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# ISOSPIN

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Résumé.-
Nous décrivons l'histoire de l'isospin, en commençant par l'introduction par Heisenberg (1932) du concept (ρ-spin) associé avec l'idée que le proton et le neutron sont deux états de la même entité (appelée plus tard nucléon). On souligne que dès le début Heisenberg avait à l'esprit l'image de la transmission de l'interaction proton-neutron par un agent (un champs). Par contre, la modification due à Majorana du schéma d'interaction de Heisenberg rejetait le formalisme d'isospin et s'éloignait de la physique des champs.

La réapparition de l'idée d'isospin par Cassen et Condon (1934) est basée sur l'indépendance de charge de l'interaction nucleon-nucleon et nous discutons l'iso-invariance formelle de cette interaction. Nous décrivons les applications étendues du concept d'isospin à la théorie de la structure des noyaux, en particulier, l'introduction (Wigner, 1937) du vecteur isospin total d'un système de nucléons, la classification des états en multiplets d'isospin et les applications aux réactions nucléaires, à la radioactivité β, etc...

Nous décrivons ensuite l'introduction de l'isospin dans les théories des champs de l'interaction nucléon-nucléon ; l'extension de l'indépendance de charge d'abord au champs (β,ν) de Fermi (Kemmer, 1937) puis au champs de Yukawa (Kemmer, 1938). Nous rendons compte brièvement de l'affermissement de la "theorie symétrique" de l'isospin basé, après guerre, sur la preuve de l'existence de l'isotriplet de pions et la meilleure compréhension du concept de renormalisation en théorie des champs. Puis nous signalons l'application de l'iso-invariance au système N-π pour la description de nombreux phénomènes.

Enfin nous décrivons rapidement les premières étapes de la théorie non-abélienne de gauge : le champs iso-vectoriel de Yang et Mills. Nous concluons par le brèves remarques sur le statut de l'isospin dans le cadre de la théorie moderne des particules.

Abstract.-
The history of isospin is described, starting from the introduction by Heisenberg (1932) of the concept (ρ-spin), associated with the idea that proton and neutron are two states of the same entity (later to be called "nucleon"). It is stressed that from the start Heisenberg had the picture of the transmission of proton-neutron interaction by a charged agent (field) in mind. It is shown that, in contrast, Majorana's modification of Heisenberg's interaction scheme rejected the isospin formalism and moved away from a field picture.

The revival of the isospin idea by Cassen and Condon (1934) on the basis of the "charge-independent" description of nucleon-nucleon interaction and the formal "iso-invariance" of that interaction is discussed. The consequent wide application of the isospin idea in nuclear structure theory is described. In particular the introduction (Wigner, 1937) of the total isospin vector of a system of nucleons, of isospin multiplet classification of states and of applications to nuclear reactions, $\beta$-decay, etc.

The introduction of isospin into field theories of nucleon-nucleon interaction is described: the charge independent extension first of the Fermi $(\beta,\nu)$ field (Kemmer, 1937) and then of the Yukawa field (Kemmer, 1938). A brief account is given of the consolidation of the "symmetric theory" of the isospin of the nucleon-pion system, following the post-war experimental proof of the existence of the pion isotriplet and the improvement in the understanding of field theories in terms of the renormalisation concept. The application of this iso-invariance of the $(N-\pi)$ system in the description of a wide range of phenomena (weak interactions) is sketched.

The first step into non-abelian gauge theory - the Yang-Mills iso-vector gauge field - is briefly described, concluding with brief remarks on the status of isospin within the framework of modern particle theory.

## 1. INTRODUCTORY REMARKS

The history* of isospin begins precisely in the year 1932 when Heisenberg brought the concept into being. The name isospin, however, is of later date. Heisenberg used the name "$\rho$-spin" but when a few years later the concept was given a new lease of life after a period during which it had nearly faded out of physics altogether, it re-emerged under the unhappily chosen name "isotopic spin". What is more, where Heisenberg had used the symbol $\rho$ in his work, the same formalism was reborn with $-\rho$ replaced by $\tau$. There followed an attempt to replace the word "isotopic" by "isobaric", which seemed a much more appropriate term at the time, but the result was a period of unhappy co-existence of the two terms, until somebody cut the Gordian knot and began talking about "isospin". This is the name that I shall use throughout, together with the now universally accepted symbol $\tau$.

In 1932 I was just beginning to learn about quantum mechanics, at Zürich, so that it could be said that I grew up with isospin, but before I begin my main story, let me look back briefly to things that were happening about 10 years earlier.

Before 1925 the theory of atomic structure relied on the Bohr-Sommerfeld model of electron orbits, each such orbit requiring three quantum numbers to fix its size, shape and orientation. Then Pauli showed that for a complete characterisation of electronic levels a fourth quantum number was needed - one that could only have two different values. In physical language this implied that each electron in an atom seemed to possess a dynamical characteristic not related to its orbital motion. Whatever it was, the two-valued nature of the new quantity was very foreign to classical thinking. Pauli himself was not prepared to give any physical interpretation to the new variable but others - Kronig in private discussion and then Goudsmit and Uhlenbeck suggested that it related to a spinning motion of the electron.

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* An excellent account of the earliest part of this history is given by G. Rasche[(1)]. A more general account of early developments of pion theory, including the introduction of isospin, is given by V. Mukherji[(2)].

When in 1925 Quantum Mechanics came into being, it became clear that in the new mechanics, the electron did indeed possess a spin capable of just two values. As Pauli himself showed, the description of electron spin required the use of $2 \times 2$ matrices normally given as

$$\sigma_x = \begin{pmatrix} 0 & 1 \\ 1 & 0 \end{pmatrix}, \quad \sigma_y = \begin{pmatrix} 0 & -i \\ i & 0 \end{pmatrix}, \quad \sigma_z = \begin{pmatrix} 1 & 0 \\ 0 & -1 \end{pmatrix}, \quad 1 = \begin{pmatrix} 1 & 0 \\ 0 & 1 \end{pmatrix} \tag{1.1}$$

To anyone trained in quantum mechanics these expressions are certainly very familiar; I need not, and in the framework of this talk could not, explain their significance, but I want to draw attention to the entirely unsymmetrical appearance they present and to stress that despite this appearance, they form part of the most beautiful <u>and</u> <u>intrinsically</u> invariant, description of quantal angular momentum, which expresses the fact that in the real world all orientations in 3-dimensional space are equivalent. Precisely this symmetry ensures the existence of the <u>conservation law of angular momentum</u>. This is only one instance of a central fact of dynamics in general and quantum dynamics in particular: mathematical invariance laws generate physical conservation laws.

However, if we look at Pauli's matrices from a purely formal, mathematical point of view, they can be useful tools in many contexts which bear no relation to rotations in space or angular momentum. This is because in general $2 \times 2$ matrices are useful in any situation in which just two possibilities are contemplated and any such matrix can be expressed as a linear combination of matrices of the same form as the Pauli matrices. Let us put

$$\tau_1 = \begin{pmatrix} 0 & 1 \\ 1 & 0 \end{pmatrix}, \quad \tau_2 = \begin{pmatrix} 0 & -i \\ i & 0 \end{pmatrix}, \quad \tau_3 = \begin{pmatrix} 1 & 0 \\ 0 & -1 \end{pmatrix}, \quad 1 = \begin{pmatrix} 1 & 0 \\ 0 & 1 \end{pmatrix} \tag{1.2}$$

and let us construct linear combinations thus:

$$\begin{aligned} \tfrac{1}{2}(1 + \tau_3) &= \begin{pmatrix} 1 & 0 \\ 0 & 0 \end{pmatrix}, & \tfrac{1}{2}(1 - \tau_3) &= \begin{pmatrix} 0 & 0 \\ 0 & 1 \end{pmatrix} \\ \tfrac{1}{2}(\tau_1 + i\tau_2) &= \begin{pmatrix} 0 & 1 \\ 0 & 0 \end{pmatrix}, & \tfrac{1}{2}(\tau_1 - i\tau_2) &= \begin{pmatrix} 0 & 0 \\ 1 & 0 \end{pmatrix} . \end{aligned} \tag{1.3}$$

Then $$\tfrac{1}{2}(1 + \tau_3)\binom{a}{b} = \binom{a}{0}, \qquad \tfrac{1}{2}(1 - \tau_3)\binom{a}{b} = \binom{0}{b} \tag{1.4}$$

and we see that application of these matrices to a pair of quantities "kills" one of the two - these matrices are "projection operators". Similarly,

$$\tfrac{1}{2}(\tau_1 + i\tau_2)\binom{a}{b} = \binom{b}{0}, \qquad \tfrac{1}{2}(\tau_1 - i\tau_2)\binom{a}{b} = \binom{0}{a} \tag{1.5}$$

which shows that these are "projection + exchange" operators. Let me stress again that such manipulations have nothing to do with symmetry or conservation laws.

## 2. NUCLEAR STRUCTURE WITHOUT NEUTRONS

Before Chadwick's work introduced the neutron into physics the picture of the structure of atomic nuclei was confused because of one apparently insoluble mystery. Many of the observed facts were very well understood. I shall list a few.

A. The mass of any nucleus is approximately an integral multiple $Am_p$ of a unit mass $m_p$, so $A$ can be interpreted as the number of heavy constituents. The relation is not exact because of

1) possible light constituents;
2) of Einstein's relation between mass and energy; the binding together of the constituents lowers the total energy and hence the total mass.

B. The nucleus is seen to behave as a quantum mechanical system, evidence for this being that

1. It has discrete energy levels - revealed by photon ($\gamma$-ray) emission.
2. Fragments ($\alpha$- particles) can escape by the tunnel effect.
3. Total nuclear angular momentum, magnetic moment and statistics (Bose or Fermi) are observable quantities.

C. The forces binding the nuclear constituents were unknown, but some of their properties were clearly emerging. They were very strong but only at short range. The rough proportionality of binding energy to $A$ is evidence that there is interaction only between neighbouring constituents, each particle having only a few neighbours - one says they show "saturation".

D. The charge on any nucleus is an exact multiple $Z$ of $e$, (electron charge $= -e$) and because $Z < A$, the charge on some of the heavy constituents presumed to be protons, must be compensated by something light and negatively charged. So in terms of particles known to exist one assumed that a nucleus contained $A$ protons and $A - Z$ electrons.

However, if one tries to associate these negative charges with electrons the great trouble begins.

1. Although electrons are seen to emerge from nuclei ($\beta$-emission) their small mass makes it difficult under the laws of quantum mechanics to have them restricted to the small volume of a nucleus.
2. In $\beta$-decay the electrons appear to violate all conservation laws (energy, angular momentum, statistics) except of charge.

