note that one of the big defects of the Dirac method is quite obvious here. The mass comes in as a constant of integration. It has no connection with any physics; you just run through certain calculations and at one point you get a constant of integration that you call the mass. So there is no physical significance in this, and certainly there is nothing in this, for example, which would stop me from taking it as negative.

The field in Eq. (78) is the modified mean field (56) which is different, depending on whether I use a retarded field or a half-retarded, half-advanced field. To evaluate U^{rad} on the world line we must use the formulas (51) and (52). If you take the retarded Green's function, then you finally get

$$m \dot{v}_\nu = \frac{2}{3} g^2 (\ddot{v}_\nu - v_\nu v_\sigma \ddot{v}^\sigma) +$$

$$+ g^2 \chi^2 v^\sigma \int_{-\infty}^{\tau} \frac{s_\nu v_\sigma - s_\sigma v_\nu}{s^2} J_2(\chi) d\tau + g v^\sigma U^{(1)ext}_{[\nu\sigma]} , \tag{79}$$

as first shown by Bhabha. If you use a symmetric Green's function, you get

$$m\dot{v}_\nu = \frac{1}{2} g^2 \chi^2 v^\sigma \int_{-\infty}^{\infty} \frac{s_\nu v_\sigma - s_\sigma v_\nu}{s^2} J_2(\chi s)\, d\tau$$

$$+ g v^\sigma U^{(1)\text{ext}}_{[\nu\sigma]} . \tag{80}$$

If you have $\chi = 0$, obviously the two integrals would be absent. Then these are just the usual electromagnetic equations. In the symmetric case you then just have interaction with the external field; in the retarded case you have radiation damping terms. As far as the integrals (for $\chi \neq 0$) are concerned, the simple physical interpretation is that these are self-action terms which come from the fact that the meson field travels with a velocity less than the velocity of light. Therefore the particle can catch up with its own field, which is impossible in electrodynamics. This produces a finite self-action even for the symmetric case, contrary to electrodynamics.

If one does not impose the condition $\partial^\mu \rho_\mu = 0$, we must have S_μ in the form (68). Then the translational equation (63) can be reduced to

$$\dot{A}_\nu = S^\sigma \partial_\nu U_\sigma . \tag{81}$$

In the same way I can write Eq. (65) as

$$\dot{B}_{\mu\nu} = S_\mu U_\nu - S_\nu U_\mu + v_\nu A_\mu - v_\mu A_\nu . \tag{82}$$

Here we can no longer get away with assuming that $B_{\mu\nu} = 0$. I won't prove this

but it can't be done. To obtain an expression for A_ν we split it into components parallel and perpendicular to the four-velocity:

$$A_\nu = A\, v_\nu + \bar{A}_\nu , \quad \bar{A}_\nu\, v^\nu = 0. \tag{83}$$

From this we have

$$\dot{A}_\nu = \dot{A}\, v_\nu + A\, \dot{v}_\nu + \dot{\bar{A}}_\nu . \tag{84}$$

Substituting Eqs. (68) and (84) into (81) we get, after contraction with v^ν

$$\dot{A} + \dot{\bar{A}}_\nu\, v^\nu = g\, v^\sigma\, \partial_\nu\, U_\sigma\, v^\nu + B^\sigma\, \partial_\nu\, U_\sigma\, v^\nu = g\, v^\sigma\, \dot{U}_\sigma + B^\sigma\, \dot{U}_\sigma$$

$$= \frac{d}{d\tau}\,(g\, v^\sigma\, U_\sigma + B^\sigma\, U_\sigma) - g\,\dot{v}^\sigma\, U_\sigma - \dot{g}\, v^\sigma\, U_\sigma - \dot{B}^\sigma\, U_\sigma . \tag{85}$$

Now one can get some more information out of the rotational equation (82) which I have not used so far. It clearly involves only $\bar{A}$. Thus I can calculate $\bar{A}_\nu$ by contracting with v^μ. We have

$$-\bar{A}_\nu = (g\, v_\nu + B_\nu)\, U_\mu\, v^\mu - g\, U_\nu + \dot{B}_{\mu\nu}\, v^\mu \tag{86}$$

and therefore

$$\dot{\bar{A}}_\nu\, v^\nu = -\bar{A}_\nu\, \dot{v}^\nu = B_\nu\, \dot{v}^\nu\, U_\sigma\, v^\sigma - g\, U_\nu\, \dot{v}^\nu + \dot{B}_{\mu\nu}\, \dot{v}^\nu\, v^\mu . \tag{87}$$

I can substitute Eq. (87) back in (85) and get

$$\dot{A} = \frac{d}{d\tau}(g\, v^{\sigma} U_{\sigma} + B^{\sigma} U_{\sigma}) - \dot{g} U_{\nu} v^{\nu} - (\dot{B}^{\nu} + B^{\sigma} \dot{v}_{\sigma} v^{\nu}) U_{\nu} + \dot{B}_{\mu\nu} \dot{v}^{\nu} v^{\mu} \tag{88}$$

This is as far as I can go without any further assumptions; I have used up all the information available from the equations of motion, and I just didn't get through; I got an expression for $\dot{A}$ which is not yet integrable. One can get more information by imposing conditions which appear physically sensible. First of all, we assume that

$$B_{\mu\nu} v^{\nu} = 0. \tag{89}$$

This must be required if you want to interpret $B_{\mu\nu}$ as an angular momentum, because what it means is that in the rest system

$$v_0 = 1, \quad v_1 = v_2 = v_3 = 0\,, \tag{90}$$

$B_{\mu\nu}$ should only have spatial components. Incidentally, in general relativity[15] Eq. (89) follows from the positive mass density without further assumptions.

From Eq. (89) the last term of (88) vanishes. The next requirement is not as obvious, but again is one which one would demand in a sensible theory, namely, one would expect that the magnitude of the intrinsic angular momentum should be constant, or

$$B_{\mu\nu} \dot{B}^{\mu\nu} = 0. \tag{91}$$

If this is imposed, Eq. (88) and similar equations in other cases become integrable. If you contract Eq. (82) with $B^{\mu\nu}$ and use (68), you get

$$B^{\mu\nu} \overset{\cdot}{B}_{\mu\nu} = 0 = 2B^{\mu\nu} B_{\mu} U_{\nu} . \tag{92}$$

But U_{ν} is essentially arbitrary, since it contains an external field. Therefore we must have

$$B^{\mu\nu} B_{\mu} = 0. \tag{93}$$

Now conditions (68), (89) and (93) together are enough to determine the form of B_{ν}. The simplest way to work it out is to consider a "standard rest system". In this system, in addition to Eq. (90), we have also

$$B_{12} = - B_{21} \neq 0, \quad \text{all other } B_{\mu\nu} = 0. \tag{94}$$

By considering our three conditions in the standard rest system, it can easily be shown that they require

$$B^{\nu} = f \epsilon^{\nu\kappa\lambda\rho} B_{\kappa\lambda} v_{\rho}, \tag{95}$$

where $\epsilon^{\nu\kappa\lambda\rho}$ is the Levi-Civita tensor density.

The only thing we don't know at this stage is whether g and f are constants or not. Now we contract Eq. (82) with $\dot{B}^{\mu} B^{\nu}$ and get

$$\dot{B}^{\mu} B^{\nu} \overset{\cdot}{B}_{\mu\nu} = - B_{\nu} B^{\nu} (U_{\mu} \dot{B}^{\mu} - v_{\mu} \dot{B}^{\mu} U_{\sigma} v^{\sigma}) + \dot{B}^{\mu} v_{\mu} \overset{\cdot}{B}_{\rho\nu} B^{\nu} v^{\rho}$$

$$- B^{\nu} v_{\nu} \overset{\cdot}{B}^{\mu} \overset{\cdot}{B}_{\rho\mu} v^{\rho} . \tag{96}$$

The left-hand side is zero by (93), since an integration by parts yields an expression contracted over two indices in which one factor is symmetric and the other antisymmetric. Similarly the last two terms vanish. Since $B_\nu B^\nu \neq 0$, we must have

$$U_\mu \dot{B}^\mu - v_\mu \dot{B}^\mu U_\sigma v^\sigma = 0. \tag{97}$$

If you substitute this in Eq. (88) the only term left which is not a perfect differential is $-\dot{g} U_\nu v^\nu$ and therefore we must take $\dot{g} = 0$. Then we can integrate and get

$$A = m + g v^\sigma U_\sigma + B^\sigma U_\sigma . \tag{98}$$

Hamermesh: The A gives you the coefficient of v^ν, so essentially the momentum?

Havas: Yes, this plus $\bar{A}^\nu$ is the momentum.

Hamermesh: This is the momentum of the field plus $\cdots$?

Havas: No, this is just the momentum of the singularity. The U's are all evaluated at the position of the singularity.

It can be shown that to have

$$B_\nu B^\nu = \text{const.} \tag{99}$$

it is only necessary to require $\dot{f} = 0$; everything else works out already from the equations of motion.

Finally we obtain for the translational equation of motion

$$\frac{d}{d\tau}\left[(m + B^{\sigma} U_{\sigma}) v_{\nu} - B_{\nu} U_{\sigma} v^{\sigma} + B_{\mu\nu} \dot{v}^{\mu} \right] = g v^{\sigma} U^{(1)}_{[\nu\sigma]} + B^{\sigma} \partial_{\nu} U_{\sigma} . \tag{100}$$

I won't evaluate the radiation damping terms for this equation because of the length of the calculation. Before discussing higher poles of spin one fields, for which the situation is very much more confused, I will talk about the scalar field because there one can work out the entire problem completely.[16] I'll just sketch it because the proofs always run the same way. One first assumes Eq. (70) which is a very strong restriction on the form of the multipole moments. Substituting this into the rotational equation and requiring (91), one obtains an equation such as (92) which implies some condition on the multipole moment. Then one splits up A^{ν} parallel and perpendicular to the four-velocity and arrives at an expression for $\dot{A}$ by making use of the rotational equations to get rid of $\bar{A}^{\nu}$. This should be a perfect differential, and one may have enough information from the rotational equations and the conditions on the multipole moment to show this. I shall just indicate it for one particular case. If you do the scalar field with the methods of Sec. VI you end up with the equations

$$\dot{A}_{\mu} = (-1)^{n+1} S^{\alpha\beta\ldots\nu} \partial_{\alpha\beta\ldots\nu\mu} U \tag{101}$$

and

$$\dot{B}_{\mu\rho} = n(-1)^{n+1} (S_{\mu}^{\beta\ldots\nu} \partial_{\rho\beta\ldots\nu} U - S_{\rho}^{\beta\ldots\nu} \partial_{\mu\beta\ldots\nu} U)$$

$$- (v_{\mu} A_{\rho} - v_{\rho} A_{\mu} . \tag{102}$$

The reason the scalar case causes very much less trouble than any others is that there are no conditions whatever on the S's from the field equations. The Lorentz condition and the condition of the traces all come in just for spin 1 or more. S must only be symmetric and satisfy Eq. (70). But for a scalar field Eq. (70) implies that S is a symmetric tensor perpendicular to v in all its indices. From this one can obtain all possible forms of the multipole moments which are compatible with Eqs. (89) and (91). One such form is

$$S^{\alpha\beta\ldots\nu} = f_n \overline{\prod_{\sigma=\alpha\ldots\nu}} B^{\sigma} \tag{103}$$

for any n, as I shall show now.

We shall also impose

$$S_{\alpha\beta\ldots\nu} S^{\alpha\beta\ldots\nu} = 0. \tag{104}$$

It can be shown that this only requires that $\dot{f}_n = 0$, and that this together with Eq. (91) implies (99).

