EARLY HISTORY OF SPIN EXPERIMENTS

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Discovered as an essential element of electromagnetic interactions, the spin is associated from the very beginning to nuclear forces and can play a selective role between different theories or models. The spin experiments have often required ingenuity and unconventional techniques. A few of them are recalled here from the early times.

1 Introduction

As it is, the history of spin experiments spans more than one hundred years. Modern time experiments, namely the experiments which in the past we had been discussing in the Spin Symposia series, were recollected and put in perspective by previous speakers, who even had an outlook on the future. Why not to complete the picture and give an overview of how did we arrive there? The acknowledgment of the roots may help in foggy days ...

With this in mind, I shall sketch some facts of the very old days, which nowadays are mainly buried in text-books and often neglected. It was out of reach to be complete, and I apologize if I happen to forget events that others may judge worth recalling.

2 The spin of the electron

> "... the word *spin* in an English dictionary, in addition to the meaning to rotate, it can mean twisting a fiber to form thread, and perhaps from there it can also mean to draw out something. Especially there is the idiom *spinning a yarn*, and this seems to describe an old sailor talking on and on about his youthful adventures."
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> Sin-Itiro Tomonaga, *The history of spin*[1]

The discovery of the spin in October 1925 by two graduate students, George Eugene Uhlenbeck and Samuel Abraham Goudsmit is indeed a long tale, the tale of the old quantum mechanics, with experiments calling for refined measurements of atomic spectra from hydrogen and hydrogen-like ions.

I should like to recall that the first hydrogen lines

H_α	H_β	H_γ	H_δ
6562.8 Å	4861.3 Å	4340.5 Å	4102.2 Å

were observed by Ångström in 1853, and the Balmer formula was discovered in 1885 [2]. The picture started to change around 1890 thanks to the high-resolution gratings designed by Rowland. Two lines of experiments (with and without magnetic field), say of thinking (*Zeeman effect* and *fine structure of spectral lines* respectively) developed in the first quarter of this century. In both cases the start was a broadening of spectral lines.

2.1 The fine structure of the spectral lines

Already in 1892 Michelson noticed that the intensity curve of the two lines H_α and H_γ of hydrogen looked as originating from a double source, thus giving the first indication of a *fine structure* for these lines [3]. For a number of years, many people, even Bohr, thought to be in presence of an instrumental effect.

In 1911, based on a novel class of experiments (the scattering of α particles by gold) Rutherford introduced the idea of *nucleus* in his model of atom [4]. Then, in 1913, Bohr stepped in to explain how hydrogen atoms could radiate [5]. The Balmer formula came out naturally from his theory; it was the first triumph of quantum dynamics. Curiously enough, Bohr had no recollection of the Balmer's formula until shortly before he worked out the implications of his model [6].

At the end of 1914 it was finally recognized that the lines H_α and H_γ were double, but there was disagreement as to the exact separation of the components. At the same time the existence of a small systematic deviation of the hydrogen lines from the position given by the Balmer law was admitted [7].

Bohr tried to work out a correction to the Balmer formula by using relativistic expressions for the electron energy and momentum, still retaining circular orbits [8]. It was a step in the right direction but the correction, of the order of α^2, was not sufficient. It should be noticed that the symbol "α" does not appear in Bohr's paper. The quantity $\alpha = 2\pi e^2/(hc)$, known as *fine structure* constant, was introduced by Sommerfeld nearly at the same time [9].

Contrary to other physicists, Sommerfeld took the atom of Bohr very seriously since the very early days. He considered the implications of elliptical orbits, namely the need for a postulate more general than the original one of Bohr and the quantization of the action integral for any pair of canonical variables [9]. This led him to the introduction of an azimuthal quantum number $n_\phi = k$ beside the radial quantum number n_r. The azimuthal quantum number defined the orbital angular momentum of the electron ℓ in units of $h/2\pi$

($k=\ell+1$). The energy levels were only depending on $n=n_r+k$, except for a slight dependence on k once the relativistic expressions were introduced [10]:

$$E_{n,k}=-R'\left[\frac{Z^2}{n}+\frac{\alpha^2 Z^4}{n^3}\left(\frac{1}{k}-\frac{3}{4n}\right)\right]+O(\alpha^4),$$

where $n=1,2,3\cdots$ and $k=1,2\cdots,n$. R' is the Rydberg constant. The first term in the square bracket gives the old Balmer formula, the second one the main correction. The formula is valid for hydrogen and hydrogen-like atoms with a nucleus of charge Ze.