With the neutron a new start to nuclear theory could be made.

## 3. THE NEUTRON: BIRTH OF ISOSPIN

As soon as the existence of the neutron was established, the idea that it could be regarded as a heavy constituent of nuclei, side by side with the proton, suggested itself - to Chadwick, though he only has a brief remark on this in his fundamental paper(3), to Iwanenko in a short note to Nature(4) and to Heisenberg(5) where the idea was immediately presented in form of a rather detailed theory. Heisenberg was very explicit in his recognition that the existence of the neutron did not <u>resolve</u> the main problem that had bedevilled the previous picture of nuclear structure - β-decay, however it did enable him to separate that problem from the discussion of nuclear structure in general.

The comparatively very rare event of any electron emerging from a nucleus and breaking all laws in doing so could be set aside as a separate problem to be solved later. Heisenberg proceeded to develop a model of the nucleus built up of Z protons and $N = A - Z$ neutrons, with a proposed law of interaction between these nuclear constituents. This interaction was nothing more than an <u>ad hoc</u> postulate to be accepted or rejected according to its degree of success. Thus "phenomenological" nuclear structure theory began. However, it is clear from his papers that in Heisenberg's own thinking the question of the <u>origin</u> of the postulated interaction still figured prominently - and this is what will interest us here. The form he chose for his interaction law was modelled on the mechanism of homopolar binding - in particular the binding of the hydrogen molecular ion as explained in quantum mechanics. Schematically one can put

| | | |
|---|---|---|
| $\underline{r}_1$ | $\underline{r}_2$ | |
| ⊕ | ⊖ ⊕ | A |
| ⊕ ⊖ | ⊕ | B |

Fig. 3.1.

The $H_2^+$ ion may be thought of as dissociated in the two ways A and B. In A a bare hydrogen nucleus (a proton ⊕) is at $\underline{r}_1$ and a hydrogen atom (a proton ⊕ with an electron ⊖) at $\underline{r}_2$. In B the electron is at $\underline{r}_1$. Binding arises by a quantum superposition of the two states A and B, with the electron shared in what can be described as an exchange of the electron between the two protons. This mechanism only takes effect when the undisturbed electron wave functions around $\underline{r}_1$ and around $\underline{r}_2$ has some overlap and thus the range of the force is short. Also, thanks to Pauli's exclusion principle the mechanism does not provide for binding of many hydrogen atoms to give large molecules. There is a saturation effect. Heisenberg saw these to be just the kind of features wanted in a nuclear interaction, which he pictured thus:

| | | |
|---|---|---|
| $\underline{r}_1$ | $\underline{r}_2$ | |
| ⊕ | O | A |
| O | ⊕ | B |

Fig. 3.2

In this case Heisenberg sees the electric charge (+) as the entity being exchanged - between the proton and the neutron. These he supposes to differ from one another only by the presence or absence of this charge. The exchanged charge is not the electron - and the best description Heisenberg can give without reopening the whole problem of β-decay is that it is "a spinless electron obeying Bose statistics". Even so, Heisenberg himself sees the analogy between Fig. 3.1 and Fig. 3.2 as quite close, with the neutron in (2) appearing as a bound state between the proton and this strange electron.

How does Heisenberg put this main idea into formulae? Here isospin enters. Heisenberg proposes to regard the proton and neutron as two states of the same particle. I shall immediately start calling it nucleon (though he did not). To distinguish between the two states he introduces† our variable $\tau_3$ so that $\tau_3 = +1$ describes the proton and $\tau_3 = -1$ the neutron. Then the idea illustrated in Fig. 3.2 can be translated into an equation thus:

$$\begin{aligned} V^{jk}_{pn} &= -\tfrac{1}{4}\left[(\tau_1{}^j+i\tau_2{}^j)(\tau_1{}^k-i\tau_2{}^k) + (\tau_1{}^j-i\tau_2{}^j)(\tau_i{}^k+i\tau_2{}^k)\right] J(r_{jk}) \\ &= -\tfrac{1}{2}(\tau_1{}^j\tau_1{}^k + \tau_2{}^j\tau_2{}^k)J(r_{jk}) \qquad (3.1) \\ &\qquad (r_{jk} = |\underline{r}_j - \underline{r}_k|) \end{aligned}$$

This then is the main term in his interaction, but, still following his analogy with the hydrogen molecule, this time the neutral $H_2$ - which I don't intend to discuss in more detail - he also introduces a force acting between any two neutrons.

$$V^{jk}_{nn} = -\tfrac{1}{4}(1 - \tau_3{}^j)(1 - \tau_3{}^k)K(r_{jk}) \quad . \qquad (3.2)$$

By analogy he expects this force to be attractive and short ranged. There then remains the interaction between any two protons; here Heisenberg postulates only the Coulomb repulsion

$$V^{jk}_{pp} = +\tfrac{1}{4}(1 + \tau_3{}^j)(1 + \tau_3{}^k)\,\frac{e^2}{r_{jk}} \quad . \qquad (3.3)$$

What I have written down in (3.1), (3.2) and (3.3) is just the interaction between a chosen pair of "nucleons" j and k. Heisenberg's Hamiltonian contains a sum of these terms taken over all nucleon pairs. His τ-operator notation is such that in

† This is again Heisenberg translated into modern language; in his papers our $\tau_1$, $\tau_2$ and $\tau_3$ appear as $+\rho_\xi$, $-\rho_\eta$, $-\rho_\zeta$.

(3.1) non-vanishing contributions come only from unlike nucleon pairs, in (3.2) only from neutron pairs and in (3.3) from proton pairs. It is worth remarking that for all practical purposes the only thing he achieves from the $\tau$-formulation that could not have been stated without it is just that he never needs to sum separately over (p,n), (p,p) and (n,n) pairs. To most contemporary readers of this paper and its sequels I believe the introduction of such an elaborate and somewhat unusual formalism with this apparently so limited purpose had little appeal.

I have no time to describe how far Heisenberg succeeded in reproducing features of observed nuclear structure in his papers. It turned out that the interactions (3.1), (3.2) and (3.3) did not give a fully satisfactory picture. In his third paper in the series(6) he was forced to introduce an additional term into (3.3) to give an explicit (p,n) repulsion at small distances. The details of this work, though by no means his central idea, were soon superseded by the idea of Majorana(7). This concerned the treatment of the spin of the nucleon, a quantity which, rather curiously, Heisenberg did not introduce explicitly into his discussions on the "saturation" of nuclear interactions.

Majorana modifies Heisenberg's model as follows:

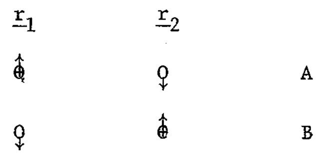


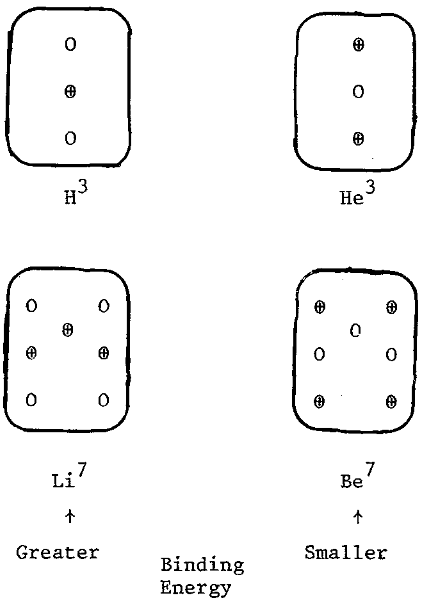

Fig. 3.3.

Looking at this picture with Heisenberg's eyes it represents exchange of charge and spin between the two nucleons (which does not mean bringing back the electron - twice the electron spin is exchanged). To Majorana's mind, however, the exchange looked like an interchange of positions by the two particles. Instead of the "inconvenient" formalism of $\tau$-matrices as Majorana puts it, the proton-neutron interaction may simply be expressed as:

$$V^{jk}_{pn} = J(r_{jk})P^{jk} , \qquad (P^{jk}\phi(\underline{r}_j, \underline{r}_k) = \phi(\underline{r}_k, \underline{r}_j)) . \tag{3.4}$$

With this assumption for the (p,n) interaction and neglecting Heisenberg's (n,n) interaction and additional (p,n) repulsion, Majorana shows that the saturation of the nuclear force for heavy nuclei and the important special feature of exceptionally strong binding of $He^4$, the $\alpha$ particle, follow simply from his equations. So work on nuclear structure over the next four years went on in the spirit of Majorana - without isospin, without much use for the concept of nucleon. Furthermore Majorana not only all but killed isospin but also moved the whole of nuclear phenomenology away from contemplation of the "field theory" underlying the forces. For in terms of any field theory the picture of whole nucleons exchanging position is certainly unacceptable!

I need not dwell on the story of this period except for one point. Finer details of nuclear structure began to make it evident that the assumption $K(r_{jk}) = 0$, i.e. <u>no</u> forces between neutron pairs needed revising, even though the effect of the (p,n) Majorana force was predominant. At the same time there was from the start very strong evidence for <u>charge symmetry</u>. This means simply that, apart from the Coulomb force, any two protons should interact with each other in exactly the same way as two neutrons do. The strongest evidence for this can be summarised diagrammatically thus:




I have deliberately chosen an absurd schematic diagram to stress that no details of nuclear structure are needed to establish charge symmetry. The point is that in such pairs of nuclei there is a full symmetry of structure. This is, of course, made imperfect by the Coulomb forces between protons. Where there are more charges the total binding is comparatively smaller. The effect of charge symmetry on Heisenberg's expressions for nuclear interaction is clearly that (3.3) must be amended.

$$V_{pp}^{jk} = \tfrac{1}{4}(1 + \tau_3{}^j)(1 + \tau_3{}^k)\left(\frac{e^2}{r_{jk}} - K(r_{jk})\right) \qquad (3.3)'$$

where $K(r_{jk})$ is the same function as in (3.4).