Now we split up A^{ν} again by use of Eq. (83). Proceeding as before we get

$$\overline{A}_{\mu} = -n(-1)^{n+1} S_{\mu}^{\ \beta\ldots\nu} \partial_{\beta\ldots\nu} \dot{U} - B_{\mu\rho} \dot{v}^{\rho} \tag{105}$$

and

$$\dot{A} = (-1)^{n+1} \left[\frac{d}{d\tau} (S^{\alpha\beta\ldots\nu} \partial_{\alpha\beta\ldots\nu} U) - \dot{S}^{\alpha\beta\ldots\nu} \partial_{\alpha\beta\ldots\nu} U\right]$$

$$+ n(-1)^{n+1} v_{\alpha} \dot{S}^{\alpha\beta\ldots\nu} \partial_{\beta\ldots\nu} U . \tag{106}$$

We must show now that the last two terms vanish. Now from Eq. (103)

$$-\dot{S}^{\alpha\beta\ldots\nu}\,\partial_{\alpha\beta\ldots\nu}\,U + n\,v_{\alpha}\,\dot{S}^{\alpha\beta\ldots\nu}\,\partial_{\beta\ldots\nu}\,\dot{U} =$$

$$-n\,\dot{B}^{\alpha}\overline{\underset{\sigma=\beta\ldots\nu}{//}}\,B^{\sigma}\,\partial_{\alpha\beta\ldots\nu}\,U + n\,v_{\alpha}\,\dot{B}^{\alpha}\overline{\underset{\sigma=\beta\ldots\nu}{//}}\,B^{\sigma}\,\partial_{\beta\ldots\nu}\,U\,. \qquad (107)$$

But from Eqs. (102), (99) and (105) we have

$$0 = \dot{B}^{\mu}\,B^{\rho}\,\dot{B}_{\mu\rho} = n(-1)^{n+1}\,[\,-\dot{B}^{\mu}\,B^{\rho}\,S_{\rho}^{\ \beta\ldots\nu}\,\partial_{\mu\,\beta\ldots\nu}\,U$$

$$+\dot{B}^{\mu}\,v_{\mu}\,B^{\rho}\,B_{\rho}^{\ \beta\ldots\nu}\,\partial_{\beta\ldots\nu}\,\dot{U}\,]\,, \qquad (108)$$

which implies the vanishing of (107). Thus we get from (106)

$$A = m + (-1)^{n+1}\,S^{\alpha\beta\ldots\nu}\,\partial_{\alpha\beta\ldots\nu}\,U \qquad (109)$$

and from this and (106)

$$A_{\mu} = [\,m + (-1)^{n+1}\,S^{\alpha\beta\ldots\nu}\,\partial_{\alpha\beta\ldots\nu}\,U\,]\,v_{\mu} - B_{\mu\rho}\,\dot{v}^{\rho}$$

$$-n\,(-1)^{n+1}\,S_{\mu}^{\ \beta\ldots\nu}\,\partial_{\beta\ldots\nu}\,\dot{U}\,. \qquad (110)$$

Hamermesh: What was the assumption you made on $B_{\mu\nu}$?

Havas: Equation (91) means that the magnitude of the intrinsic angular momentum is constant. Equation (89) means really that it is a decent angular momentum.

Hamermesh: And Eq. (70)?

Havas: This is not an assumption. You also want the multipole moment to have constant magnitude and you want the intrinsic angular momentum to have constant magnitude. Physically this is sensible and also this seems to be the only way to get a well-defined problem.

Hamermesh: Why shouldn't the intrinsic angular momentum change and pass things over to the field?

Havas: It can change in orientation — that is how we got the rotational equations. You can have exchange of angular momentum between the intrinsic angular momentum and the field momentum.

Hamermesh: But it doesn't change its magnitude?

Ekstein: This is perhaps the definition of an elementary particle.

Havas: Yes. I don't say you can't have it; that's what you usually want to have if you want to talk of a well-defined particle. There is no mathematical necessity to introduce it. The trouble is that at this stage it is not a well-defined problem if you don't accept certain conditions because then you are left with a general form of equations, Eqs. (63) and (65), and there is no further way in which you can connect the quantities. In this sense, there is a theory possible for spin 2, for example. What it says is that there is a momentum of the particle and an intrinsic angular momentum and multipole moments which are essentially unrelated except through these equations. You just can't say any more.

Peshkin: Can one make other requirements to relate them?

Havas: There might be others, but these are the natural ones to make. I don't see how one can get along without Eq. (89) because if you drop this, it amounts to not having any equations of motion when you have no fields. The particles can do anything; you have no restriction on the motion. So you don't seem to be able to get away from (89). You might conceivably drop (91), and as I said, it then is not a well-defined problem any more.

It can be shown that the most general form of the multipole moments you can have for a spin 0 field is an arbitrary linear combination of moments of the form

$$S_p^{\alpha\ldots\nu} = g_{np} \sum B^{\alpha}_{\lambda} B^{\lambda\beta} B^{\gamma}_{\rho} B^{\rho\delta} \ldots B^{\pi-1}_{\eta} B^{\eta\pi} \prod_{\sigma=\pi+1,\ldots\nu} B^{\sigma} . \tag{111}$$

The sum goes over all permutations of $\alpha\ldots\nu$, so that this expression becomes symmetric as it has to be, and $p \leq n$, with p even. Therefore for scalar fields the answer can be given completely.

Now return to the vector fields and pseudovector fields for spin 1. Here the answer has not yet been given completely. It turns out that you certainly can have multipole moments of all orders as long as you don't require $\partial^{\mu} \rho_{\mu} = 0$. The question is whether you can have multipole moments of all orders if you do require this condition, which is necessary in the case of the electromagnetic field. At this stage I don't know the answer.*

* Note added in proof: I have since proved that this is possible. A detailed account is in preparation.

For spin 2 or more one can not satisfy all the conditions for the charge-symmetric field; the answers are essentially the same. The additional requirement of conservation of electric charge in the charge-symmetric field doesn't add anything to the difficulties. On the contrary, it seems to allow more possibilities, at least for the linear fields which we have considered, where we did not explicitly introduce an interaction with the electromagnetic field.

Hamermesh: What was the basic assumption?

Havas: It was conservation of energy-momentum and of angular momentum.

Hamermesh: Total?

Havas: Yes, and that leads to a translational equation for A_ν and a rotational equation for $B_{\mu\nu}$. Then condition (89) is more or less imposed on you if $B_{\mu\nu}$ is to be interpreted as intrinsic angular momentum. Equation (91) is not necessary, but it looks like a natural requirement.

Hamermesh: This has no analogue in quantum theory?

Havas: In quantum theory you imply it when you write down an equation for a particle with a definite spin; you then have the equivalent of (91).

Equation (70) is really no restriction. It follows if you want pure multipole moments without lower order multipoles mixed in.

Hamermesh: Do you really have that, though? You have a particle and a field — isn't that the picture you have?

Havas: Yes.

Hamermesh: Then the thing that bothers me is that you have many particles possible. I start with one, but it can suck onto itself things from the field.

Havas: Now you are thinking of excited states?

Hamermesh: Yes — isobars.

Havas: That is not excluded — you can have them here, too.

Hamermesh: But that would violate that condition?

Havas: When you have isobars you would not just consider the magnitude of the intrinsic angular momentum, but you would take that plus the field and integrate over the field.

Hamermesh: Oh, I see.

Havas: That has been worked out.[17]

Tanaka: Can we have isobars with more than one mass?

Havas: I can't say anything about mass directly, as the calculations for excited states assume the particles to be at rest. One would have to calculate the mass or energy in the field for this case.

Peshkin: In the classical theory of fields the singularities play the role of the particles. When I quantize it, it would appear that I am faced with particles of two kinds — there are the singularities and then there are the ordinary particles that I get by quantizing the field. I understand why, for instance, the second kind of particles have spin 2

in a tensor field, but I don't understand how the spins of the singularities are determined by the theory. I understand why, with particles of the second kind I can't have high multipole moments, but I don't understand about the particles of the first kind.

Hamermesh: No, but what you should do in the quantum theory is to take two types of fields. The first field is the one of which you just take the singularities. These interact with the second field.

Peshkin: How is it possible to have arbitrary multipole moments here while in quantum theory the moments are limited?

Havas: Well, I can answer it in part — in quantum mechanics you certainly can not do it if you want to describe the moments for a Dirac particle in the usual way of introducing interactions. But as you know, you can also introduce extra terms of the Pauli type except that I have been told that Schwinger has proved that this can not be done consistently. This excepted, you can, of course, add additional properties but that is what you usually want to avoid. Otherwise you are limited by the algebra of the matrices you have.

Ekstein: In Peshkin's question, what symmetry statement is directly involved?

Peshkin: It is basically that for a spin 1 particle, for instance, you have only three quantum states, while to describe, say, a quadrupole moment, you need five.

Havas: I can only give a mathematical answer which, of course, is not what you want. Here the multipole moments are determined in a way from the intrinsic angular momentum. In anything I wrote down, the $B_{\mu\nu}$'s entered explicitly. You have here also a connection between the multipole moment and the angular momentum. If you had no angular momentum you could not have any moments.

Peshkin: In a scalar field I can still have multipole moments. Would you say that that is in some sense because I have sucked some orbital angular momentum from the field into the particle?

Ekstein: Are we talking about the sources of fields? These can have high multipole moments and can have angular momentum whose numerical value is not restricted.

Havas: Here the particle is described by $B_{\mu\nu}$ and I haven't said anything about its magnitude. In quantum theory you have theories of particles of spin 1/2, 3/2, etc., which are distinct theories. Here everything is lumped together. The mechanical properties of the particle are specified by its mass and the $B_{\mu\nu}$. So this $B_{\mu\nu}$ covers the theories of all spins.

Hamermesh: I don't think it would be any different if you assigned a value to the spins of the singularities, then these terms will start to contract when you write out these multipoles.

Havas: What you mean is that if you write out products of matrices you can reduce that by the algebra of the matrices.

Hamermesh: Even if I don't write it out explicitly, I know that it satisfies an equation of finite degree. Clearly I can't write down terms greater than that degree.

Ekstein: The elementary particle is not something that has a simple symmetry. But the point is that it maintains whatever asymmetry it has no matter how it is buffeted around by the field.

Havas: Yes.

Lecture 4.

Last time we used the Dirac method, which is essentially a requirement of conservation of energy-momentum and of angular momentum to derive two equations; one is a translational equation of motion which tells you that a certain expression in terms of the fields should be equal to the derivative of a four-vector A_ρ and a rotational equation which gives you a $B_{\mu\nu}$ in terms of the field, the interpretation being that $B_{\mu\nu}$ is the intrinsic angular momentum of the particle.

This is as far as one can get without imposing additional conditions. It seems necessary for a physical interpretation of $B_{\mu\nu}$ as an angular momentum to impose Eq. (89), $B_{\mu\nu} v^\nu = 0$. On the other hand, for the multipole moment the equivalent Eq. (70) $v^a S_{a\ldots} = 0$, does not have to be imposed — it simply specifies what is meant by a pure moment of a certain order. Without this condition the different orders would be mixed up. If in addition one requires (91) and (104), i.e., that the magnitude of the angular momentum and of the multipole moment should be constant, there are enough conditions to find explicitly the form of all the multipole moments which are described by these equations. For a scalar or a vector field, one can have multipole moments of all orders. For fields of spin 2 or greater one can't satisfy these conditions and so no poles of any order are possible.

Ekstein: What quantities in this theory would you dignify with the adjective "observable"?

Havas: There is a number of constants in here, the mass, the magnitude of the angular momentum, and the coupling constants involving

all the multipole moments; certainly at least these.

Ekstein: Coupling to what — either the fields or their derivatives you consider as observables?

Havas: I do, but actually that question doesn't enter here because I have here equations of motion of particles. I could say, just look at the particles and observe their motion and what can I find then?

Ekstein: Just z. In order to confront this theory with experiment you must have a definite problem — fields and the z as an observable. That certainly is an observable.