It was immediately recognized that the Sommerfeld formula solved at the same time the problem of the *fine structure* and that of the correction to the Balmer formula. Paschen had investigated experimentally the fine structure [11], in particular that of the simply ionized helium He^+. The agreement of his precise measurements with the Sommerfeld formula could not be better.

There were some difficulties still left which, however, did not affect the validity of the Sommerfeld formula. A comparison with the fine structure of the line $\lambda=4686$ Å of He^+ showed that most of the 12 components predicted for the transition $(n=4)\rightarrow(n=3)$ were seen in presence of a magnetic field, but their number dropped to one half when the field was removed.

Bohr [12], and independently A. Rubinowicz [13], a student of Sommerfeld, proposed the introduction of selection rules. The rule $\Delta\ell=\pm1$ reduced the number of components from 12 to 5. Altogether, the situation appeared very satisfactory, and the discovery of the Sommerfeld formula was later considered as a turning point for the discovery of the spin. One dark point: the six lines observed at the predicted frequency included a component forbidden by the $\Delta\ell$ rule. Seven more years had to pass by before the solution could be found!

2.2 The Zeeman effect

The Zeeman effect was discovered at the end of the nineteenth century. In 1896 Zeeman found a broadening of the sodium D-lines as an effect of magnetic field [14]. The following year he reported for the first time a splitting of lines, and well in agreement with Lorentz calculations [15]. But he was observing the blue line of cadmium (4800 Å), and the rare-earths do not behave like the alkalis in the same magnetic field. In the case of cadmium one observes the *normal* Zeeman effect: the lines are split into doublets or triplets, according to whether the light is emitted parallel or perpendicular to the magnetic field. In the case of sodium one has the *anomalous* Zeeman effect: had Zeeman resolved the D-line broadening, he would have seen not two, but ten lines (four for D_1 and six for D_2)! In 1912 Paschen and Back recognized that the normal Zeeman effect takes place in a strong magnetic field (the Paschen-Back effect) [16].

The normal Zeeman effect was explained in 1916, with the discovery of *space quantization* by Sommerfeld and Debye [17]. The application of a constant magnetic field to a hydrogen-like atom induces a Larmor precession with a third frequency of the motion. Sommerfeld and Debye showed that the component of angular momentum in the field direction is also quantized, with values $mh/2\pi$ and the "magnetic quantum number" m taking integral values from $-\ell$ to $+\ell$: $m = -\ell, -\ell+1, \cdots + \ell$. The existence of space quantization was confirmed by Stern and Gerlach in 1920, in Frankfurt, with a famous series of experiments [18].

Understanding the anomalous Zeeman effect on the contrary took several years, starting in 1919, when a pupil of Zeeman [19] suggested that if in absence of magnetic field the frequency of the light emitted is given by $h\nu = \Delta E_2 - \Delta E_1$, in a magnetic field the frequency shift should be $h\Delta\nu = \Delta E_2 - \Delta E_1$. The latter expression provided a way to organize the experimental data, even if an equation for ΔE was missing.

In 1921 Alfred Landé [20] introduced a factor g, the Landé factor, in the expression for the energy of a Bohr magneton μ in a magnetic field: $\Delta E = mg\mu H$. He assumed in general $g \neq 1$ ($g = 1$ in the case of the normal Zeeman effect) which implied a gyromagnetic ratio $g \times e/(2m_0 c)$.

In the early twenties people used to describe the quantum state of the electrons in an atom with two quantum numbers j and ℓ. j was called the inner quantum number. The two numbers referred to the ensemble of electrons in an atom, not to a single electron in some orbit, and were always assumed to be integer. Landé proposed that the magnetic quantum m had to be half-integer in the case of the alkalis. The equality $m = -j, -j+1, \cdots + j$ was still valid and this implied that j was half-integer when such was m.

In the same year 1921 Heisenberg published his first paper [21]. He considered a model for the alkalis in which he distinguished between a valence electron and a *core*, or *Atomrumpf*, consisting of the ensemble of all the internal electrons. Valence electron and core, each had angular momentum $1/2$. Apparently Heisenberg had the idea of half-integer quantum numbers before Landé, but he did not published it because Sommerfeld did not like it [6].

Inspired by Heisenberg's paper Landé took the idea of a core angular momentum that he called R (from Rumpf!). He made $R = 1/2$ for the alkalis, proposing the vector relation $\vec{j} = \vec{\ell} + \vec{R}$, with $\vec{\ell}$ the orbital momentum of the valence electron and $\vec{R}$ the angular momentum of the core.