## 4. CHARGE INDEPENDENCE

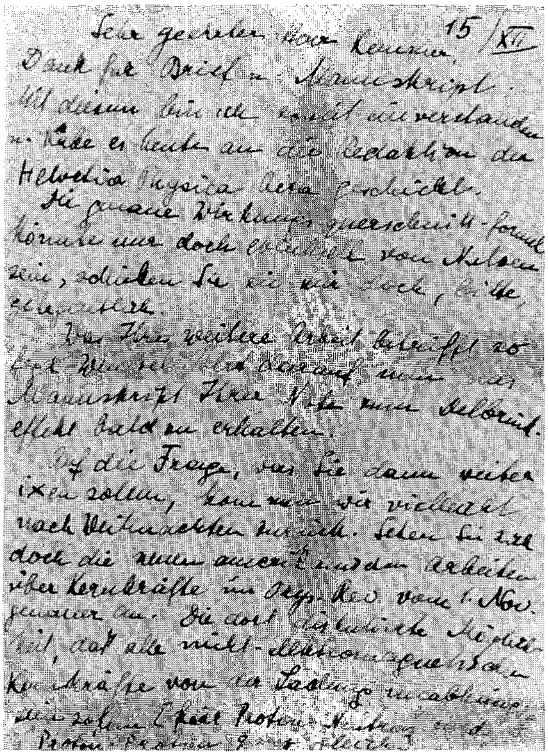
At this point my narrative must take a personal turn. In the autumn of 1936 I had just taken up a Beit Scientific Research Fellowship at Imperial College, London. I had a few pieces of unfinished work to send back to Zürich, but after that I had to look round for something new to work on. Early in December a postcard arrived from Pauli - in reply to a letter from me (cf. next page). Here is the text of a few sentences from it, translated from German.

"Have a good look at the new American papers on nuclear forces in Phys. Rev. of 1st November. The possibility discussed there that all non-electromagnetic nuclear forces are independent of charge (exactly the same for proton-neutron and proton-proton) has a certain intrinsic reasonableness ("Vernunft"). Quite possibly this might give some sensible problem to calculate."

To me the issue of Physical Review that Pauli mentions is memorable for three papers grouped together. They are by Tuve, Heydenburg and Hafstad(8), Breit, Condon and Present(9), and Cassen and Condon(10).

The first is an experimental one; following similar work by White(11) whose experimental resources had proved insufficient for complete success, these workers had measured proton-proton scattering at what were then high energies ($\sim$ 1 MeV) in order to discover deviations from what would be expected if there was only electromagnetic interaction between them. This was the way to detect the presence of the term in $K(r_{jk})$, in practice. To do any measurements on (n,n) scattering was impossible and one had to rely on charge symmetry to estimate (nn) forces. The very accurate data provided by this work were analysed in the second paper(9), with a clear-cut result in terms of parameters for the potential $K(r)$. The analysis was detailed and precise and came out with quite an unexpected conclusion: the parameters found were identical with the parameters defining the interaction of two unlike nucleons in the comparable states of the (p,n) system. This finding led at once to the "charge independent hypothesis". The meaning of comparable states is simply this. Two protons or two neutrons can only exist in states allowed by Pauli's Principle, i.e. states that are anti-symmetric for interchange of the position and spin coordinates of the two particles. The states possible for the (p,n) system are not restricted by Pauli's Principle and some are symmetric for the same interchange. The lowest state of the (p,n) system, the ground state of the deuteron is in fact one of the symmetric states and has no counterpart in the like particle system. So Breit, Condon and Present had to compare their parameters with those for the first excited state of the deuteron.

This result was from anyone's point of view a remarkable one, promising an unsuspected new regularity or symmetry within the scheme of nuclear interactions - as Pauli said on his postcard. For Cassen and Condon, the authors of the third paper, this was the signal to launch a revival of the isospin idea. Let me



Sehr geehrter Herr Kemmer, 15/XII

Dank für Brief u. Manuskript.
Mit diesem bin ich soweit einverstanden u. habe es heute an die Redaktion der Helvetica Physica Acta geschickt.
Die genaue Wirkungsquerschnitt-formel können Sie doch [illegible] von Heitler sein, schicken Sie sie mir doch, bitte, [illegible].
Was Ihre weitere Arbeit betrifft, so lege ich Wert darauf, [illegible] Manuskript Ihrer Note zum [illegible]effekt bald zu erhalten.
Auf die Frage, was Sie dann weiter ixen sollen, komme ich vielleicht nach Weihnachten zurück. Sehen Sie sich doch die neuen amerikanischen Arbeiten über Kernkräfte im Phys. Rev. vom 1. November genauer an. Die dort diskutierte Möglichkeit, daß alle nicht-elektromagnetischen Kernkräfte von der Ladung unabhängig sein sollen (d.h. Proton-Neutron und Proton-Proton gleich)

Bergmann ist [illegible], die Stelle in Rostow am Don zu bekommen!

RORSCHACH - HAFEN

WEIHNACHTSPOST BEIZEITEN AUFGEBEN
CHRISTMAS
ZÜRICH 14-15 15.XII.1936 BRIEFVERSAND
20 HELVETIA

POSTKARTE CARTE POSTALE CARTOLINA POSTALE

hat eine gewisse innere Konsequenz für sich. Es könnte wohl sein, daß sich da vernünftige Probleme zum Rechnen ergeben.
Bleiben Sie zu Weihnachten in London? Jedenfalls gute Feiertage u. frohes Neujahr. Grüße auch von Fierz.
Stets Ihr W. Pauli

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In 1935 after V. Weisskopf relinquished Pauli's "Assistent" post, Pauli offered it to me, but the authorities held that a person of such little experience who was not Swiss deserved only half of the post. I shared it with Dr. Guido Ludwig. It was a great honour to be in that position, but hardly enough to support me, so that with generous support from Pauli and Wentzel I succeeded on my application for a "Beit Scientific Research Fellowship" at Imperial College London. There I was entirely isolated from people of like interests. I first completed some pieces of work which are referred to on the postcard, but then had to think what to do next. Evidently I had posed that question in a previous letter to Pauli. The reply is on the postcard. It reads :

Auf die Frage, was sie dann weiter ixen sollen kommen wir vielleicht nach Weihnachten zurück. Sehen sie sich doch die neuen amerikanischen Arbeiten über Kernkräfte im Phys. Rev. vom 1. Nov genauer an. Die dort diskutierte Möglichkeit da alle nicht elektromagnetischen Kernkräfte von der Ladung unabhängig sein sollen (für Proton-Neutron und Proton-Proton ganz gleich) hat eine gewisse innere Vernunft in sich. Es könnte wohl sein, dass sich da vernünftige Probleme zum Rechnen ergeben.

(Editor's note : Ixen equals long computations with x's).

The first sentence refers to what I must have told him in my letter about re-visiting Zürich at Christmas. The card evidently was re-directed to me from Imperial College, to my private address in London and back to Switzerland. So it will have reached me about a month after my 25th birthday.
When Pauli died, I sent what I thought was my entire correspondance with Pauli to be copied for the Pauli archives. This postcard was not included. I had not kept it with the other letters. But latter on 1961, around the time of my 50th birthday I happened to be looking at other old papers - and there it was - It was good to be reminded, 25 years after the event, what I owed to Pauli for directing me towards thinking about isospin. I had forgotten it !

illustrate what they did first in terms of Heisenberg's original and by then virtually forgotten formalism. Let us look back on his equations (3.1), (3.2) and (3.3) in its amended form (3.3)', but omitting the Coulomb term. To incorporate the newly found result we simply have to put:

$$K(r_{jk}) = J(r_{jk}) \tag{4.1}$$

and then find

$$V_{NN}^{jk} = -(1 + \tau_1{}^j\tau_1{}^k + \tau_2{}^j\tau_2{}^k + \tau_3{}^j\tau_3{}^k)J(r_{jk})$$

or in a new notation

$$= -\tfrac{1}{2}(1 + \underline{\tau}^j\cdot\underline{\tau}^k)J(r_{jk}) \; . \tag{4.2}$$

Here I have used the familiar language of vector algebra. This new notation is an entirely formal step. The "scalar product" looks invariant, but there is as little physical meaning to be associated with any imagined change of coordinate frame as there was before. However, since the outward form of invariance has emerged, all the mathematical consequences of this form can be exploited. They are all well known from the theory of real spin: given two particles 1 and 2 of spin $\frac{1}{2}$. their total spin is

$$\underline{S} = \frac{1}{2}(\underline{\sigma}^j + \underline{\sigma}^k) \qquad \text{and we know that}$$

$$(\underline{S})^2 = \frac{3}{2} + \frac{1}{2}\,\underline{\sigma}^j\cdot\underline{\sigma}^k = S(S+1) = \begin{cases} 2 & \text{Triplet} \\ 0 & \text{Singlet} \end{cases} \tag{4.3}$$

Therefore $\frac{1}{2}(1 + \underline{\sigma}^j\cdot\underline{\sigma}^k) = \begin{cases} 1 & \text{Triplet} \\ -1 & \text{Singlet} \end{cases}$ .

The $(S = 1)$ triplet of states is $\frac{1}{\sqrt{2}}(\uparrow\downarrow + \downarrow\uparrow)$

(4.4)

and the singlet is the state $\frac{1}{\sqrt{2}}(\uparrow\downarrow - \downarrow\uparrow)$

We note that the triplet states are symmetric for the interchange of spins and the singlet anti-symmetric. Consequently the operator

$$S^{jk} = \frac{1}{2}(1 + \underline{\sigma}^j\cdot\underline{\sigma}^k) \tag{4.5}$$

can be interpreted as the operator of spin exchange.

In exact analogy we can now interpret the operator in (4.2)

$$T^{jk} = \tfrac{1}{2}(1 + \underline{\tau}^j \cdot \underline{\tau}^k) \tag{4.6}$$

as effecting exchange of $\tau_3$ value - or <u>charge</u> - between the two nucleons j and k. So we can put

$$V_{NN}^{jk} = V_H^{jk} = - T^{jk} J(r_{jk}) \quad . \tag{4.2'}$$

This is the Heisenberg form of force adapted to charge independence but it is obvious that the additional spin dependence by which Majorana's interaction differs from Heisenberg's can be incorporated very simply:

$$V_M^{jk} = - S^{jk} T^{jk} K(r_{jk}) \quad . \tag{4.7}$$

There is an important point to note here. When the Majorana force is expressed as above it is clearly implied that the state functions of the nucleon system on which operators like (4.6) act are functions of five variables, the three positions coordinate a two valued spin coordinate and a similar two valued isospin (or charge) coordinate. This was in fact implied by Heisenberg in his first paper but was not entirely explicit. Majorana's and practically all subsequent work dispensed with the fifth coordinate. Now it is not only back but back in a more fundamental way. We know now that the three isospin states

$$p\ p, \quad \frac{1}{\sqrt{2}}(pn + np), \quad n\ n$$

of a 2 nucleon system belong to the eigenvalue +1 of the isospin exchange operator and the state

$$\frac{1}{\sqrt{2}}(pn - np)$$

to the eigenvalue -1. The experimental facts of charge independence were that the same interaction is felt by all pairs of nucleons which are in states antisymmetric in position and spin. If an expression like (4.7) is used for their interaction we are bound to associate the symmetric isospin function with the "comparable" states and then the antisymmetric function must belong to the "non comparable" states. So in terms of the five variables, all nucleon states are anti-symmetric in these variables. This reformulation does not introduce any new physics, but is essential to make the whole scheme consistent. (What I have said for two particles works out in just the same way for any number of nucleons)†.