Havas: Yes, but you can make other deductions; e.g., when you put an electric charge in the field you can determine the electric charge.

Ekstein: Yes, because the electric field is observable, but if you don't consider the field as observable I don't know what this means.

Havas: No, I could, without talking about a field, take two electric charges and observe their motion and from this deduce that there is some interaction between them which is characterized by a constant e.

Ekstein: Eliminating the field?

Havas: Yes, I could in principle avoid talking about the field; this is really what I am going to discuss today.

Hamermesh: Is the statement that if you have a field of spin greater than 1, then something goes haywire?

Havas: Yes, if you want to have interactions. Nothing goes wrong with the free fields, but ...

Peshkin: If you want to have interactions with particles having definite spin?

Havas: You mean you want me to put a condition on $B_{\mu\nu}$ — I mean just to fix the magnitude?

Peshkin: No, I wanted to know exactly what would go haywire. If you don't fix the magnitude of $B_{\mu\nu}$ then things don't go haywire?

Havas: If I let condition (91) go then I have to stop at (63) and (65); I would simply say that A_ν is the momentum and I don't know what that stands for except that that's the thing which varies with time according to Eq. (63). I have no way of expressing it in terms of mass or velocity or anything — it's just another undetermined property. This is certainly not the usual way in which one talks about particles, that one is unable to define the momentum in any way except explicitly through the way it varies with time in a given field. If you want to take $B_{\mu\nu}$ as an intrinsic property and get A_ν out explicitly in terms of other things, then if you don't assume Eq. (91) you can't go on. Of course, you might stop here and say this is it, but this is not the usual way one considers equations of motion.

Hamermesh: Is there any way of imitating the results for a field of spin greater than 1 in the quantum theory?

Havas: I hope so, but I have no results yet. Of course, one of the points I was after was to see if one can't classically get some general statements, because obviously it is very difficult to get general statements out of the quantum theory of interacting fields, while here it is possible.

Peshkin: From the physical point of view, isn't it so that all these properties of the particles are simply reflections of those conditions which you put on the field? Actually nothing else has been done except these few assumptions that you wrote down.

Havas: Yes, I have really not assumed anything about the particle except that it is a singularity. The only gap is that the mass comes in as a constant of integration. This would be gone too if you do it from the point of view of general relativity, where everything would follow from the assumption of a mass singularity.

Hamermesh: If you take a field of spin 2 then this says that you can't get interactions?

Havas: Yes.

Hamermesh: I know a field of spin 2.

Havas: Namely?

Hamermesh: The gravitational field.

Havas: Pauli and Fierz unfortunately made the statement that their theory of spin 2 has something to do with gravitation. It has absolutely nothing to do with gravitation. The reason is quite obvious, actually,

because the conditions for the field must be $\Box U_{\alpha\beta} = 4\pi \rho_{\alpha\beta}$, $\partial^{\alpha} U_{\alpha\beta} = 0$, and $U^{\alpha}_{\alpha} = 0$. But in the gravitational case you could only have trace zero if the field had no sources. If you put sources in it is impossible — you just can't do it; and so whatever the linearized theory of gravitation means, it is no longer equivalent to the Fierz spin-2 theory.

Hamermesh: It must be a mixture?

Havas: Yes.

Hamermesh: The trace condition eliminates the mixture?

Havas: Yes. That is one of the conditions you need to eliminate the mixing. This is absolutely impossible to maintain once there is a source in it, essentially because then the trace is essentially the mass; so you can't satisfy this condition. For pure fields it's all right, the equations are formally equivalent, but the question here is precisely what happens with interactions.

VIII. RELATIVISTIC DYNAMICS IN TERMS OF DIRECT INTERPARTICLE ACTION

So far we always considered the external field as something arbitrary. On the other hand everybody agrees that the only fields which are physically acceptable are those which can be related to sources, even if it usually is not convenient or necessary to do this. I want to remind you very briefly that electrodynamics started out from Coulomb's law; attempts were made to modify it for moving charges (e.g., Weber's law), but soon it was realized that the correct

formulation of such a law was not easy. It is rather remarkable that Gauss understood the problem in 1845. He wrote to Weber that what is needed is "a derivation of additional forces — to be added to the interaction of electrical charges at rest, when they are both in motion — from an action which is propagated not instantaneously but in time, as is the case with light".[18]

Maxwell solved the problem indirectly through a study of the field equations instead of the force laws. Afterwards physicists got used to talking in terms of fields exclusively, in particular because emission of radiation from an accelerated charge was a phenomenon which could be described very simply in terms of an emitted field, but seemed to be incapable of description in terms of particles alone. So one stuck to the field concept in spite of all the difficulties resulting from the self-energy of point charges. All we did in these lectures so far was to try to get around these difficulties with self-energy.

Nevertheless, even if one considers the field to be a basic quantity and would not want to accept action at a distance, one can still expect that all the fields are due to sources, and one can still attempt to formulate the theory in terms of the sources alone; but then, as I shall indicate very briefly, it is impossible to distinguish the two points of view in classical electrodynamics.[19]

You are probably all familiar with Fokker's variational principle.[20] All of electrodynamics can be derived from a variational principle

$$\delta J = 0 ,$$

where

$$J = \sum m_i \int v_i \quad d\tau_i + \sum_{i<k} e_i e_k \iint v_{i\nu} v_k^{\nu} \delta (s_{ik}^2) \, d\tau_i \, d\tau_k , \tag{112}$$

with

$$v_i = (v_{i\rho} v_i^{\rho})^{1/2} , \quad s_{ik}^2 = (z_{i\mu} - z_{k\mu}) (z_i^{\mu} - z_k^{\mu}) .$$

First define "field quantities" in terms of the particle variables. We define the potential

$$U_k^{\nu} (x) = e_k \int \delta (s_k^2) \, v_k^{\nu} \, d\tau_k , \quad s_k^2 = (x_{\mu} - z_{k\mu}) (x^{\mu} - z_k^{\mu}), \tag{113}$$

and then the field as usual is

$$F_{\mu\nu} = \partial_{\mu} U_{\nu} - \partial_{\nu} U_{\mu} . \tag{114}$$

Now we can get the equations of motion from (112) by variation with respect to the particle coordinates. Considering, say, the ath particle, we get

$$m_a \dot{v}_{a\nu} = e_a v_a^{\sigma} \sum_{k \neq a} F_{k\nu\sigma} (z_a) . \tag{115}$$

The quantity (114) is the symmetric field, because Eq. (113) contains the half-retarded, half-advanced Green's function, as all the integrals go from $-\infty$ to $+\infty$. I don't have the time to discuss the physical problem of advanced vs retarded potentials at this stage. But to have a theory in terms of particle variables which is derivable from a variational principle, one must use the

symmetric Green's function.

Peshkin: You may answer this question by yes or no. You have a choice of using either the symmetric or the anti-symmetric one. Must you use half-advanced plus half-retarded?

Havas: You must use plus.

Equation (112) also contains Maxwell's equations, as I'll show again very briefly. The $\delta(s^2)$ satisfies

$$\Box\, \delta(s^2) = 4\pi \delta^4(s). \tag{116}$$

We define

$$j_{k\nu} = e_k \int v_{k\nu}\, \delta^4(s)\, d\tau_k . \tag{117}$$

We then multiply Eq. (116) on both sides by $e_k\, v_{k\nu}\, d\tau_k$ and integrate. We obtain

$$\Box\, U_{k\nu} = 4\pi\, j_{k\nu} . \tag{118}$$

Now from Eq. (113) it follows that

$$\partial^\sigma\, U_{k\sigma} = 0. \tag{119}$$

Then, taking ∂_ν of Eq. (119) and subtracting this from (118) we get, using (114)

$$\partial^\sigma\, F_{k\sigma\nu} = 4\pi\, j_{k\nu} , \tag{120}$$

i.e., Maxwell's equations.[21]

Peshkin: Do you automatically also get that the particle doesn't act on itself?

Havas: Yes, because in (112) there is a summation which does not go over i = k, so you have no self-action term included and this leads to k ≠ a also in Eq. (115).

We have started from (112) because physicists like to have a theory which starts from a variational principle. But electrodynamics has been considered from the point of view of action at a distance before without going to this formulation — in particular by Frenkel [22] and Synge [23], who simply postulated that you should take equations of motion with retarded interactions, but with no radiation damping, and show that this still allows a consistent theory. I don't want to discuss this any further except to note that almost everything which we are going to discuss here one could also do with those equations. The one thing one can't do properly in the Frenkel-Synge formalism is to describe radiation.

It looks as if we had lost something as compared to field theory, namely detailed conservation of energy momentum. Detailed conservation requires that

$$\frac{d A_{a\nu}}{d\tau_a} + \frac{d}{d\tau_a} \int \partial^\mu T_{\mu\nu} \, d^4 x = 0 \qquad (121)$$

in the neighborhood of particle a, and that the integral in Eq. (121) should vanish if the region of integration does not include a particle. If we take for $T_{\mu\nu}$ the expression analogous to the one introduced by Frenkel,[22]

$$T_{\mu\nu} = \sum_{i \neq k} T_{\mu\nu} \left(F_i^{sym}, F_k^{sym} \right) , \qquad (122)$$

then this, plus the equations of motion, guarantees Eq. (121). Therefore we have detailed conservation of energy and momentum by simply defining a suitable quantity. Of course, from the present point of view this has no meaning physically, but mathematically it is all right.

Incidentally, after Dirac did his original work discussed in Sec. VI, it was realized by Pryce [24] that one could modify the usual energy-momentum tensor of electrodynamics so that the infinities would not appear. Then Harish-Chandra showed that this can be done in general. [25] I really don't understand the idea behind this. According to field theory the physical state at a point is determined by the field without regard to its sources. But the modified energy-momentum tensors of Pryce and Harish-Chandra can only be formed with a knowledge of the sources; so this is no longer field theory.

Equation (121) holds with the particular energy-momentum tensor (122) and similarly with one due to Wheeler and Feynman which they call the canonical tensor. It differs from the tensor (122) by the additional term

$$\sum_{i\neq k} T_{\mu\nu}\left(F_i^{rad}, F_k^{rad}\right),$$

formed from the radiation fields, which obviously has a vanishing divergence. Then we get

$$T_{\mu\nu} = \sum_{i\neq k} T_{\mu\nu}\left(F_i^{ret}, F_k^{adv}\right). \tag{123}$$

If one attaches any physical significance to the densities of energy and momentum, then this canonical tensor of Wheeler and Feynman has some advantages for certain purposes. Conservation of angular momentum follows from either (122) or (123) as usual.

Now we turn to the problem of radiation. The equations of motion (115) involve time-symmetric interactions, and the question arises right away if this can ever describe radiation; the usual idea is that one needs retarded fields precisely to obtain radiation loss. This question was discussed, for example, by Einstein and Ritz[26] fifty years ago; Ritz thought that the use of retarded fields was something essential for the second law of thermodynamics, while Einstein felt that the basic laws of physics were time-symmetric and that radiation was a statistical phenomenon.

The first attempt to give such a statistical description consistently is due to Wheeler and Feynman.[27] Their essential assumption in addition to Eqs. (115) is that sufficiently many particles are present to absorb completely the radiation given off by the source. They go through three different, rather special and complicated non-relativistic derivations which I won't discuss, and then they give a general, relativistic, derivation. Before discussing this derivation I wish to stress that this statistical description has no bearing on the problem of action-at-a-distance vs field theory.[19] The argument of Wheeler and Feynman is based on the use of the equations of motion (115) (with time-symmetric fields). Now, of course, Eq. (115) can be obtained in two different

ways, either (as we just did) from Fokker's variational principle, without talking about fields at all, or by the Dirac method (as we did in Sec. VI). Thus one can get the time-symmetric equations of motion in a consistent manner from both points of view; any subsequent derivations are not relevant for the equations of fields vs action-at-a-distance.