†Making this extension to a 5 variable description would formally suggest introduction of isospin states which mix protons and neutrons. But whatever the formalism, one remains constrained by the "super-selection rule" for charge.

Note that in this formulation it is no longer useful to think of non-symmetrical proton-neutron states, as, for instance, Majorana's equation (4.7) implies. Whether you have an exchange operator or not in your interaction, any matrix element between symmetrised states is bound to have exchange terms and non-exchange terms. As Cassen and Condon point out, one has to consider on an equal footing the four types of interaction

$$V_M^{jk} = -S^{jk}T^{jk}\,J(r_{jk}) \qquad V_H^{jk} = -T^{jk}\,J(r_{jk})$$
$$V_B^{jk} = S^{jk}\,J(r_{jk}) \qquad V_W^{jk} = J(r_{jk}) \tag{4.8}$$

The suffix W indicates that the name for the "ordinary" force used by Cassen and Condon is "Wigner force" in recognition of Wigner's fundamental studies on the properties of the deuteron, n,p scattering, etc. in terms of just such a potential. The suffix B relates to the name "Bartlett force" given to the previously unmentioned fourth possibility of spin exchange only, which was first introduced explicitly by Bartlett(12). All four types of force have the same claim to be considered as parts of a general nuclear interaction, in particular if one wants to ensure that the resultant force saturates. This must come about by cancellation of non saturating terms in the combined interactions.

Note also that the "generalised Pauli Principle" that we mentioned above can be given expression in the form

$$P^{jk}\,S^{jk}\,T^{jk} = -1 \tag{4.9}$$

which shows incidentally that the form of $V_M^{jk}$ given in (4.7) is equivalent to Majorana's own (3.4). The signs in the four expressions of (4.7) are conventionally chosen as given, simply to ensure that in the deuteron ground state ($P^{jk} = +1$, $S^{jk} = +1$, $T^{jk} = -1$) all four expressions equal $+J$.

Also incidentally, it is nothing but an act of faith on the part of Cassen and Condon, to assume in all the different terms only a single function of distance $J(\tau)$ should be involved - a faith in the simplicity of the unknown agency producing the interaction!

Of the three papers that Pauli suggested I should study, I, being easily attracted to questions of formal structure, devoted most of my study to the third with its idea of isospin. The way I first made use of the idea is in the context of saturation conditions. Various authors began fitting the Charge Independence Hypothesis into nuclear structure investigation, and in particular I read a paper by Volz(13) investigating the status of saturation conditions in the new situation†.

† It is a matter of historical fact that I got involved in this work through reading Volz. There was extensive discussion of the same subject in the United States. See ref. (13a).

He pointed out among other facts that it was not sufficient to satisfy oneself that nuclear interactions given by some combination of the four types of force (2.7) produced the required saturation for nuclei with realistic values of Z and N = A - Z. It is also essential to ensure that nuclei not found in nature are not predicted to be more tightly bound, i.e. do not experience non saturating interactions. Volz gave inequalities to be satisfied by the strengths of the four kinds of force to ensure no stability for tightly bound clusters of neutrons, Z = 0. My first and only contribution to nuclear structure theory was simply to point out in a note to Nature(14) that a similar inequality was needed to ensure non stability for "ferromagnetic" nuclei (S = A). I did not have to do any calculations - my inequality was simply Volz' inequality with the roles of $S^{jk}$ and $T^{jk}$ exchanged.

For me this was an excellent first experience in thinking in isospin language. For the development of the isospin idea I do not think it meant a great deal - not many years later, particularly after the deuteron quadrupole moment was observed the hope that one could explain the droplet structure of nuclei by assuming only saturating central forces faded away - in this respect nature proved to be not so very simple (see Volkoff(15)).

In a rather different direction the really great advance in nuclear structure physics, based on the isospin idea with charge independence, was soon on the way, with Wigner(16) as its leading figure. He not only led the advance but also, later, produced an excellent summary of the whole field(16a) which enables me to be very brief about it in my present account. Central to this work is the recognition of the importance of the total isospin of a nucleus - or, in later developments - of interacting pairs or indeed groups of nuclei.

$$\underline{T} = \tfrac{1}{2} \sum_j \underline{\tau}^j \quad . \tag{4.10}$$

Like everything else about isospin this construct still had to be seen as a formal thing. Its third component had a very concrete physical meaning; for one has

$$Z = \tfrac{1}{2} \sum_j (1 + \tau_3{}^j) = \tfrac{1}{2}A + T_3 \tag{4.11}$$

and one needs no proof to see that since both Z and A are constants, so is $T_3$. But one can also "verify" that quantum mechanics permits $T_3$ to be constant because the operator in (4.9) commutes with any Hamiltonian, which involves only the nuclear interaction expressions of (4.7). But then it becomes clear that, provided electromagnetic interaction and the proton-neutron mass difference may be neglected because of their relative smallness, the other components of $\underline{T}$, i.e.

$$T_1, T_2 \quad \text{or} \quad T_1 \pm iT_2 \tag{4.12}$$

are also approximately constant and so is

$$\underline{T}^2 = T_1^{\,2} + T_2^{\,2} + T_3^{\,2} . \tag{4.13}$$

Exactly as for real spin, $T_3$ and $T^2$ can be used as quantum numbers. Then one has for their eigenvalues

$$\underline{T}^2 = T(T+1) \qquad \underbrace{T_3 = T,\ T-1,\ \ldots\ldots -T}_{2T+1 \text{ values}} \tag{4.14}$$

Also, the operators $T_1 \pm iT_2$ effect transitions from any state with $T_3 = T'$ to the states with $T_3 = T' \pm 1$. Such transitions are precisely what one has to deal with in certain β-decay processes. Thus, although the formulae are <u>exactly</u> valid only in the idealised world in which electromagnetism does not exist, the classification of nuclear levels, the characterisation of 'allowed' and 'forbidden' nuclear reactions and β-transitions is greatly refined by the use of isospin ideas.

It should be noted in this connexion that an isospin multiplet of levels is a set of levels, not in one nucleus but in a set of isobaric neighbours. An example is

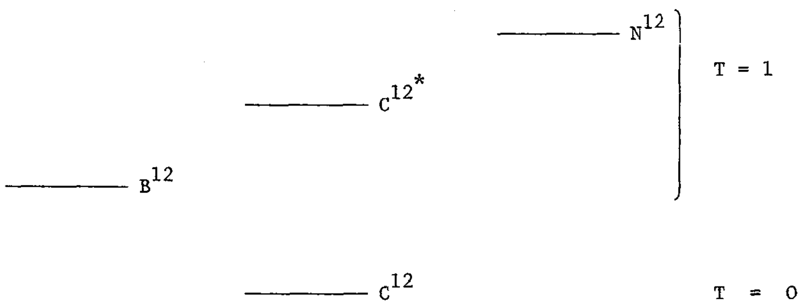


Fig. 4.1

The singlet level exists only in C and is the lowest state, the lowest states of B and N, together with the excited state of C form an iso-triplet. The difference of energy within the triplet is the effect of Coulomb repulsion, which must increase with Z or $T_3$.

## 5. <u>FIELD THEORIES OF NUCLEAR INTERACTION</u>

We must now go back for a few years, to discuss the development of ideas on the nature of the agent that produces nucleon-nucleon interactions. We saw that Heisenberg could do no better than speak of an imagined 'spinless electron', obeying

Bose Statistics. The only description of the action of such a thing available to him could be illustrated thus:

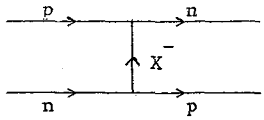


Fig. 5.1

(It must be realised that this schematic representation is not a Feynman diagram. At the time field theory was in such a rudimentary state that all the quantitative implications that such a picture carries nowadays were completely absent at the time.) Note that the "electron" is seen to be emitted from the neutron and taken up by the proton. When the positron was discovered in 1932, only months after Heisenberg's first paper, the picture could be slightly changed. Now the arrow on the diagram could go, since emission and absorption could go both ways

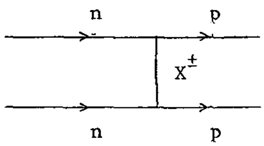


Fig. 5.2

So one moved a step away from Heisenberg's close analogy with $H_2^+$ which carried at least an implication that the neutron was the bound state of p and $X^-$ and perhaps not elementary.

Then within 2 years a hint as to the nature of the X became available. The description of β-decay was put on a new firm footing by Fermi[17] with the aid of Pauli's hypothetical neutrino. Fermi's theory resolved all the problems of broken conservation laws in β-decay and of how to confine an electron in a nucleus. The electron and neutrino were, he postulated, created in the elementary act of β-decay. Was this then just what was needed for nuclear interaction?

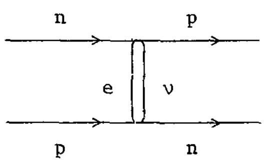


Fig. 5.3

Qualitatively this suggestion, which was first made in print simultaneously (but independently) by Tamm[18] and by Iwanenko[19] appeared to fit the needs of the problem very well, but it was seen very quickly that as an agent for transmission of nuclear interaction the Fermi field is entirely inadequate. Even though at that time no consistent field theories existed, what rough estimations of resulting nuclear interaction could be made showed that both the magnitude and the radial dependence of such an interaction were completely unlike what was required of them.

Even so there was one question that could usefully be studied in terms of the Fermi field. Could whatever nuclear interactions followed from the theory be made charge independent? Both Gamow and Teller[20] and Wentzel[21] had remarked that to have p-p and n-n forces additional Fermi-like interactions with zero charge transfer would be needed†.

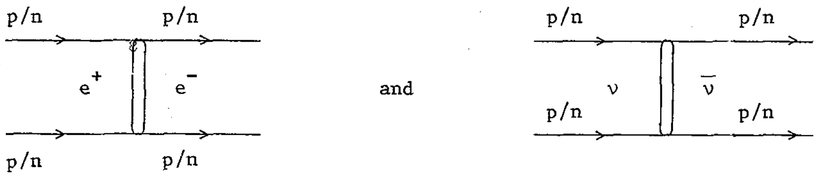


Fig. 5.4

My dear former teacher and friend, the late Gregor Wentzel, actually stated that the addition of such interactions could not produce charge independence of nuclear forces. Gregor was one of the majority of theorists of the period who still didn't think much of the isospin formalism, but I managed to convince him that he had slipped up. Let me indicate briefly the essence of my procedure[22]. The schematic diagram for β-decay can be drawn thus:

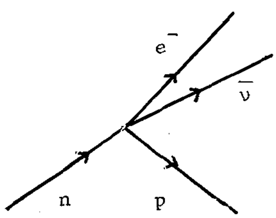


Fig. 5.5

and by the rules of the game the "canonical" form of this interaction is

† Gamow and Teller[20] also introduced the same idea but not in the context of charge independence.