Wheeler and Feynman in their general derivation start from the statement that complete absorption implies that a test charge placed anywhere outside the absorbing medium will experience no disturbance, i.e.,

$$\sum_{\text{all } k} F_k^{\text{sym}} = 0 \qquad \text{(outside the absorber).} \tag{124}$$

Now the symmetric field is half the sum of the retarded and the advanced field. But retarded fields correspond to outgoing waves and advanced fields to incoming waves. Then they conclude that since an outgoing wave can never cancel an incoming wave, the retarded and advanced fields have to vanish separately outside the absorber, and therefore also

$$\sum_{\text{all } k} (F_k^{\text{sym}} - F_k^{\text{adv}}) = 0 . \tag{125}$$

But this is a solution of the homogeneous equation and thus if it vanishes in a region outside the absorber, it has to vanish everywhere else. So (125) must actually hold everywhere.

Then they take the summetric field which enters into the equations of

motion and rewrite it as

$$\sum_{k\neq a} \frac{1}{2}\,(F_k^{\,ret} + F_k^{\,adv}) = \sum_{k\neq a} F_k^{\,ret} + \frac{1}{2}(F_a^{\,ret} - F_a^{\,adv})$$

$$- \frac{1}{2} \sum_{\text{all } k} (F_k^{\,ret} - F_k^{\,adv}). \qquad (126)$$

The first term is what you expect if the fields of all particles are retarded. The second term is the field which determines the radiation reactions in the equations of motion (where the fields have to be evaluated on the world line), and the third term, from what we just said about (125), is zero. Thus it looks like what you got from the symmetric case is what we need in the retarded case, namely,

$$m_a \dot{v}_{a\nu} = e_a v_a^{\sigma} \left[\sum_{k\neq a} F_{k\nu\sigma}^{\,ret} + F_{a\nu\sigma}^{\,rad} \right]. \qquad (127)$$

We note first that one could have seen this without any calculations. If you take the condition of complete absorption [in the form (125)] then you get the same results no matter what combination you use, because (125) implies that

$$\sum_{\text{all } k} F_k^{\,ret} = \sum_{\text{all } k} F_k^{\,adv} = \sum_{\text{all } k} \frac{1}{2}\,(F_k^{\,ret} + F_k^{\,adv}). \qquad (128)$$

These are the total fields due to all particles, and in field theory it is the total field which must be used in the energy-momentum tensor to obtain the equations

of motion. Thus one obtains the same equations in the time-symmetric case and the retarded case. Things have been fixed up so that there is no distinction between these cases initially, the total field being the same in all cases. Again it had already been suggested by Einstein,[28] that there might exist a condition such as Eq. (128).

At this stage we made the symmetric equations of motion look as if they were the same as the retarded ones; but this by itself does not assure us that we have radiation because we could have done just the opposite — we could have gone to the advanced field and we would have apparently obtained radiation gain instead of radiation loss. This is where the statistical considerations come in. Wheeler and Feynman conclude that the "irreversibility of the emission processes is a phenomenon of statistical mechanics connected with the asymmetry of the initial conditions with respect to time", or to put it another way, for statistical reasons it so happens that the sum of the retarded fields which enters in the equations of motion is essentially independent of the particle, while this is not so for the sum of the advanced fields. This appears reasonable, but on the other hand it is an argument which is based on an analogy with classical statistical mechanics which still has to be justified, because all the results of classical statistical mechanics are derived on the basis of instantaneous interaction. But here we don't have instantaneous interactions and nobody knows anything about statistical mechanics with non-instantaneous interactions.

The main difficulty is in accepting Eq. (124) because it is based on a

division of the world into something inside and outside, and on a consideration of the effects of all particles on a test particle outside, which is a concept one should not use if one wants to talk in terms of particles alone. One should not have to bring in another particle in addition to all particles and talk about what happens to it. But if one does accept Eq. (124) and the plausible statistical statements, then one can calculate radiation effects as usual, using the canonical energy-momentum tensor (123) which furnishes a flow of energy outward. One does not have to think of this in terms of a flow of energy in the "field" – one could say that "radiation" means that the effect of all other particles on a particle is given by what looks like a wave to that particle.

One should really have to solve the equations of motion (127) keeping in mind conditions (125), namely that the total retarded minus the total advanced field should be the same. This, it turns out, can not be done in any sensible way.[29] One has again to take recourse to a statistical interpretation which will not allow a rigorous statement on the total retarded and advanced fields in solving a problem.

As mentioned before, the conclusions and arguments have nothing to do with field vs action-at-a-distance. The equations obtained starting from (115) are identical; the only distinction one could make would be between a Wheeler-Feynman type argument which in the end uses statistical considerations and a field theory with retarded interactions which would say that Eq. (127) does not involve statistical considerations. But clearly it is hopeless to decide experimentally whether a classical charge in a classical field obeys (127) as interpreted statistically or taken as exact. Since such a distinction between the Wheeler-Feynman type theory and a retarded

interaction theory is not possible, there is no verifiable distinction between field theory and action-at-a-distance theory in electrodynamics.

Now here, I believe, meson theory could be helpful. One can describe meson theory in terms of particles alone just as well as electrodynamics. It can be done for both neutral fields [30] and charged fields. [31] I won't discuss charged fields; in neutral vector meson theory I shall only consider the form of the theory with conservation of mesic charge which means that the mesic current is still proportional to v_μ. If you use the mesic equation of motion (80) for the vector case and the analogous equations for the scalar case, but now explicitly introduce time-symmetric interactions, then for the vector case we have

$$m_a \dot{v}_{a\nu} = g_a v_a^{\ \sigma} \sum_{k\neq a} U^{(1)\,\mathrm{sym}}_{k[\nu\sigma]} + \frac{1}{2} g_a^2 \chi^2 v_a^{\ \sigma} \int_{-\infty}^{\infty} \frac{s_{a\nu} v_{a\sigma} - s_{a\sigma} v_{a\nu}}{s_a^2} \times$$

$$\times J_2(\chi s_a)\, d\tau \qquad (129)$$

and for the scalar case

$$m_a \dot{v}_{a\nu} = -g_a \sum_{k\neq a} \partial_\nu U_k^{\mathrm{sym}} + g_a \frac{d}{d\tau_a}\left(\sum_{k\neq a} U_k^{\mathrm{sym}} v_{a\nu} \right)$$

$$- \frac{1}{2} g_a^2 \chi^2 \int_{-\infty}^{\infty} \frac{s_{a\nu}}{s_a^2} J_2(\chi s_a)\, d\tau - \frac{1}{2} g_a^2 \chi \frac{d}{d\tau_a}\left(v_{a\nu} \int_{-\infty}^{\infty} \frac{1}{s_a} J_1(\chi s_a)\, d\tau \right). \qquad (130)$$

Notice that even in the time-symmetric case you have self-action terms, namely the integrals over the entire motion of the particle. Even from the point of view of field theory this means trouble; mathematically it is all right, but

conceptually it certainly is not. What one means by a field to begin with is something which is defined in terms of its action on a test particle. There is no difficulty connected with this in electrodynamics, but there certainly is one here because what happens to a particle of a given position and momentum depends on its entire motion. One can not, as in electrodynamics, bring a test charge to some point in space and thereby investigate the field there. The result would depend on how you brought the test charge in there.

From the point of view of action at a distance, Eqs. (129) and (130) are not acceptable because from this point of view the equations of motion should only involve actions of other particles on a particle and no self-actions. Now the question is, "Can one get a consistent theory by omitting all the self-action terms?" The answer is yes, one can do it and it is consistent in the sense in which electrodynamics was consistent, namely, I can write down a variational principle from which I can get the equations of motion, from which I can get the field equations for quantities defined in terms of the particles, and from which I can get detailed conservation of energy and momentum if I wish.

I shall first write down the variational principle. For the following it is somewhat more convenient to use instead of Green's function $\overline{\Delta}$ defined by (33) the function ${}_sG \equiv 4\pi\,\overline{\Delta}$. Then the variational principle for the vector meson case is

$$J = \sum_i m_i \int v_i \, d\tau_i + \sum_{i<k} g_i g_k \iint {}_sG(s_{ik}^2)\, v_{i\nu}\, v_k^{\nu} \, d\tau_i \, d\tau_k$$

$$= \text{extremum}, \tag{131}$$

which differs from Fokker's principle only in the use of ${}_sG$ instead of δ. For the scalar case we have

$$J = \sum_i m_i \int v_i \, d\tau_i - \sum_{i<k} g_i g_k \iint {}_sG(s_{ik}^2) \, v_i \, v_k \, d\tau_i \, d\tau_k$$

$$= \text{extremum} . \tag{132}$$

We can again define field quantities by introducing

$$U_{k\nu}(x) = g_k \int {}_sG(s_k^2) \, v_{k\nu} \, d\tau_k \tag{133}$$

and

$$U_k(x) = g_k \int G(s_k^2) \, v_k \, d\tau_k \tag{134}$$

for the vector and scalar case, respectively. Carrying out the variation with respect to the particle coordinates, we then obtain the equations of motion without self-action terms

$$m_a \dot{v}_{a\nu} = g_a v_a^{\sigma} \sum_{k \neq a} U_k^{(1)} [\nu\sigma] \tag{135}$$

and

$$m_a \dot{v}_{a\nu} = -g_a \sum_{k \neq a} \partial_\nu U_k + g_k \frac{d}{d\tau_a} \left(\sum_{k \neq a} U_k \, v_{a\nu} \right). \tag{136}$$

From the equation satisfied by ${}_sG$

$$(\Box + \chi^2) \, {}_sG(s^2) = 4\pi \, \delta^4(s) \tag{137}$$

we can get the inhomogeneous wave equation for the potentials as before. This is all we need in the scalar case; in the vector case this can be shown to imply

the Lorentz condition, and thus we have regained the field equations of meson theory which we had in Sec. III. Thus again the variational principle in each case contains both the equations of motion and the field equations.

We can obtain detailed conservation laws by defining energy-momentum tensors just as in electrodynamics, using either the Frenkel tensor (122) or the canonical one (123). The $A_{a\nu}$ of Eq. (121) must be taken as $m_a v_{a\nu}$ in the vector case and as $(m_a - g_a \sum_{k\neq a} U_k)\, v_{a\nu}$ in the scalar case.

Concerning the question of radiation, nothing new has to be added, because in the argument presented before nothing implied that one was talking about electric fields. One can proceed from Eq. (124) as before. Thus for the vector case, if one starts out from the time-symmetric field-theoretical Eq. (129), one would end up with the retarded equations of field theory (79). If one starts with the equations (135), obviously one gets something different. Whereas in the field-theoretical case (79) one has an integral from $-\infty$ to τ_a in the action-at-a-distance case these integrals are replaced by

$$\frac{1}{2}\left(\int_{-\infty}^{\tau_a} - \int_{\tau_a}^{\infty}\right),$$

i.e., we get

$$m_a \dot{v}_{a\nu} = g_a v_a^{\sigma} \sum_{k\neq a} U^{(1)\,ret}_{k[\nu\sigma]} + \frac{2}{3} g_a^2 (\ddot{v}_{a\nu} - v_{a\nu} v_{a\sigma} \dot{v}_a^{\sigma})$$

$$+ \frac{1}{2} g_a^2 \chi^2 v_{a\nu} \left\{ \int_{-\infty}^{\tau_a} - \int_{\tau_a}^{\infty} \right\} \frac{s_{a\nu} v_{a\sigma} - s_{a\sigma} v_{a\nu}}{s_a^2} J_2(\chi s_k)\, d\tau \,. \tag{138}$$

Similarly we get in the scalar case

$$m_a \dot{v}_{a\nu} = -g_a \sum_{k\neq a} \partial_\nu U_k^{ret} + g_a \frac{d}{d\tau_a} \left(\sum_{k\neq a} U_k v_{a\nu} \right)$$

$$+ \frac{1}{3} g_a^2 (\ddot{v}_{a\nu} - v_{a\nu} v_{a\sigma} \dot{v}_a^{\sigma}) + \frac{1}{2} g_a^2 \chi^2 v_{a\nu}$$

$$- \frac{1}{2} g_a^2 \chi^2 \left\{ \int_{-\infty}^{\tau_a} - \int_{\tau_a}^{\infty} \right\} \frac{s_{a\nu}}{s_a^2} J_2(\chi s_a)\, d\tau$$

$$- \frac{1}{2} g_a^2 \chi \frac{d}{d\tau_a} \left\{ v_{a\nu} \left[\int_{-\infty}^{\tau_a} - \int_{\tau_a}^{\infty} \right] \frac{1}{s_a} J_1(\chi s_a)\, d\tau \right\} . \tag{139}$$

Thus in both cases the entire difference is in the integrals, but it is a difference which in principle at least, provides a way to distinguish between the two points of view. In electrodynamics there was none. Here you get two different descriptions of radiation, depending on which time-symmetric equations of motion you start out with, which can be used to calculate all kinds of processes.[32,17] Unfortunately, whatever differences have been calculated so far are just a little too small to reach any experimental decision, but at least in principle one has a method for experimental decision available which one does not have in electrodynamics.