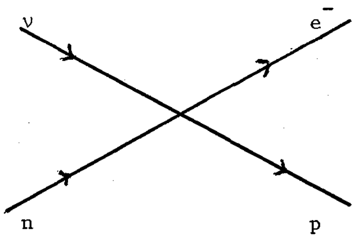


Fig. 5.6

The algebraic equivalent of that diagram is the interaction term†

$$\overline{\psi}_p \; \mathrm{Op} \; \psi_n \cdot \overline{\psi}_{e^-} \; \mathrm{Op} \; \psi_\nu \quad . \tag{5.1}$$

The charge independent extension of this is

$$\overline{\psi}_N \; \mathrm{Op} \; \underline{\tau} \; \psi_N \cdot \overline{\psi}_L \; \mathrm{Op} \; \tau \; \psi_L \tag{5.2}$$

where $\psi_N = \begin{pmatrix} \psi_p \\ \psi_n \end{pmatrix}$ and $\psi_L = \begin{pmatrix} \psi_{e^-} \\ \psi_\nu \end{pmatrix}$

or diagrammatically,

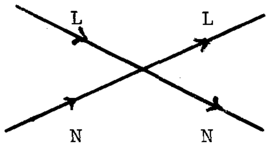


Fig. 5.7.

Such an interaction automatically ensures charge independence of the nuclear interaction. But this was an entirely academic exercise; rightly it attracted little interest. Using modern language, it may be worth noting that this was probably the first appearance of "neutral currents", (in papers (20) and (21) as well as mine.)

## 6. CHARGE INDEPENDENT PION THEORY

While these ideas were going around very little notice was taken of the fact that as early as 1935 the seed of a highly effective new approach had already been sown. In that year Yukawa[23] had in effect revived Heisenberg's "spinless electron

† For simplicity the entirely parallel picture for $e^+$ emission is suppressed.

that obeyed Bose statistics". The main novelty of his idea was to postulate that this particle should be rather heavy - about 200 times as heavy as the electron. It was to be created singly and absorbed just as lepton pairs were in β-decay and it was to be governed by the field equation for charged bosons as developed by Pauli and Weisskopf(24) just a year before Yukawa's work appeared*. To visualise the Yukawa idea we can use Fig. 5.2, the $X^{\pm}$ simply becoming $\pi^{\pm}$. He could show that it would provide a (p,n) interaction with

$$J(r_{jk}) = C.e^{-\kappa r}/r \tag{6.1}$$

where $\kappa$ is proportional to the particle's mass: $\kappa = \frac{mc}{\hbar}$ .

The elementary process analogous to Fig. 5.5 for β-decay is in Yukawa's Theory evidently†

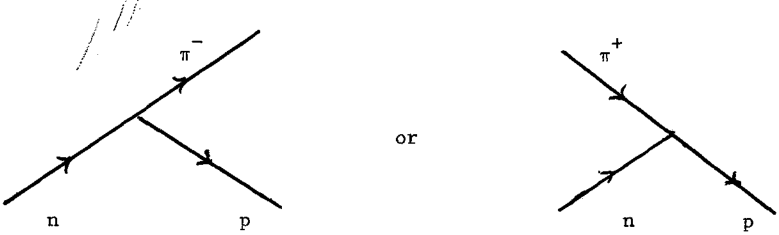


Fig. 6.1

which has the algebraic equivalent

$$\bar{\psi}_p \;\cdots\cdots\; \psi_n \;\cdots\cdots\; \phi \;\cdots\cdots \tag{6.2}$$

where the rows of dots refer to various spin operators, derivatives and vector suffixes that appear in various versions of the theory.

Partly because this work was published in a little read Japanese journal (albeit in English), partly because entirely hypothetical particles were not popular, the work did not receive the attention it deserved. In 1937, however, a charged particle of comparable mass was discovered in cosmic radiation by Neddermayer and Anderson(25). Immediately Yukawa's theory began to be studied extensively. Variants of the original theory were developed (equation (6.2) is meant to include these). A spin 1 model of the pion became fashionable and a lot of effort was put into working out predictions of this theory and comparing this with

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* In Yukawa's first paper he sees his $\psi$ as the equivalent of the scalar potential of electromagnetism and not as a Pauli - Weisskopf scalar. However his paper was later read as implying spin 0 for his particle. There is an overall sign difference between the two interpretations.

† We again suppress mention of $\pi^+$ emission.

experiment. Since by then like particle forces were well established nearly everyone working in the field was prepared to say that to obtain like particle interactions, one would need to postulate neutral pions, in addition to Yukawa's charged ones. In my case I could go further with little effort because, having learnt what was needed in my study of the charge independent Tamm-Iwanenko model, the entire scheme was very easy to write down. I postulated an interaction[26] between the nucleon doublet and an isovector triplet of pions:

$$\underline{\phi} \quad = \quad (\phi_1, \phi_2, \phi_3). \tag{6.3}$$

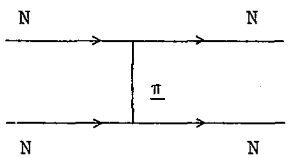


Fig. 6.2

defined in terms of Yukawa's $\phi$ and a real $\phi_o$ by

| Charged pion | Neutral pion |
|---|---|
| $\phi = \frac{1}{\sqrt{2}} (\phi_1 + i\phi_2)$ | |
| | $\phi_o = \phi_3$ (6.4) |
| $\phi^+ = \frac{1}{\sqrt{2}} (\phi_1 - i\phi_2)$ | |

In formulae this gave the interaction Hamiltonian:

$$H_{int} = \overline{\psi}_N \cdots \underline{\tau}\psi_N \bullet \cdots \underline{\phi} \cdots \tag{6.5}$$

The rows of dots again stand for a multitude of variants. The simplest interaction is given by the scalar meson theory

$$\overline{\psi}_N \, \underline{\tau} \, \psi_N \bullet \underline{\phi} \quad , \tag{6.6}$$

while (for future reference) in vector meson theory one had

$$\overline{\psi}_N \, \gamma_\mu \, \underline{\tau} \, \psi_N \bullet \underline{\phi}_\mu \quad . \tag{6.7}$$

From these expressions, it was but a small step to derive charge independent N-N interaction. However it should not be forgotten that "derivation", whether in Yukawa's theory or in this charge independent extension, meant in those days making a calculation that could be trusted in first non-vanishing order of perturbation theory, but which, if taken any further became entangled with infinities which nobody had as yet learned to deal with by renormalization. Moreover, it was soon seen that in all Yukawa type theories - unlike electrodynamics - the expansion parameter was not a small quantity. Methods other than perturbation theory had not then been thought of - in fact the whole theory was being developed on a very unsure footing.

Knowing all this very well, I nevertheless included in my paper - and as I remember it, simply as an afterthought - some remarks to the effect that because the proposed interaction term in the Hamiltonian was in charge-independent form, the same mechanisms of calculation which were found to lead to invariant results for real spin would produce charge-independent - i.e. formally invariant results - in terms of isospin in any future theory. I indicated explicitly what the charge-independent form of the next order ($g^4$) N-N potential would be.

At the time the paper was received, as intended, as a small annexe to the other paper on Yukawa's Theory that Fröhlich, Heitler and I had produced in close parallel with the work by Yukawa and his collaborators and of Bhabha. The name "symmetrical theory" was first used for my formalism by Bethe(27) in a paper in which he preferred a "neutral" theory (based on a $\pi^o$ only) to account for nuclear interactions.

If I had had any faith or encouragement to see more in that work than a formalism - perhaps a pointer towards a deeper design, it would have been quite possible to derive then a good deal of what later became standard knowledge about isospin.

At this point war intervened and after it was over I did not personally pick up the threads of the development. Soon Lattes, Powell, Occhialini and Muirhead(28) demonstrated that a charged pion did exist in nature and, not long after, its neutral partner $\pi^o$ was also found to exist (Bjorklund, Crandall, Moyer and York(29); Sternberger, Panofsky and Steller(30)). In due course it became clear that the pseudoscalar, "symmetric" version of Yukawa's theory gave a good account of many experimental findings. In parallel with this, field theories were placed on a much firmer footing with the introduction of the ideas and techniques of renormalisation. Fortunately the pseudoscalar pion theory proved to be renormalizable - though far less tractable than electrodynamics because of the strength of $N - \pi$ coupling.

Very many authors in introducing new developments quoted my paper and by implication attributed concepts to me which I had not in fact thought out. I have found it difficult to discover who was first to have said some of the things I might have said but did not. Let me summarise some of these results.

In terms of the state function $\phi$ for the charged pions, the charge current vector has the form

$$i(\phi^+ \frac{\partial}{\partial x_\mu} \phi - \phi \frac{\partial}{\partial x_\mu} \phi^+) \tag{6.8}$$

In terms of the isovector components this becomes

$$-(\frac{\partial}{\partial x_\mu} \phi_1)\phi_2 + (\frac{\partial}{\partial x_1} \phi_2)\phi_1 \ . \tag{6.9}$$

These charged pions interact with protons whose charge current vector is

$$i\bar{\psi}\gamma_\mu \tfrac{1}{2}(1 + \tau_3)\psi = \tfrac{1}{2}\bar{\psi}\gamma_\mu\psi + \tfrac{1}{2}\bar{\psi}\gamma_\mu \tau_3 \psi \ . \tag{6.10}$$

Thus the total charge current density vector can be represented as

$$J_\mu = J_\mu^{(S)} + (\underline{J}_\mu)_3 \tag{6.11}$$

$$J_\mu^{(S)} = \frac{i}{2}\bar{\psi}\gamma_\mu\psi \quad \text{and} \quad (\underline{J}_\mu)_3 = (\frac{i}{2}\bar{\psi}\gamma_\mu \underline{\tau}\psi - \frac{\partial}{\partial x_\mu}\underline{\phi} \times \underline{\phi})_3 \tag{6.12}$$

The notation for the last expression implies that it is a third component in isospin space.