Similar results can be obtained with higher poles and with charge-symmetric fields; the theory can always be formulated consistently in terms of interparticle interaction.

IX. EQUATIONS OF MOTION IN GENERAL RELATIVITY AND GRAVITATIONAL RADIATION

I want to spend just a few minutes on general relativity because I made several references to it before, and because, somewhat surprisingly, it gives some support to the idea that one should think of radiation in terms of direct particle interactions although, of course, it is a field theory par excellence. When I started looking into the problem of gravitational radiation I did not expect any connection with the ideas just discussed; but the connection was implicit in what had been known before — I just had not known enough to realize it.

In general relativity it is possible to derive equations of motion from the field equations.[33, 34] I discussed the Lubanski method in Sec. VII; the essential feature was that when you start out from a certain equation you get from it information by multiplying it by a δ-function and integrating. The equation we can use for this purpose in general relativity is the equation for conservation of energy-momentum

$$\partial_\sigma \left(T_\alpha{}^\sigma \sqrt{-g}\right) - \frac{1}{2}\sqrt{-g}\; T^{\rho\sigma}\, \partial_\alpha g_{\rho\sigma} = 0 . \qquad (140)$$

Here $g_{\rho\sigma}$ is the metric and g is the determinant of the metric, and $T_{\mu\nu}$ is the energy-momentum tensor of matter and non-gravitational fields. From this one can get the equations of motion exactly, but at the expense of having in them the unknown metric. To be able to apply the equations one has to go back and try to get an expansion for the metric, as the metric is determined by non-linear partial differential equations. I shall just state the result for the equations of motion in second order for the retarded case with no other fields present. (In this order the results with other fields present are the same as obtained by the Dirac method.) We get

$$m_a \frac{d}{d\tau_a}\left[\left(1-\frac{1}{2}\chi^{\rho\sigma}v_{a\rho}v_{a\sigma}-\frac{1}{4}\chi_{\sigma}^{\ \sigma}\right)v_a^{\ \nu}+\chi^{\nu\sigma}v_{a\sigma}\right]$$

$$=\frac{1}{2}m_a\partial^{\nu}\chi^{\rho\sigma}v_{a\rho}v_{a\sigma}-\frac{1}{4}m_a\partial^{\nu}\chi_{\sigma}^{\ \sigma}-\frac{11}{3}Gm_a^2(\ddot{v}_a^{\ \nu}-v_a^{\ \nu}v_{a\sigma}\ddot{v}_a^{\ \sigma}). \tag{141}$$

Here G is the gravitational constant and $\chi^{\rho\sigma}$ is a first order approximation to a particular combination of the components of the metric tensor. It is again a solution of an inhomogeneous wave equation and is given by

$$\chi^{\rho\sigma}=-4G\sum_{k\neq a}\left(\frac{m_k v_k^{\ \rho}v_k^{\ \sigma}}{K}\right)_{\tau_r}. \tag{142}$$

Equation (141) is Lorentz-invariant and is essentially of the same type as the equations of special relativity we considered before. In Eqs. (141) and (142) the indices are understood to be raised and lowered by means of the Minkowski metric $\eta_{\mu\nu}$ rather than $g_{\mu\nu}$ as in Eq. (140).

Now suppose one wants to calculate a radiation problem, using the radiation reaction term in (141). How should one go about it? As you all know from electrodynamics, relativistic two-body problems are terribly difficult. Everybody solves one-body problems instead, approximating the electromagnetic field of one particle by a fixed external field acting on the other particle, and then using the Lorentz equation of motion to calculate the radiation. But a similar attempt to approximate $\chi^{\rho\sigma}$ by an external field leads to nonsensical answers. The nonsense is of the same nature as in the following example: Suppose I drop a piece of chalk. I have conservation of momentum for the system of chalk and earth when it drops. However, if I want to consider as an approximation that the earth is fixed, it does not make sense to talk about conservation of momentum of the piece of chalk falling in an external field; I lost this particular feature because of the way I approximated.

Gravitational radiation calculations are of the same type because of the absence of dipole radiation. The gravitational dipole moment is identically zero. This comes from the fact that the ratio of gravitational mass to inertial mass is the same for all particles. These questions are discussed extensively in Landau and Lifshitz [35]; I am not saying any more than Landau and Lifshitz did except that I explicitly consider equations of motion. The question now is how to approximate these. The fact that the dipole moment is identically zero means that if you do not use a one-body approximation but take the retardation into account by making an expansion of the gravitational potentials in v/c, then the first retardation term you get from the other particles will just cancel out

with the radiation reaction term in Eq. (141). The next term you'll get is quadrupole radiation. But if you use a one-body approximation to (141) then it appears that the radiation term gives you the radiation loss directly, while from the above discussion it follows that by itself the term is meaningless.

In short, then, the situation is that in electrodynamics, very fortunately or very unfortunately, it so happens that a one-body approximation is a physically excellent approximation to radiation problems. In gravitational theory a one-body approximation to the radiation problem is nonsense. You can't calculate radiation problems in gravitation except by explicitly considering the entire system of interacting masses.

I want to finish up by saying that for the problems we were considering in these lectures, general relativity is relevant in two ways. First of all it gives you a complete derivation of special relativistic equations of motion (if you stay within the linear theory) without having to assume conservation laws as Dirac does. I mentioned before that in the Dirac method there is an ambiguity because there are more conservation laws for some free fields than can be maintained in the presence of particles. General relativity selects the right conservation laws for you without ambiguity. In that respect it gives a full justification for the special relativistic equations we considered. Second, it also tells you that, although Einstein started to derive the fundamental equations from field theoretical concepts, it turns out that for radiation problems one can not get sensible answers if one wants to idealize a situation as one particle in an external field; one has to consider radiation in terms of a system of

particles and their effect on each other.

Peshkin: You say that in electrodynamics I know that I'm not making such a mistake because it is, after all, a linear theory?

Havas: No, I didn't say that — what I said was that it so happens that in electrodynamics you get a very good approximation because there the dipole moment is not zero in general. But it could be zero and you could rig up problems so that you get the wrong answer there too. If you would take a system consisting only of electrons, and then look at only one electron and idealize the effect of the others by an external field, you would get the same wrong answer because the dipole moment of the whole system is zero. If I calculate dipole radiation I get the wrong answer. But in electrodynamics you have to rig it up to get such a situation. In gravitation you get it necessarily.

Peshkin: So then in electrodynamics I am still making an error. Is it only that the effect I am calculating is much larger than the error?

Havas: Ordinarily, yes.

Peshkin: You say there is no way in which I could automatically correct for recoil terms?

Havas: No.

Peshkin: Essentially by making recoil corrections on all the operators or all the dynamical variables of the theory?

Havas: No, none that I am aware of anyhow. If you want to get Lorentz-invariant equations — which is the least you should hope for —

you are essentially stuck with Eq. (141). The original derivation of the general relativistic equations of motion by Einstein-Infeld-Hoffmann[33] assumed slow motion, which is a heck of a way of doing relativistic equations of motion, and which is an impossible way to get information about radiation. To obtain radiation terms you have to go to fantastically high orders of approximation and you get into such a mathematical mess that essentially you can't conclude anything about radiation with certainty. Now here you can get Lorentz-invariant equations and presumably what one knows about radiation from other such equations ought to be helpful. As I said, however, Eq. (141) as it stands misleads you if you want to make the usual one-body approximation to get around the mathematical difficulties of relativistic many-body problems.

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Bound States of Many-Particle Systems

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LECTURE I.

Ekstein: This is the first of two talks by Dr. Coester about bound states of many-particle systems. We have talked about the approach of the common man to problems of science so much and we have always exemplified the common man by the Iowa farmer, so today we are going to hear a genuine representative of the Iowa farmers, Professor Fritz Coester.

Coester: (Perhaps I should apologize to those readers whose hopes for an earthy treatment of the subject have been unduly raised by this kind introduction.)*

I would like to review some aspects of the perturbation theory of bound states. Specifically let's keep in mind a reasonably heavy nucleus, as an example. Much or most of what I have to say today will be more general. On the other hand certain things which are typical features of field theory will be ruled out. It is a peculiar feature of quantum mechanics that you can solve both time-dependent problems and time-independent problems with either time-dependent or time-independent theories. Scattering is a time-dependent problem. Nevertheless, there is a time-independent scattering theory and a time-dependent scattering theory. On the other hand, bound state problems are

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* Note added in proof.

essentially time-independent. Nevertheless there are time-dependent and time-independent methods for finding bound states and energy eigenvalues. It is a curious phenomenon that time-dependent theories are most popular with authors writing on bound state problems and time-independent theories seem to be more popular with authors writing on scattering theory. I will not attempt to explain this phenomenon. But I intend to review the formal relationship between various forms of perturbation theory.

Before I give an outline as to what I want to do today and why, let me introduce some notations and definitions so that I can state my problem more easily. I have a Hamiltonian H which I assume can be split into a zero order Hamiltonian H_0 and a perturbation V. Perturbation theory usually has a double connotation. On one hand it means that we relate a zero order state such as a shell model state, to the true eigenstate of the full Hamiltonian of the system. That is the weaker part of what one implies in perturbation theory and beyond this it also means that the zero order state is in some sense already an approximation to the true state and that we can get at the true state by expanding in powers of a small parameter. I would like to keep these two aspects separate as much as possible. The first one will always be present -- we have this relation of two states or groups of states, but instead of power series expansion, we shall use rigorous equations from which the perturbation series can be generated by iteration.

The standard procedure in quantum mechanical perturbation theory is to talk first about non-degenerate states and then generalize to the degenerate

case. I shall skip the non-degenerate part and treat the general perturbation theory in the first instance. So we have a set of g eigenstates of the Hamiltonian H_0 which belong to the same eigenvalue $\mathcal{E}_0$.

$$H_0 \phi_\nu = \mathcal{E}_0 \phi_\nu \qquad \nu = 1 \ldots g \tag{1}$$

The ϕ_ν span a g-dimensional vector space which can be specified by a projection operator Λ which, like all projection operators is hermitian and idempotent,

$$\Lambda^\dagger = \Lambda, \quad \Lambda^2 = \Lambda. \tag{2}$$

In this particular case it also has the property that

$$\Lambda H_0 = H_0 \Lambda = \Lambda H_0 \Lambda \tag{3}$$

and

$$\Lambda H_0 \Lambda = \mathcal{E}_0 \Lambda . \tag{3a}$$

The Hamiltonian H has a correspondent set of g eigenstates that do not all belong to the same eigenvalue. Let P be the projection operator which projects into this space.

$$P^2 = P \qquad P^\dagger = P, \quad PH = HP \tag{4}$$

and you can write

$$P = \sum_{n=1}^{g} P_n; \tag{5}$$

the projections P_n are orthogonal:

$$P_n P_m = \delta_{mn} P_n. \tag{6}$$

We may write

$$PH = \sum_n E_n P_n \tag{7}$$

where E_n are the eigenvalues of H. ψ_n are the normalized eigenfunctions of H.