It is not difficult to see that each of these three charge-current quantities, the sum and its two parts separately lead to conserved quantities. The flux integral of the left hand side is naturally the total charge $Q$, while $\int J_\mu^{(S)} d\sigma_\mu = \frac{1}{2}B$ where $B = N - \bar{N}$ is the baryon (difference between nucleon and anti-nucleon) number and $\int (\underline{J}_\mu)_3 \, d\sigma_\mu = \underline{T}_3$ is the third component of total isospin. So we have

$$Q = \tfrac{1}{2}B + T_3 \ . \tag{6.13}$$

(If we replace $B$ by $A$ - which is what $B$ reduces to in nuclear physics and similarly $Q$ by $Z$, we regain equation (4.11)). Thus we see that for <u>pions</u> charge and third component of $\underline{T}$ are the same thing. I should note in passing that in my paper it was not even mentioned that pions have unit isospin, although either discussion of the most elementary $N$-$\pi$ interactions or, alternatively, one look at (6.9), shows that this is the case!

Thus we have the three connected conservation laws of charge, of baryon number and of $\underline{T}_3$ which are exact within the nucleon-pion system; but it is possible to conclude further just as Wigner did for nuclear structure, that in the absence of electromagnetism or any other interactions that lack the formal rotational symmetry of (6.5) the other two components of total isospin, i.e. $T_1$ and $T_2$ or alternatively $T_1 \pm iT_2$ and also $(\underline{T})^2$ are constant. In the real world they will be approximate constants.

Therefore just as Wigner and others following him found a real physical significance for the full isospin picture in their study of nuclei, so now the full isospin picture began to be useable in the study of particle interactions with $T(T + 1)$ and $T_3$ defining states and $T_1 \pm iT_2$ effecting transitions between them.

The first paper I am aware of, which applies the idea of isospin conservation to experiment ($N - \pi$ scattering) is by Heitler [34].

## 7. FURTHER DEVELOPMENTS. SELECTED TOPICS

In the time available let me first select a particular group of ideas that illustrates both the general fact that in post-war physics isospin became an essential part of the general picture and at the same time describes the remarkable way it was found to interconnect with the theory of weak interactions, for which of course isospin has no direct relevance. In post-war physics "weak interaction" had come to mean something much wider than just the Fermi interaction of β-decay. In this widened scheme the following processes are included:

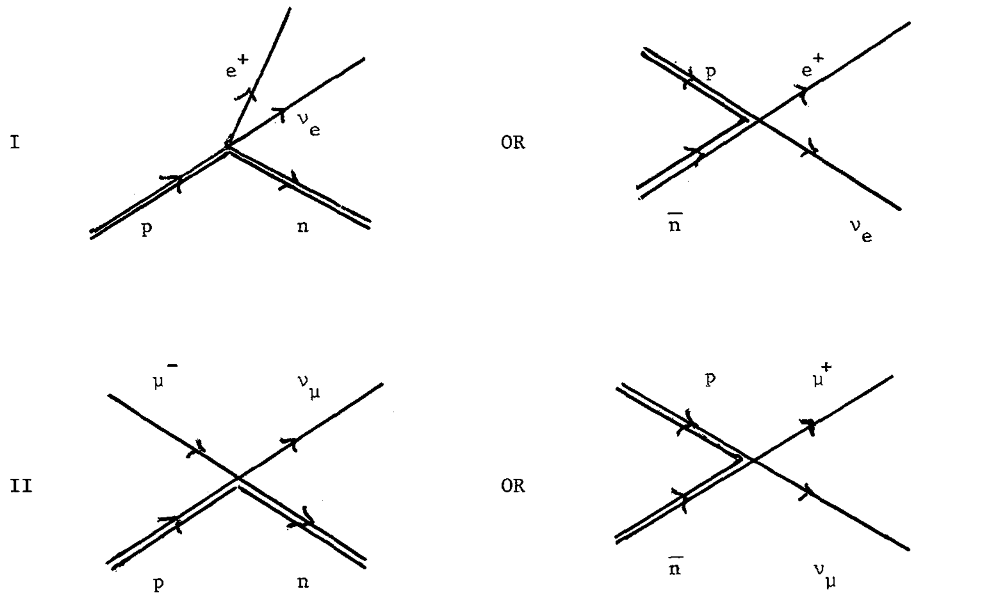


Fig. 7.1

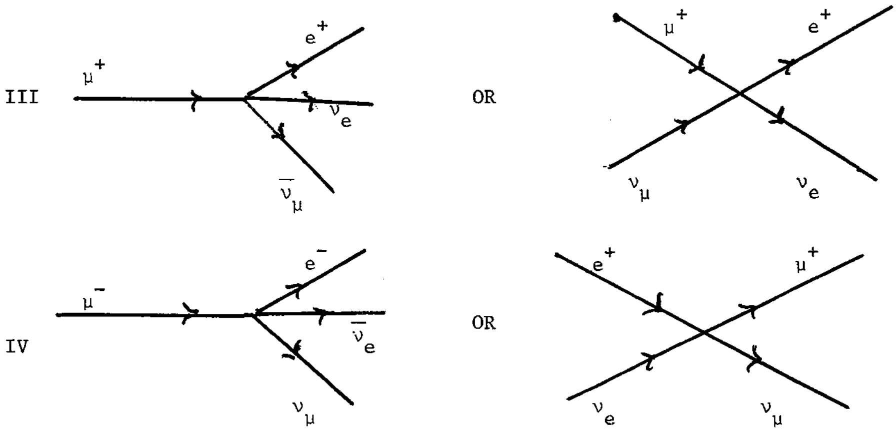


Fig. 7.1

The left-hand diagrams indicate the observed physical processes, the right-hand sides display a "canonical" form in which the same interactions can be stated. In this form the close similarity between the structures of these processes becomes very evident and suggests that they should all be of the same strength. This is the hypothesis of the Universal Fermi Interaction. However a trained eye sees that these diagrams are not equivalent and that in general one ought to be surprised if all the coupling constants were measured to be the same. The difference is indicated by the thickness of the baryon lines as distinct from the lepton lines. This is meant to remind you that for the interaction involving baryons, what is drawn as a simple elementary process is in reality an interaction of the leptons with a complicated system - the baryon carrying all its strong interactions, at least with the pions - maybe with other things as well. It is not obvious that the leptons interacting with this system should have the same magnitude of coupling as when they interact with each other, as in III.

But why is it then that the electric charge - the measure of a particle's interaction with photons is the same for a proton and an electron? The reason that this is so was recognised early in renormalisation theory and can be demonstrated diagrammatically thus:

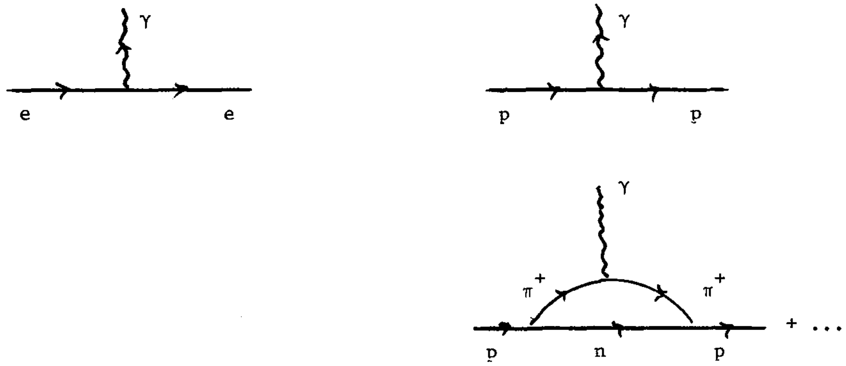


Fig. 7.2

We know that the coupling of the electromagnetic field with the proton + pion system is such that the electric charge current vector satisfies a conservation equation, i.e. that e is conserved. In simpler language we may say that even while the proton is not a proton but a neutron + pion system the electromagnetic field remains coupled with the system with unchanging strength. If, however, there were no laws of charge conservation the interaction strength of electrons and of protons in the photon would not necessarily be the same.

The Russian physicists Gershtein and Zel'dovich(31) studied weak interactions in 1955 and remarked on this very point at a time when the precise form of weak interaction Hamiltonians had not been correctly established. They noted the unfortunate absence of a similar conservation law which might have applied there:

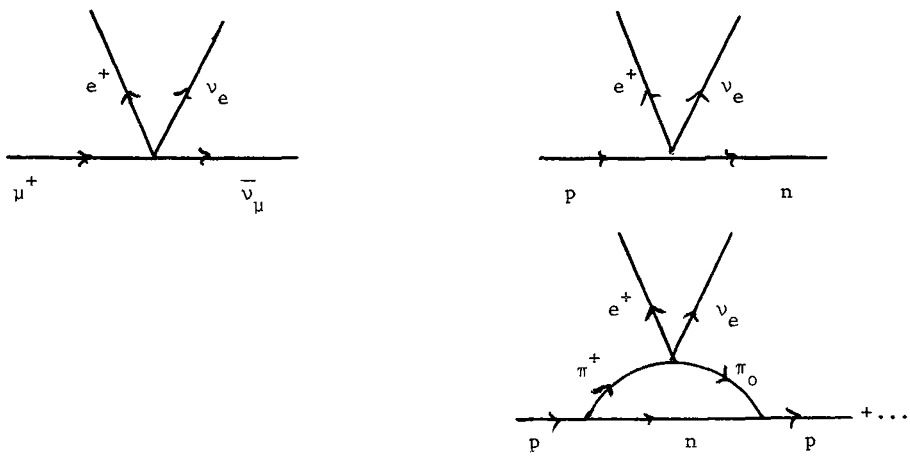


Fig. 7.3

Thus they concluded that the β-decay coupling constant would require renormalization in the real world.