Ekstein: Everything is discrete?

Coester: Everything I have talked about so far is discrete. I have not talked about all eigenstates of H_0, some of which may be in the contimum. I have not talked about eigenstates of H other than those under consideration. The spectrum of H may be partly continuous -- that does not matter.

So far we have introduced the Hamiltonian H_0 and H and the projection operators Λ and P. I need one more symbol

$$Z \equiv \Lambda P \Lambda . \tag{8}$$

I call it Z because it reminds me of an old friend: In the non-degenerate case where the sub-spaces are one-dimensional, the only non-vanishing eigenvalue of Z would be equal to the probability of finding the unperturbed state in the unperturbed state.

Peshkin: Does Λ commute with P?

Coester: Λ does not commute with P. If you want to have a geometrical picture take three-dimensional space as a model for the whole Hilbert space and take two planes which have an angle between them as the model for the sub-spaces. P and Λ are respectively the projection operators into these planes.

Moldauer: I don't understand the definition of P. Is that meant to be a unique definition?

Coester: Yes. This perhaps becomes more apparent if one writes P in the familiar form

$$P = \sum_n \psi_n \psi_n^* .$$

Moldauer: But what is n ?

Coester: n labels eigenstates of the Hamiltonian. Your question is, if I understand it correctly, whether the correspondence with the eigenstates ϕ_ν of H_0, which I have postulated here, is unique. The correspondence is unique if the perturbation expansion in powers of V converges. If the expansion does not converge but V is bounded, then the modified problem with the interaction ηV $(0 < \eta < 1)$ has a converging perturbation expansion for sufficiently small η. If solutions of the modified problem can be found for all $\eta (0 < \eta \leq 1)$ and if these solutions depend continuously on η then this continuity establishes a unique correspondence. We should remember that the choice of H_0 is largely

arbitrary. The requirement that there should be a unique correspondence between the relevant eigenstates of H_0 and H restricts the acceptable choices for H_0.

The matrix $Z_{\mu\nu} \equiv (\phi_\mu , Z\phi_\nu)$ defined by Eq. (8) has an inverse if there is no vector in the space P, which is orthogonal to every vector in the space Λ. There is then an operator Y which by definition has the following properties

$$Z Y = Y Z = \Lambda \tag{9}$$

and

$$Y \Lambda = \Lambda Y = Y . \tag{10}$$

The matrix $Y_{\mu\nu}$ is the inverse of the matrix $Z_{\mu\nu}$. Y and P satisfy the identity

$$P Y P = P \tag{11}$$

which can be easily proven by introducing a set of basis vectors in the spaces P and Λ respectively.

Our problem is to find eigenvalues E_n and eigenfunctions ψ_n of the Hamiltonian H. Different perturbation theories yield perturbation series for different quantities. For instance, non-degenerate Rayleigh-Schrodinger perturbation theory yields directly a series for an eigenfunction Ω of H which is normalized according to $(\phi , \Omega) = 1$ where ϕ is the unperturbed state. A series for the normalized eigenfunction Ψ may be obtained indirectly from the equation

$$\Psi = \frac{\Omega}{\sqrt{(\Omega , \Omega)}} . \tag{12}$$

On the other hand, Kato's perturbation theory yields directly a series for P. The purpose of my talk today is to review these different perturbation schemes in an operator form which automatically allows for degeneracies and to discuss the relationships between them.

In order to formulate the Rayleigh-Schrodinger perturbation theory in this manner we need an operator F with the following properties:

$$F\Lambda = F \tag{13}$$

$$\Lambda F = \Lambda \tag{14}$$

and

$$HF = F\Lambda HF\,. \tag{15}$$

This operator F plays in the general case the part of the wave function Ω mentioned earlier. We see immediately that $F \equiv PY$ satisfies Eqs. (13) and (14); we proceed to prove that it also satisfies Eq. (15). We have

$$HF = HPY = PHY\,. \tag{16}$$

From Eq. (11) follows

$$PHY = PYPHY = PYHPY\,. \tag{17}$$

Hence we obtain from Eqs. (16) and (17)

$$HF = FHF = F\Lambda HF\,. \tag{18}$$

Equation (15) has thus been verified.

In order to obtain a perturbation solution of Eq. (15) we write it in the form

$$-(H_0 - \mathcal{E}_0) F = V F - F G \tag{19}$$

where $\mathcal{E}_0$ is defined by (1) and G is by definition

$$G = \Lambda H F - \mathcal{E}_0 \Lambda \tag{20}$$

or

$$G = \Lambda V F . \tag{20a}$$

Because of Eq. (14), Eq. (19) may be written in the form

$$F = \Lambda + (1-\Lambda)(\mathcal{E}_0 - H_0)^{-1} (V F - F G) . \tag{21}$$

This is a non-linear integral equation because G also depends on F. One can iterate by successive substitution and arrive at a perturbation series for F in this manner. In the non-degenerate case this gives directly the Rayleigh-Schrodinger series for the wave function and for G which would then be the energy shift. In a non-degenerate problem $\mathcal{E}_0 + G$ is the eigenvalue of H, and Eq. (15) is the stationary state Schrodinger equation. In the degenerate case we are not through yet; we must determine the eigenvalues.

Let ϕ_ν $(\nu = 1 \ldots g)$ be a set of basis vectors in the space Λ. We obtain then the eigenfunction ψ_n of H in the form

$$\psi_n = \sum_{\nu=1}^{g} F \phi_\nu T_{\nu n} \qquad n = 1 \ldots \tag{22}$$

where the coefficients $T_{\nu n}$ are yet to be determined. From

$$H \psi_n = E_n \psi_n , \tag{23}$$

Eqs. (22) and (15) follow

$$\sum_{\nu} (G_{\mu\nu} - (E_n - \tilde{\sigma}_0) \quad \delta_{\mu\nu}) T_{\nu n} = 0 . \tag{24}$$

$(G_{\mu\nu})$ is in general not a hermitian matrix and $(T_{\nu n})$ is not a unitary matrix. The "good" zero order states $\sum_{\nu} \phi_\nu T_{\nu n}$ need not be orthogonal.* Since

$$(\psi_n, \psi_m) = \delta_{nm} \tag{25}$$

by definition there follows

$$(\psi_n, \psi_m) = \sum_{\mu\nu} T_{\mu\nu} Y_{\mu\nu} T_{\nu m} = \delta_{mn}. \tag{26}$$

Y can be computed from F according to

$$F^\dagger F = Y P Y = Y . \tag{27}$$

Our formalism lends itself to certain easy generalizations. It is not necessary that all states in Λ belong to the same eigenvalues of H_0. Let us retain Eq. (3) but abandon (3a). The energy $\mathcal{E}_0$ in Eqs. (19), (20) and (21) is

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* This was recently emphasized by C. Bloch (ref. 2).

now no longer an eigenvalue of H_0 but a parameter chosen for convenience. Equation (20a) is no longer valid . In this manner we obtain a very flexible perturbation scheme. If we enlarge the space Λ we improve the convergence of the perturbation solution of (21). At the same time, the complexity of the finite eigenvalue problem (24) increases. In the non-degenerate case we may choose the numbers $\mathcal{E}_0$ and G in any convenient manner consistent with the relation $\mathcal{E}_0 + G = E$. As a special case we get from (21) with $\mathcal{E}_0 = E$ and $G = 0$

$$F = \Lambda + (1 - \Lambda)\,(E - H_0)^{-1}\, V F \,. \tag{21a}$$

Iteration of (21a) yields the Wigner-Brillouin perturbation series.

A perturbation theory which yields directly a perturbation series for P is due to Kato. It uses the following integral representation for P

$$P = \frac{1}{2\pi i} \oint_C \frac{dE}{E - H} \,. \tag{28}$$

The contour C encloses all the eigenvalues E_n. The perturbation theory is based on the identity

$$\frac{1}{E - H} = \frac{1}{E - H_0} + \frac{1}{E - H_0}\, V\, \frac{1}{E - H} \,. \tag{29}$$

By iterating (29) one obtains a perturbation series for $(E - H)^{-1}$ and hence

$$P = \sum_{n=0}^{\infty} \frac{1}{2\pi i} \oint \left(\frac{1}{E - H_0} V\right)^n \frac{1}{E - H_0} \,. \tag{30}$$

In order to compare this result to the Rayleigh-Schrodinger series we must compute Z by (8) and F by $F = PY$. An expansion of P rather than F may be advantageous if one wants to compute the expectation values of quantities other than the energy.

I finally come to the time dependent method.

Ekstein: What are the references on this, or is this yours?

Coester: The substance of my talk is general knowledge. There is, of course, a lot of literature on the subject. I'd like to mention a series of papers by Bloch[2] and a UCRL report by De Witt.[3] The perturbation expansion of the resolvent $(E - H)^{-1}$ has recently been extensively investigated by Van Hove and Hugenholtz.[4]

A popular method of solving bound state problems is to switch on the interaction V adiabatically. I would like to show how the energy eigenvalues and eigenfunctions are obtained by this procedure and discuss its relation to the perturbation schemes we have discussed so far.

Let

$$H_\alpha \equiv H_0 + e^{\alpha t} V \ , \ t<0 \qquad\qquad \alpha > 0 \ , \tag{31}$$

and

$$V_\alpha(t) \equiv e^{iH_0 t} \ V \ e^{-iH_0 t} \ e^{\alpha t} \ . \tag{32}$$

The Schrodinger equation in the interaction picture is

$$i \frac{d U_a(t, -\infty)}{dt} = V_a(t)\, U_a(t, -\infty). \tag{33}$$

The corresponding integral equation is

$$U_a(t, -\infty) = 1 - i \int_{-\infty}^{t} dt'\, V_a(t')\, U_a(t', -\infty). \tag{34}$$

If one solves (34) by iteration and carries out all the time integrations, one finds with (3a)

$$U_a(0, -\infty)\, \Lambda = \Lambda + \left(\sum_{n=1} \frac{1}{\mathcal{E}_0 - H_0 + in\alpha} V \frac{1}{\mathcal{E}_0 - H_0 + i(n-1)\alpha} V \cdots \frac{1}{\mathcal{E}_0 - H_0 + i\alpha} \right) \Lambda. \tag{35}$$

Formally, $U_a(0, -\infty)$, $\alpha \to 0$ is analogous to the Møller operator in scattering theory. In fact the Møller operator may be calculated in exactly the manner outlined above. The exact form of the imaginary parts of the energy denominators does not matter in simple scattering problems. It is well known that for simple scattering problems one may replace $n_\alpha \to \alpha$, $(n-1)\,\alpha \to \alpha \ldots$ in (35) in the limit $\alpha \to 0$. In our case the situation is quite different. $\lim_{\alpha \to 0} (U_\alpha(0, -\infty)$ does in general not exist, and for those quantities which have a limit the exact form of the denominators is important. We have defined a modified time dependent Hamiltonian, we have solved the time dependent Schrodinger equation

in terms of an integral equation and in terms of a series. So far we know nothing of what this has to do with the bound state problem. The connection is established by the "adiabatic Theorem".[5]

$$\lim_{a\to 0} \; U_a(0, -\infty)\; \Lambda\; U_a(0, -\infty)^\dagger \;=\; P\,. \tag{36}$$

A formal proof which assumes the convergence of the perturbation series can easily be obtained from (35) as follows:

$$\lim_{a\to 0} \; U_a(0, -\infty)\; \Lambda\; U_a(0, -\infty)^\dagger$$

$$= \Lambda + \lim_{a\to 0} \sum_{n=1}^{\infty} \sum_{m=0}^{n} \frac{1}{\xi_0 - H_0 + im a} V \cdots \frac{1}{\xi_0 - H_0 + ia} V\Lambda V \frac{1}{\xi_0 - H_0 - ia} \cdots V \frac{1}{\xi_0 - H_0 - (n-m)ia}$$

$$= \Lambda + \sum_{n=1}^{\infty} \lim_{a\to 0} \frac{1}{2\pi i} \oint \left(\prod_{m=0}^{n-1} \frac{1}{E - H_0 + i a (n-m)} V \right) \frac{1}{E - H_0} \, dE. \tag{37}$$

In the last expression we can go to the limit $a \to 0$ and obtain

$$\lim_{a\to 0} \; U_a(0, -\infty)\; \Lambda\; U_a(0, -\infty)^\dagger \;=\; \sum_{n=0}^{\infty} \frac{1}{2\pi i} \oint \left(\frac{1}{E - H_0} V \right)^n \frac{1}{E - H_0} \, dE = P. \tag{38}$$

Haag: I would like to object to the name "bound state" problem because the part of the spectrum of H which you do use is the same as that of H_0. What one usually means by bound-state problem is the problem of finding eigenstates of H which you didn't have before in H_0.