Within a year the theory of weak interactions underwent drastic revision as a result of the discovery of parity violation. The new form of interaction was found to take on a standard "universal" shape which split into two parts, the "V" and "A" parts and one of these two parts, the "V" part, involved precisely the charge-current term const. $\bar{\psi}_N\gamma_\mu(\tau_1 \pm i\tau_2)\psi_N$ as its baryon factor. Using the earlier result (6.12) we see that this expression, together with a pion term ensures the conservation in the "V" part of $T_1 \pm iT_2$ (in the ideal world of no electromagnetism). So if those β-decays that are governed by the "V" interaction are to have the same coupling constant as corresponding μ-decays (and μ-capture processes), it is essential to postulate the existence of the decay

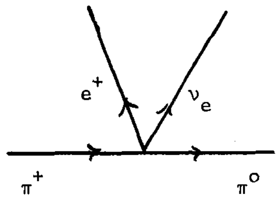


Fig. 7.4

at a precisely prescribed rate. Since experiment showed that with considerable precision the "V" parts of all weak interactions had a universal strength of coupling, the case for isospin conservation gained much in strength. These results were published by Feynman and Gell-Mann(32) with acknowledgements to Gershtein and Zel'dovich. The universality of coupled constants was not the only test of this idea - the process of Fig. 7.2 was something to look for and in addition Gell-Mann(33) suggested a most ingenious means of confirming the conserved vector current still further. We go back to Fig. 4.1 slightly amplified. The very set of levels displayed there has special properties that permit a most interesting test:

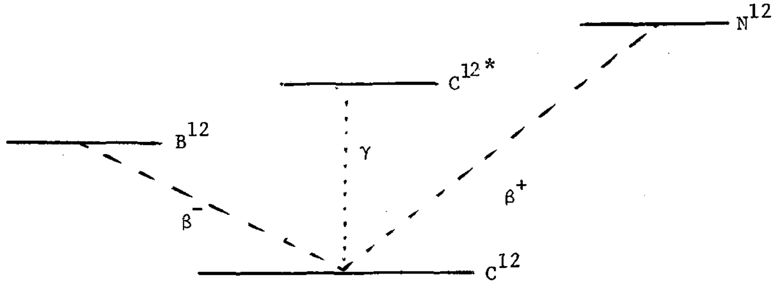


Fig. 7.5

The $\gamma$ decay of $C^{12^*}$, since it goes from a $T = 1$ to a $T = 0$ state, does not involve $J^{(S)}$, only the third iso-component of $\underset{\sim}{J}_{\mu}$. In that case the coupling of the electromagnetic field in the $\gamma$-transition is related to $T_3$ in just the same way as parts of the $\beta$ transitions are related to $T_1 \pm iT_2$ and a precise quantitative relation can be stated between the transition rates of these processes. Because the $\gamma$ transition is of the magnetic dipole type Gell-Mann[(33)] calls the effect producing the $\beta$ transitions "weak magnetism".

## 8. THE YANG-MILLS AND SHAW THEORY

We have seen from these examples that by the 1950's isospin had penetrated deeply into current physical thinking. Nevertheless, it still seemed to stand outside "real physics" in two, not unconnected ways. The first, obviously, was that it was exactly invariant only in a world free of electromagnetism; the second was that its symmetry was completely unconnected with all the other symmetries that were part of the basic space-time framework of physics. This second aspect was discussed in 1954 in a pioneering paper by Yang and Mills[(34)]. When that paper appeared in print, it was a disappointment to a young man, Ronald Shaw, who was just completing his Ph.D. in Cambridge. The last chapter of the thesis that he had completed a few months before contained virtually the same ideas and formalism. It was never published[†].

The starting point is the remark that just one of the established exact symmetries is also quite unconnected with space-time structure. This is the "phase invariance" of the state vectors of all charged particles. It is sufficient to ensure the conservation of electric charge. However, it had long been understood that there was a much deeper way of looking at electric charge and its conservation; electric charge is essentially connected with the electromagnetic field. In place of phase invariance, which is unrelated to space-time (it is "global") one had recognised the existence of gauge invariance in which the phase of state functions is transformed at every point of space time independently, interlocking with this

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† It is also noteworthy that a full 16 years earlier something very close to the same mathematical structure was presented in a paper to a conference in Warsaw by O. Klein[(35)]. His point of departure was entirely different and he was concerned with the interaction of nucleons with photons and spin 1 pions. Previously Klein and Kaluza had developed a theory unifying gravitation and electromagnetism within a five-dimensional generalization of space-time. In the paper in question Klein introduces $2 \times 2$ matrices into this formalism and derives a coupling of nucleons, initially to photons and charged pions, but in a discussion remark, also to neutral pions. His equations display precisely the same central features of non linearity in very much the same form as in the Yang-Mills theory.

the electromagnetic potential field - the "gauge field" - undergoes a transformation. Could the same relationship be constructed for isospin, interlocked with its new "gauge field"?

Yang and Mills show how this can be done and here I will indicate how by a juxtaposition of their procedure with the familiar one in electromagnetism.

| (EXACT) CHARGE CONSERVATION | | (APPROXIMATE) ISOSPIN CONSERVATION | |
|---|---|---|---|
| | are ensured by the invariance | | |
| $\psi' = e^{i\alpha}\psi, \quad \psi^{+'} = e^{-i\alpha}\psi^{+}$ | | $\psi' = S^{-1}\psi, \psi^{+'} = \psi^{+}S \quad (S^{+} = S^{-1})$ | (8.1) |
| | Wider (local) invariance is made possible by introducing a 4-vector field, which enters the equations in the combination | | |
| $\frac{1}{i}\frac{\partial}{\partial x_\mu} - e\,A_\mu(x)$ | | $\frac{1}{i}\frac{\partial}{\partial x_\mu} - \varepsilon\,B_\mu(x)$ | (8.2) |
| $A_\mu$ = electromagnetic 4-vector potential | | $B_\mu$ = 4-vector potential of matrices. | |
| E.M. field: | | Y.M. (matrix) field: | |
| $F_{\mu\nu} = \frac{\partial}{\partial x_\nu}A_\mu - \frac{\partial}{\partial x_\mu}A_\nu$ | | $F_{\mu\nu} = \frac{\partial}{\partial x_\nu}B_\mu - \frac{\partial}{\partial x_\mu}B_\nu + i\varepsilon(B_\mu B_\nu - B_\nu B_\mu)$ | (8.3) |
| | the transformations are: | | |
| $A_\mu' = A_\mu + \frac{1}{e}\frac{\partial}{\partial x_\mu}\chi$ | | $B_\mu' = S^{-1}B_\mu S + \frac{i}{\varepsilon}S^{-1}\frac{\partial}{\partial x_\mu}S$ | (8.4) |
| | coupled with | | |
| $\psi' = e^{i\chi(x)}\psi$ | | $\psi' = S^{-1}(x)\psi$ | (8.5) |
| | and | | |
| $F'_{\mu\nu} = F_{\mu\nu}$ | | $F'_{\mu\nu} = S^{-1}F_{\mu\nu}S$ . | (8.6) |

Yang and Mills then show that if one writes

$$B_\mu = \underline{\tau}\cdot\underline{b}_\mu \quad (\text{and } F_{\mu\nu} = \underline{\tau}\cdot\underline{f}_{\mu\nu}) \tag{8.7}$$

one defines a 12 component (four-vector × iso-vector) basic gauge field $\underline{b}_\mu$ which not only links to the isodoublet as used above, but similarly to any other iso-multiplet. There is a structural similarity to vector meson theory, the interaction of $\underline{b}_{\mu}$ with other fields being essentially the same as for the state vector $\underline{\phi}_{\mu}$ of the vector pion, but there is a vital difference. As exemplified in (8.3) the equations of the new ("non abelian") gauge field are highly non linear. I need not display them, but one consequence needs mentioning; there exists a conserved charge current density of isospin

$$\underline{J}_{\mu} = i\, e\, \bar{\psi}\, \gamma_{\mu}\, \underline{\tau}\, \psi - 2e \frac{\partial}{\partial x_{\mu}} \underline{\phi} \times \underline{\phi} + \ldots + 2\varepsilon\, \underline{b}_{\nu} \times \underline{f}_{\mu\nu} \qquad (8.8)$$

to which the gauge fields themselves contribute the last term. The + . . . refers to any other isospin multiplets of particles that there might be.

The third component of the total isospin derived by integration of $\int J_{\mu 3}\, d\sigma_{\mu}$ includes a contribution from the last term - and it follows that the Yang-Mills field carries a charge which is $^{e}/\varepsilon$ times the 3rd component of its isospin.

In 1954 this "non abelian" gauge theory of isospin was a highly original new idea. Its relevance to the real world was rather problematical. It postulated the existence of new spin 1 particles whose mass was not predicted - this at a time when spin 1 particle theories were under a cloud because they could not be renorised.

How great a future there was to be for this new idea is not for me to relate, but I cannot refrain from saying in conclusion that, 'non abelian' gauge theories, of which this was the first, have not only been made respectable, but that, linked with the revolutionary idea of <u>spontaneous</u> symmetry breaking, one has learnt that at a deeper level than was previously contemplated, isospin - enlarged to be part of much more subtle symmetry schemes, is to be seen as part of a highly symmetrical real world - in the fullest possible sense a part of real physics.

## REFERENCES


(1) Rasche, G., Archiv. for Hist. of Exact Sci. 7 (1971) 257.

(2) Mukherji, V., Archiv. for Hist. of Exact Sci. 13 (1974) 27.

(3) Chadwick, J., Nature 129 (1932) 312;
Proc. Roy. Soc. A136 (1932) 705.

(4) Iwanenko, D., Nature 129 (1932) 798.

(5) Heisenberg, W., Zeits. f. Phys. 77 (1932) 1.

(6) Heisenberg, W., Zeits. f. Phys. 80 (1933) 587.

(7) Majorana, E., Zeits. f. Phys. 82 (1933) 137.

(8) Tuve, M.A., Heydenburg, N. and Hafstad, L.R., Phys. Rev. 50 (1936) 806.

(9) Breit, G., Condon, E.U. and Present, R.D., Phys. Rev. 50 (1936) 825.

(10) Cassen, B. and Condon, E.U., Phys. Rev. 50 (1936) 846.

(11) White, M.G., Phys. Rev. 49 (1936) 309.

(12) Bartlett, J.H., Phys. Rev. 49 (1936) 102.

(13) Volz, H., Zeits. f. Phys. 105 (1937) 537.

(13a) Breit, G. and Feenberg, E., Phys. Rev. 50 (1936) 850.

(14) Kemmer, N., Nature 140 (1937) 192.

(15) Volkoff, G.M., Phys. Rev. 62 (1942) 134.

(16) Wigner, E., Phys. Rev. 51 (1937) 106.

(16a) Proceedings of the Robert A. Welch Foundation. Conferences on Chemical Research, (Houston, Texas) Vol. I, Chapter 4 (1958).

(17) Fermi, E., Zeits. f. Phys. 85 (1934) 161.

(18) Tamm, J., Nature 133 (1934) 981.

(19) Iwanenko, D., Nature 133 (1934) 981.

(20) Gamow, G. and Teller, E., Phys. Rev. 51 (1937) 289.

(21) Wentzel, G., Helv. Phys. Acta 10 (1937) 108.

(22) Kemmer, N., Phys. Rev. 52 (1937) 906.

(23) Yukawa, H., Proc. Phys.-Math. Soc., Jap. 17 (1935) 48.

(24) Pauli, W. and Weisskopf, V., Helv. Phys. Acta 7 (1934) 709.