I just mention it because this morning we talked about the Bethe-Salpeter equation. Now this thing here is a completely different problem. With the Bethe-Salpeter equation you determine a state which has no counterpart among the eigenstates of H_0.

Coester: What one usually means by bound-state problems depends somewhat on whether one is a field theorist. The problem of finding the binding energy and ground state wave function of Pb^{208} would usually be called a bound state problem by people interested in such problems. The methods discussed here are certainly relevant to this problem. Quite generally the bound state problem is the problem of finding normalizable eigenstates of H and the corresponding eigenvalues. The method of guessing a suitable space of trial states Λ, constructing H_0 and improving these trial solutions systematically, seems to be a legitimate approach to the general problem among others. H_0 and Λ are not God-given quantities which then "usually" have the wrong properties, but guesses involving considerable freedom. In order that these methods be applicable one must have some information about the topological structure of the spectrum of H. Such qualitative information is available for nuclei. On the other hand it is, of course, not difficult to write down Hamiltonians for coupled fields for which that is not the case.

Haag: Then what you are doing is just perturbation theory in the discrete spectrum in an operator form?

Coester: Yes.

Haag: For practical purposes is it necessary to multiply out two expansions as you do in Eq. (36)?

Coester: Equation (36) is the simplest relation I know which establishes a connection between $U_a(0, -\infty)$ and the time independent formalism. U_a itself does not have a limit $a \to 0$. I shall soon come to discuss formulae which are more useful than (36) for practical computations. They are all easily derived from (36).

Ekstein: There are two questions on the equation. Would you please write it down for the case where P is one-dimensional operator for the benefit of part of the audience, including me? The second question is, "In what sense has this theorem been proved?"

Coester: The theorem has been proved by comparing the perturbation expansion for P term by term with the perturbation expansion of the left-hand side of Eq. (36).

Tanaka: What does this prove?

Coester: It proves Eq. (36) if the expansions converge; if they don't converge it does not prove anything.

Now your other question: Write (36) down for the case where P is one dimensional. Λ is then also one dimensional. Let ψ and ϕ be the corresponding vectors. We may then write (36) in the form

$$\lim_{a \to 0} \quad U_a \phi \, (U_a \phi)^+ = \psi \psi^+ . \tag{36a}$$

Haag: Could you put this in the form

$$\lim_{a\to 0} U_a \phi = \psi \ ?$$

Coester: No, because this limit does not exist.

Haag: Can you explain how this can happen? After all, the projection into a single vector and the vector itself are almost the same thing. I mean, how is it that the projection exists and the vector does not?

Coester: I should have been more careful in discussing the limits involved. Let $U_a^{(n)} \Lambda$ be a partial sum of the expansion (35)

$$U_a^{(n)} \Lambda = \Lambda + \sum_{n'=1}^{n} \frac{1}{\epsilon_0 - H_0 + in'a} V \ldots \qquad \Lambda.$$

It is easy to verify that $\lim_{a\to 0} U_a^{(n)} \phi$ does not exist in general. Therefore,

$$\lim_{a\to 0} U_a^{(n)} \Lambda U_a^{(n)\dagger}$$

also does not exist. On the other hand we have proven that

$$\lim_{a\to 0} \sum_{m=0}^{n} U_a^{(n-m)} \Lambda U_a^{(m)\dagger}$$

does exist. In fact we have proven

$$\lim_{a\to 0} \sum_{m=0}^{n} U_a^{(n-m)} \Lambda U_a^{(n)\dagger} = \sum_{n'=0}^{n} \frac{1}{2\pi i} \oint \left(\frac{1}{E - H_0} V\right)^{n'} \frac{1}{E - H_o} dE \ . \tag{38a}$$

The existence of the limit $n\to\infty$ is merely assumed. I referred to this limit

when I assumed earlier "the convergence of the perturbation expansions".

From (36) follows

$$Z = \Lambda P \Lambda = \lim_{a \to 0} z_a z_a^\dagger = \lim_{a \to 0} Z_a \tag{39}$$

and

$$P\Lambda = \lim_{a \to 0} U_a z_a^\dagger \tag{40}$$

where by definition

$$z_a = \Lambda U_a \Lambda \tag{41}$$

and

$$Z_a = z_a z_a^\dagger . \tag{42}$$

There follows further

$$F = P Y = \lim_{a \to 0} U_a y_a , \tag{43}$$

where y_a is related to z_a in the same manner in which Y is related to Z. [(see Eqs. (9) and (10)]. Equation (43) takes a familiar form * if Λ is one dimensional (non-degenerate case)

$$\Omega = F\phi = \lim_{a \to 0} \frac{1}{\langle U_a \rangle} U_a \phi . \tag{44}$$

If we replace V by ηV in the expansion (35), we can easily verify that

$$\Lambda V U_a = i a \Lambda \left(\frac{\partial U_a}{\partial \eta} \right)_{\eta = 1} . \tag{45}$$

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* See, for instance, M. Gell-Mann and F. Low, ref. 6.

There follows from (43) and (45)

$$G \equiv \Lambda V F = \lim_{a \to 0} i a \Lambda \left(\frac{\partial U_a}{\partial \eta} \right)_{\eta = 1} y_a \qquad (46)$$

In non-degenerate case (46) reduces to the following well-known formula for the energy shift

$$G = \lim_{a \to 0} ia \left(\frac{\partial \ell n \langle U_a \rangle}{\partial \eta} \right)_{\eta = 1} . \qquad (47)$$

So far we have discussed bound state perturbation theory without utilizing the particle structure of the system. In my next lecture I intend to discuss those features of the theory which occur specifically in many-particle problems.

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LECTURE II.

All the perturbation schemes which I discussed last time have been applied to many-particle systems. Today only the time-independent formulation will be used. In order to keep the formulas reasonably simple, I assume that the zero order state is non-degenerate. Concrete results are available only for that case. It is easy, however, to formulate the basis equation also for the degenerate case. I shall outline later on how this is done.

Let me quickly review those results of the first lecture which we need today. Let ϕ be the normalized zero order state and Λ the projection operator onto ϕ. The corresponding eigenstate Ω of H is normalized by the condition

$$(\Omega, \phi) = 1 . \tag{1}$$

The operator F which transforms ϕ into Ω ($\Omega = F\phi$) has by definition the properties

$$F \Lambda = F \tag{2}$$

$$\Lambda F = \Lambda \tag{3}$$

$$H F = F \Lambda H F \tag{4}$$

$$\Lambda H F = E \Lambda \tag{5}$$

where E is the desired eigenvalue of H. In the following, ϕ is always represented by a Slater determinant formed from suitable single particle wave functions $u_\nu(x)$. In an occupation number representation ϕ is represented

by

$$\phi = \prod_{\nu}^{\Gamma} a_{\nu}^{+} \phi_0 \tag{6}$$

where the product $\prod^{\Gamma}_{\nu} \ldots$ is taken over all occupied single particle states, and ϕ_0 is the vacuum state. a_{ν}^{+} creates a particle in the state ν. The operator F can then be represented in the form

$$F = \Lambda + \sum_{k=1}^{A} \underset{\sim}{F}_k \Lambda \tag{7}$$

where by definition

$$\underset{\sim}{F}_k = \sum_{\rho_1 \ldots \rho_k} \sum_{\nu_1 \ldots \nu_k} \frac{1}{(k!)^2} a^{\dagger}_{\rho_1} \ldots a^{\dagger}_{\rho_k} a_{\nu_1} \ldots a_{\nu_k} F_k(\rho_1 \ldots \rho_k, \nu_1 \ldots \nu_k). \tag{8}$$

The indices ρ and ν vary respectively over the empty states and the occupied states. The entire system of single particle states is by assumption complete and orthonormal.

$$\sum_{\nu} u_{\nu}(x)\, u^{*}_{\nu}(x') + \sum_{\rho} u_{\rho}(x)\, u^{*}_{\rho}(x') = \delta(x - x') . \tag{9}$$

$F(\rho_1 \ldots \rho_k, \nu_1 \ldots \nu_k)$ is the relative probability amplitude for finding in the state Ω the configuration with k excited particles in the states $\rho_1 - \ldots \rho_k$ and k holes in the states $\nu_1 - \ldots \nu_k$. The relation between Ω and ϕ is very similar to the relation between the physical vacuum and the bare vacuum in field theory. The choice of particular single particle states $u_{\nu}(x)$ is so far quite arbitrary.

The functions F_k change in a very complicated manner if the single particle basis is altered.

If the Hamiltonian H is of the form

$$H = -\int dx\, a^+(x) \frac{\nabla^2}{2m} a(x) + \frac{1}{2} \int dx \int dx'\, a^+(x)\, a^+(x')\, V(x-x')\, a(x')\, a(x) \tag{9}$$

$$a(x) = \sum_\nu a_\nu\, u_\nu(x) + \sum_\rho a_\rho\, u_\rho(x) \tag{10}$$

it is easy to see that only F_1 and F_2 contribute to the expression for the energy

$$E = (\phi\,,\ H\underset{\sim}{F}\phi) = (\phi\,,\ H(1+\underset{\sim}{F}_1+\underset{\sim}{F}_2)\phi)\ . \tag{11}$$

A good approximation to the energy but <u>not</u> to the wave function can therefore always be obtained by construction a good approximation to $\underset{\sim}{F}_1$ and $\underset{\sim}{F}_2$ only. The vector equation

$$H\underset{\sim}{F}\phi = E\underset{\sim}{F}\phi \tag{12}$$

is equivalent to the system

$$\phi\,,\ H(1+\underset{\sim}{F}_1+\underset{\sim}{F}_2)\phi = E \tag{13}$$

$$(\phi,\ a_\nu^+\, a(x)\ H(1+\underset{\sim}{F}_1+\underset{\sim}{F}_2+\underset{\sim}{F}_3)\phi) = E\,\psi_\nu(x) \tag{14}$$

$$\left(\phi\,,\ a_{\nu_1}^+\, a_{\nu_2}^+\, a(x_2)\, a(x_1)\, H(1+\underset{\sim}{F}_1+\underset{\sim}{F}_2+\underset{\sim}{F}_3+\underset{\sim}{F}_4)\phi\right) = E\,\psi_{\nu_1\,\nu_2}(x_1\,x_2) \tag{15}$$

$$(\phi\,,\ a_{\nu}^{+}\ldots a_{\nu_k}^{+}\quad a(x_k)\ldots a(x_1)\ H(1+\sum_{q=1}^{k+2}\ \underset{\sim}{F}_q)\ \phi) = E\,\psi_{\nu_1\ldots\nu_k}(x_1\ldots x_k) \tag{16}$$

where the wave functions on the right hand side are defined by

$$\psi_{\nu_1\ldots\nu_k}(x_1\ldots x_k) = (\phi\,,\ a_{\nu_1}^{+}\ldots a_{\nu_k}^{+}\ a(x_k)\ldots a(x_1)\ \underset{\sim}{F}\phi). \tag{17}$$

The problem is to replace this system by an approximate set of equations which determine $\underset{\sim}{F}_1$ and $\underset{\sim}{F}_2$. The simplest and crudest way of doing this is the Hartree-Fock approximation. It consists of putting $\Omega \cong (1+\underset{\sim}{F}_1)\phi$ and retaining only eqs. (13) and (14). These equations determine F_1 and E in this approximation. The equations take on a more familiar form if one requires the $u_\nu(x)$ to be self-consistent single particle functions defined by the condition $u_\nu(x) = \psi_\nu(x)$ or $\underset{\sim}{F}_1 = 0$. Equation (14) is then the usual Hartree-Fock equation for the self-consistent single particle wave functions.

Broadly speaking, the Brueckner approximation is an improvement of the Hartree-Fock approximation: $\underset{\sim}{F}_1$ and $\underset{\sim}{F}_2$ are determined from approximations to Eqs. (14) and (15). Originally the approximation was developed only for nuclear matter, that is for an unlimited medium. Homogeneity of the medium fixes the $u_\nu(x)$ to be plane waves and $\underset{\sim}{F}_1 = 0$. The approximate equation for $\underset{\sim}{F}_2$ was at first obtained by heuristic considerations rather than a systematic approximation from the exact Schrodinger equation. Later Goldstone[7] derived the equation for $\underset{\sim}{F}_2$ by summation of selected terms in the Rayleigh-

Schrodinger perturbation expansion. This expansion can be obtained by iterating the equation

$$F = \Lambda + \frac{1-\Lambda}{E_0-H_0}(W-G)F \tag{18}$$

where $H = H_0 + W$ and $E = E_0 + G$. In every order of the expansion one obtains a large number of terms for F and G due to the complexity of the many particle structure. One can best keep track of these many terms with the aid of diagrams similar to the Feynmann diagrams of field theory. Matrix elements which are products of independent matrix elements are represented by disconnected diagrams. Essential for Goldstone's derivation was the proof that all disconnected diagrams in the expansion of G cancel. The perturbation series of G is obtained entirely in terms of connected diagrams.

Kaplan: What is the undesirable feature of terms described by disconnected diagrams and what is their physical significance?

Coester: For nuclear matter, diagrams without external lines are proportional to the volume. Since the total energy is volume proportional products of disconnected diagrams must cancel in the expansion for G.

It is possible to generate the perturbation series in such a manner that only linked diagrams arise and the procedure of collecting and cancelling terms is avoided.[8] For that purpose we define an operator S by the equation

$$F = e^{\underline{S}}\Lambda \tag{19}$$

where $\underset{\sim}{S}$ is of the form

$$\underset{\sim}{S} = \sum_{k=1}^{A} \underset{\sim}{S}_k$$

$$\underset{\sim}{S}_k = \sum_{\rho_1 \cdots \rho_k} \sum_{\nu_1 \cdots \nu_k} a^+_{\rho_1} \cdots a^+_{\rho_k} \; a_{\nu_1} \cdots a_{\nu_k} \; \frac{1}{(k!)^2} \underset{\sim}{S}_k(\rho_1 \cdots \rho_k \, \nu_1 \cdots \nu_k). \tag{20}$$

Haag: What is the justification for writing F as an exponential?

Coester: Equation (19) with (20) is not an assumption about F, but a definition of $\underset{\sim}{S}$. It is a compact form of the system

$$\begin{aligned} \underset{\sim}{F}_1 &= \underset{\sim}{S}_1 \\ \underset{\sim}{F}_2 &= \underset{\sim}{S}_2 + \frac{1}{2} \underset{\sim}{S}_1^2 \\ \underset{\sim}{F}_3 &= \underset{\sim}{S}_3 + \underset{\sim}{S}_1 \cdot \underset{\sim}{S}_2 + \frac{1}{3!} \underset{\sim}{S}^3 \end{aligned} \tag{21}$$

which can always be solved for the $\underset{\sim}{S}_k$ in terms of the $\underset{\sim}{F}$'s. The exponential series breaks off with the power A because of the exclusion principle. The purpose in the definition (19) is to obtain operators S_k which have only linked diagrams in their perturbation expansion. I could write a power series with arbitrary coefficients for (19) and then later determine the coefficients from the requirement that $\underset{\sim}{S}_k$ contain only linked diagrams.

Haag: The $\underset{\sim}{F}_k$ only have a physical meaning if you apply them to this one state ϕ. What fixes them uniquely, I mean what is so special when you can take many $\underset{\sim}{F}$'s which would do the same thing?

Coester: The $\underset{\sim}{F}_k$ contain by definition only creation operators for the empty states and annihilation operators for the filled states. The $\underset{\sim}{S}_k$ have the same property. Hence they all commute with each other. Any other operators which have the same effect on ϕ differ by terms containing annihilation operators a_ρ of empty states and creation operators a_ν^+ of filled states.

With the definition (19) we may write instead of (12)

$$e^{-\underset{\sim}{S}} H e^{\underset{\sim}{S}} \phi = E \phi \ . \tag{22}$$

The expansion

$$e^{-\underset{\sim}{S}} H e^{\underset{\sim}{S}} = H + [H, S] + \frac{1}{2!} \left[[H, \underset{\sim}{S}] \underset{\sim}{S} \right] + \ldots$$

breaks off with the fourth order term if H is given by (9). A perturbation series for $\underset{\sim}{S}$ can be generated as follows. Let $H = H_0 + W$, $H_0 \phi = E_0 \phi$. If we multiply (22) by $(1-\Lambda)$ we have

$$[H_0, \underset{\sim}{S}] \phi + (1-\Lambda)(W + [W, \underset{\sim}{S}] + \frac{1}{2} \left[[W, \underset{\sim}{S}], \underset{\sim}{S} \right] + \ldots) \phi = 0 \tag{23}$$

Upon expansion of $\underset{\sim}{S}$ in powers of W

$$\underset{\sim}{S} = \sum_{n=1}^{\infty} \underset{\sim}{S}^{(n)} \tag{24}$$

Eq. (23) gives us $[H_0, \underset{\sim}{S}^{(n)}]$ and therefore $\underset{\sim}{S}^{(n)}$ in terms of lower order $\underset{\sim}{S}^{(n')}$, $n' < n$.

Ekstein: Do you mean to say that the commutator of an unknown operator with H_0 determines this operator?

Coester: Not in general, but in this case it does because of the structure which $\underline{S}$ has by its definition, Eq. (20). Specifically we have

$$[H_0, \underline{S}_k] = \sum_{\rho_1 \cdots \rho_k} \sum_{\nu_1 \cdots \nu_k} a^+_{\rho_1} \cdots a^+_{\rho_k} a_{\nu_1} \cdots a_{\nu_k} \left(\frac{1}{k!}\right)^2$$

$$\times (\omega_{\rho_1} + \omega_{\rho_2} + \cdots + \omega_{\rho_k} - \omega_{\nu_1} - \omega_{\nu_2} \cdots \omega_{\nu_k}) \; S_k(\rho_1 \cdots \rho_k, \nu_1 \cdots \nu_k) \qquad (25)$$

where ω_ρ and ω_ν are the single particle energies of the levels ρ and ν respectively. Since $\omega_\rho > \omega_\nu$ for all ρ and ν the energy difference can be divided out. By definition $\underline{S}_k$ does not have any diagonal matrix elements with respect to the eigenstates of H_0. The perturbation series for $\underline{S}$ obtained from (23) in this manner contains only matrix elements represented by linked diagrams. The $\underline{S}$'s occur everywhere in multiple commutator expressions which guarantee that every factor is "linked" to every other factor.

The physical ideas involved in the Brueckner approximation have been discussed at length by Gomez, Walecka and Weisskopf.[9] One must assume that the exact wave function is well approximated by a Slater determinant in that region of configuration space where the distance between any two nucleons is larger than a certain "healing distance" d. The single particle wave functions which make up this Slater determinant are the "self consistent" single particle

wave functions. If they are used to construct a complete single particle basis we have $\underset{\sim}{F}_1 = 0$. Systematic approximations to Eqs. (13), (14), (15), based on these ideas have been discussed by Brenig[10] and Kümmel[11]. One can obtain in this manner a set of simultaneous equations which determine the functions $u_\nu(x)$ and $\psi_{\nu_1 \nu_2}(x_1, x_2)$. These equations are similar to the equations on which Brueckner, Gammel and Weitzner base their "Theory of Finite Nuclei".[12] They differ, however, in significant aspects. Nothing is known at this time about the numerical importance of these differences.

Ekstein: Could you say a few words about the effective two-body interaction which replaces the singular two-body potentials in this theory?

Coester: One defines a matrix $(x_2 x_1 \left| K \right| \nu_1 \nu_2)$ by

$$(x_2 x_1 \left| K \right| \nu_1 \nu_2) = v(x_1 - x_2) \psi_{\nu_1 \nu_2}(x_1 x_2). \tag{26}$$

The total energy of the system is then

$$E = -\sum_\nu \int dx\, u_\nu^*(x) \frac{\nabla^2}{2m} u_\nu(x) + \frac{1}{2} \sum_{\nu_1 \nu_2} (\nu_2 \nu_1 \left| K \right| \nu_1 \nu_2) \tag{27}$$

where

$$(\nu_2 \nu_1 \left| K \right| \nu_1 \nu_2) \equiv \int dx_1 \int dx_2\, u_{\nu_2}^*(x_2)\, u_{\nu_1}^*(x_1)\, (x_2 x_1 \left| K \right| \nu_1 \nu_2) \tag{28}$$

The matrix (26) is uniquely defined and it arises naturally in the theory. The definition of an operator which has the matrix elements (28) is quite ambiguous. Some of the differences which I mentioned earlier are due to this ambiguity.

Haag: What remains in this scheme of the original idea that one could get away from a nucleon-nucleon potential and get results in terms of experimental nucleon-nucleon phase shifts?

Coester: Nothing, I believe. In the case of an infinite medium the matrix $(\underline{k}'_2\, \underline{k}'_1 \,|K|\, \underline{k}_1\, \underline{k}_2)$ satisfies an equation similar to a scattering equation. It is not an acceptable approximation to mutilate this equation to the point where it becomes identical with a scattering equation for free nucleons.

So far our scheme is applicable only if all single particle levels are either completely filled or completely empty. If some levels are partly filled the zero order energy is degenerate. The formalism can then be generalized along the lines described in the first lecture. The operator F has still the form given in Eqs. (7) and (8) but the number of single particle states u_ν is larger than the particle number A. The subspace Λ is spanned by all Slater determinants formed with A of the u_ν's. Let ϕ_α, ϕ_β, ... be basis vectors in this subspace. The energy is now obtained by diagonalizing the finite matrix

$$(\phi_\alpha,\ HF\phi_\beta)\ .$$

$u_\nu(x)$ and $F_2(x)$ must be obtained by methods similar to those used before.

In conclusion I might summarize the present situation as follows: It is quite plausible that the energies of finite nuclei can be calculated to a good approximation by the general methods we have discussed. The development of practical approximation procedures along these lines is still in a preliminary stage. There is no reason why these methods should lead to good approximations for nuclear moments, the density distribution and transition matrix elements.

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