(25) Neddermeyer, S.H. and Anderson, C.D., Phys. Rev. 51 (1937) 1005.

(26) Kemmer, N., Proc. Camb. Phil. Soc. 34 (1938) 354.

(27) Bethe, H.A.. Phys. Rev. 55 (1939) 1261.

(28) Lattes, C.M.G., Powell, C.F., Occhialini, G.P.S. and Muirhead, H., Nature 159 (1947) 694.

(29) Bjorklund, R., Crandall, W.E., Mayer, B.J. and York, H.F., Phys. Rev. 77 (1950) 213.

(30) Steinberger, J., Panofsky, W.H.K. and Steller, J., Phys. Rev. 78 (1956) 802.

(31) Gershtein, S.S. and Zel'dovich, Ya.B., J.E.T.P. 29 (1955) 698 transl. Sov. Phys. J.E.T.P. 2 (1956) 576.

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

(33) Gell-Mann, M., Rev. Mod. Phys. 31 (1959) 834.

(34) Heitler, W., Proc. Roy. Irish Acad. 51 (1946) 33.

(34) Yang, C.N. and Mills, R.L., Phys. Rev. 96 (1954) 191.

(35) Klein, O., New Theories in Physics. Internat. Inst. of Intellectual Cooperation, Paris, 1939. (1938 Warsaw Conference organised in collaboration with I.U.P. and Polish Intell. Coop. Ctee.)

DISCUSSION

E. WIGNER.- I would like to add a tiny thing to the early part of this wonderful address and this is that the pp scattering which was investigated by TUVE, HEYDENBURG and HAFSTAD was also investigated by two people in Princeton, DALSASSO and WHITE, and actually I learned from them much more since I saw them in my own institution.

N. KEMMER.- You're quite right. In my written report a paper by WHITE is mentioned, briefly, but I was talking of this group of three papers in the way it came to me and influenced my work.

E. WIGNER.- My second point was mentioned already yesterday to some extent and today again ; that is the so called superallowed transitions which in my opinion proved most strongly the validity of the Isotopic spin concept. It turns out that the matrix element for a β-decay is very much the same as the matrix element we heard of which transforms the different parts of an isotopic spin multiplet into each other. It would follow from this that the only β transitions which are allowed are those between different members of an isotopic spin multiplet. Of course that is not so. Other transitions do take place. But it is true that the β transitions between members of an isostopic spin multiplet have generally a transition probability one thousand times greater than the other ones, and this is very interesting.

When one listens to general discussions about nuclear structure, one is struck by the fact that the interaction is very local and the shell structure is very valid. However, is it possible to consider the more complex, more higher mass nuclei as so having something like a metallic structure with the pions and so on, the role of that of the electrons in the metals, in other words that there is a cloud of pions which keeps the nucleons together and as a result the nucleons do move independantly of each other just as electrons do move in a metal. Has this been considered and is this a possibility or would you say this is very unreasonable ?

N. KEMMER.- I don't think I am well enough informed on the nuclear structure ideas to answer myself, but could I invite anyone else to answer it ?

E. WIGNER.- This is too crazy an idea !

N. KEMMER.- I would like to ask Professor Gell-Mann if he wants to say something now or later.

M. GELL-MANN.- During Professor Kemmer's delightful talk, I was reminded by my own thought processes and also by a whispered remark by my neighbor Viki Weisskopf that charge independence is still not well understood. What is in fact the present state of our understanding ?

Of course, the fundamental underlying principle is that the strong color force belongs to a SU(3) Yang-Mills theory and is therefore universal for all the flavors of quarks. But what about the quark masses that violate the flavor symmetry ? There are five known flavors of quarks and there may be six (or possibly even more), but for the most familiar hadrons it is the very light ones u and d that count. The high-frequency masses (or current quark masses) are the relevant parameters for approximate isospin conservation and those do not seem to have a ratio near 1, but rather a ratio closer to 2. What is important is that those masses are both very small. In the limit in which they go to zero, the vector and axial vector isospin currents are conserved (apart from electromagnetic and weak corrections). The approximate symmetry is really chiral SU(2) X SU(2).

The symmetry of the vector current charges is realized by approximate degeneracy (isotopic spin conservation), while the symmetry of the axial vector current charges is realized by having a Nambu-Goldstone boson of nearly zero mass, the pion, an effect that we called "partially conserved axial vector current."

Thus in the expression $\underset{\sim}{\tau}\cdot\underset{\sim}{\pi}$, both the approximate conservation of isotopic spin (including $\underset{\sim}{\tau}/2$) and the prominence of the $\pi$ come from the near-vanishing of the high-frequency masses of the u and d quarks.

Why those masses are so tiny we still do not understand.

C.N. YANG.- A simple historical remark. The idea that charge conservation is related to phase transformation invariance was due to Weyl. He had in 1918-1920 proposed a scale transformation invariance which he called "gauge" invariance. That gauge theory was opposed among others, Einstein. After quantum mechanics the factor $\sqrt{-1}$ was inserted and phase transformation took over. Weyl then emphasized the relationship between charge conservation of phase invariance.

L. BROWN.- Although in his beautiful talk, Professor Kemmer related Yukawa's meson theory to the Pauli-Weisskopf theory, the 1935 paper of Yukawa did not use that theory and it was not known to Yukawa at that time. In fact, Yukawa's first meson paper quantized the fourth component of a 4-vector field, analogous to the electromagnetic scalar potential. Thus Yukawa's meson was spin zero, but not a relativistic scalar.

N. KEMMER.- Thank you very much. That's quite true. If my memory is right my reading of Yukawas paper at the time immediately related to Pauli-Weisskopf. But as Professor Amaldi can confirm, just yesterday I made that very point to him ! You know, this is related to the fact that people who interpreated Yukawa's first paper as dealing with a scalar meson said he had got the sign wrong ! If you replace a scalar by the 4th component of a 4-vector which is what he did, you get the opposite sign. However, in that first paper Yukawa did not yet have the full (Proca) equations for a spin 1 particle.

C. PEYROU.- Commenting on your introduction and the relation between ordinary spin and isotopic spin I wish to mention that in introducing the exclusion principle Pauli did not use the spin but an abstract internal quantum number with two values. In his Nobel prize speech, he says that he was very reluctant to admit the spin concept because it was a mechanical dynamic quantity (angular momentum) and he preferred the abstract two valued variable he had introduced.

E.C.G. SUDARSHAN.- The first application of isospin to pion-nucleon scattering was by Heitler in 1945. Some ideas were used by Fermi, Anderson et al. for $\pi$N scatering dominated by the (3,3) resonance. All these used reduced matrix element and C-G coefficients.

The same results were derived by Smushkevich by a simpler arithmetical method (reviewed by Marshak-Sudarshan in their book).

The first application of broken isospin symmetry was by Okubo, Marshak and Sudarshan in Phys. Rev. 1956 to find the relation between magnetic moments and mass differences of isospin multiplet. The idea was to deal with the reduced matrix elements of tensor operators. Okubo used it later to find SU(3) mass relations to derive what is now called the Gell-Mann/Okubo mass formula. Mac Farlane and Sudarshan used it for SU(3) electromagnetic relation in Nuovo Cimento 1960.

N. KEMMER.- I must thank you very much for these remarks which, I hope, will be published in the proceedings. As regard Heitler's paper, I was intending to mention that. I have omitted it from my manuscript, it was a lapse of memory. There was to have been a short reference but yesterday, I was discussing it with Professor Dalitz, and I said "Oh my goodness". I must add a footnote to my manuscript ! You have extended that and I very much hope that it will appear as an addendum to what is reported by me. Thank you.

V. TELEGDI.- It has often be said that Stückelberg invented the meson theory of nuclear forces independly of Yukawa. Since you were in Zurich at that time, would you please comment on this ?

N. KEMMER.- Perhaps Professor Weisskopf can say a little more about it than I. Stückelberg certainly was thinking on the same lines. It was very difficult to communicate with him and a lot of his written stuff was really rather poorly written and he has not received the credit that he should have for his work on that. That's absolutely true.

V.F. WEISSKOPF.- I do not remember Stückelberg's contribution to the pion exchange theory of nuclear forces. Most of his important contributions to relativistic field theory and renormalization procedures were made before others reinvented them. But even the great Pauli was unable to make sense of his talks and papers. This is why his contribution did not get the deserved credit and do not get it today.

N. KEMMER.- I think I remember a paper on meson theory. I will have to look it up. I should have done it for my talk, but did'nt. It appeared and it really was not very clear. [Phys. Rev. 52 (1937), 41 ; 54 (1938), 137]

J. TIOMNO.- Using the advantage of my position I will make an observation which I could not do yesterday because the session was closing. It is also connected with Professor Kemmer's talk because he mentioned the question of the V-A interaction. I like mention my 1954 paper (Nuovo Cimento, 1, 226) where I first introduced the $\gamma^5$ transformation for massive particles which I called "mass reversal" (after Peaslee) later called chiral transformation. I have shown there, before parity violation was discovered, that the only possible weak interactions which were invariant under the $\gamma^5$ transformation for all particles and are antisymmetric or symmetric in the permutation of neutral particles are A-V or S + P - T. Actually the only transformation of interactions which does not have the $\gamma^5$ invariance but is invariant under permutation of all particles is the S - A - P which consequently Yang and myself had proposed (1950) as the first theory of universal Fermi interaction. The previous papers including my own with Wheeler gave the evidence that there was some universality but no hint about a unique form of the theory.

In 1957, I wrote the paper, which I believe was the first (Nuovo Cimento 6, 912) on the universal Fermi interaction parity violation theory, picking up, the S + P + T interaction which "obviously" (!) was in agreement with experience. For this purpose I used only 1/2 $(1 \mp \gamma_5)$ projections for $(^{\nu}_{\nu_\mu}e)$, respectively.

In the S + P - T case the $\gamma^5$ transformation is done only for the neutrinos but in the A-V it is done for all particles which could have been a hint of its superiority.

L. MICHEL.- This universal interaction S-A-P that you said you proposed with Yang was already well-known. It was called the Critchfied and Wigner interaction (L. CRITCHFIED, E. WIGNER, Phys. Rev. 60, 412, 1941). And of course, for instance, in my paper in μ-decay, I quoted them and gave the corresponding value of ρ i.e. $\frac{1}{2}$.

J. TIOMNO.- This is correct. The totally antisymmetric form of the Fermi interaction is due to Wigner and Critchfield (1942) and we gave credit to them in our paper. This was our choice as the unique form of the Universal Fermi Interaction. Actually this was the first parity conserved Universal Fermi Interaction theory and, to my knowledge, the occasion when the expression Universal Fermi Interaction was coined.