In some way he then worked out a formula:

$$g = 1 + \frac{j(j+1) + R(R+1) - \ell(\ell+1)}{2j(j+1)}$$

from which one gets $g = 2$ for the core ($j = R$, if $\ell = 0$), and this formula, in some way, solved the problem of the anomalous Zeeman effect (the spin had not yet been invented!). Is this an example of how people worked in the days of the old quantum theory: *gifted physicists were able to make important progress without knowing what they were doing. The formula looked peculiar, but it worked*[6].

In December 1924 Pauli proved in his paper on the Zeeman effect [23] that the idea of a core angular momentum did not make sense. Thus, he assigned zero angular momentum to all the closed shells (including the suspected K-shell). However, the Landé's formula for g worked correctly, which implied that R had to be kept. The most natural thing for Pauli was to admit that R had something to do not with the core but with the valence electron. In Pauli's own words the anomalous Zeeman effect was "due to a peculiar not classically describable two-valuedness [Zweideutigkeit] of the quantum properties of the valence electrons".

Pauli could easily take the next *obvious* step and discover the spin, but his mind went in a different direction. He discovered instead the Pauli exclusion principle, taking over from Stoner's rule. Stoner had just observed that the number of electrons in each completed shell was equal to twice the sum of the inner quantum numbers [24].

Pauli proposed[25] to generalize his two-valuedness hypothesis and to extend it to all the atoms (not only to the alkalis). He set two postulates: the two-valuedness applies to every atomic electron; it is expressed by a new two-valued quantum number. The introduction of such a fourth quantum number doubles the number of possible states and explains the factor 2 found by Stone.

Pauli concluded that each set of the four quantum numbers defined a state and that a single state could be occupied only by one electron. However, he admitted that he was not able to justify this rule. His four quantum numbers were later re-defined by Goudsmit in a more clear way[26]: n, ℓ, m_ℓ and a fourth quantum number $m_R = \pm 1/2$.

Clearly m_R was the quantum number of the spin, embedded in the exclusion principle, waiting to be taken out ...

2.3 Goudsmit and Uhlenbeck, their first paper

Goudsmit and Uhlenbeck were graduate students at the University of Leiden, under Ehrenfest [27]. Uhlenbeck, senior to Goudsmit by two years, had been for a while uncertain whether to move to humanities or to continue physics. Ehrenfest persuaded him to learn first what was happening in physics by working with him and learning from Goudsmit, who was already known in the field of optical spectroscopy.

In August 1925 Goudsmit and Uhlenbeck started regular meetings. In a very short time they were no more a teacher and a pupil but two people working together and, what's more, tied by lasting friendship. Uhlenbeck, interested in physics, but still somewhat detached with his mind half turned towards history, went on raising problems and putting that type of simple questions that some time do not come to the mind of people conversant with the subject, a modern Socrates. In particular he could not understand why the model describing hydrogen-like atoms was not valid for hydrogen, which instead required the *old* Sommerfeld theory. Why not to apply the same model also to hydrogen? Uhlenbeck's doubt contaminated Goudsmit.

A new level scheme was then worked out in analogy with the classification of the states for the alkalis, using half-integer numbers as it had been done successfully by Landé for the anomalous Zeeman effect. The relation $k = \ell + 1$ (see section 2.1) was replaced by $k = j + 1/2$, with j obviously half-integer: $j = \frac{1}{2}, 1\frac{1}{2}, 2\frac{1}{2}, \cdots, (n-1)\frac{1}{2}$ (the spin had not been invented yet!). In addition the selection rule $\Delta\ell = \pm 1$ was replaced by $\Delta j = 0, \pm 1$. Goudsmit and Uhlenbeck realized that the level splitting was the same as in the Sommerfeld theory, with different selection rules. With these assumptions the fine structure of the 4686 Å line in He^+ was explained. Ehrenfest was somewhat perplexed, but suggested that they wrote a little note, which they did – in Dutch – a note that did not attract any attention until much later. It was their first joint paper [28] that Uhlenbeck called the *August paper*.

2.4 The discovery of the spin. – I

After explaining the Pauli scheme which assigned *four* quantum numbers to each electron, Goudsmit went on with the January 1925 paper where Pauli formulated the exclusion principle [25], and recalled his own re-definition of the Pauli quantum numbers [26] (n, l, m_l, m_s) with $m_s = \pm 1/2$. Uhlenbeck was impressed.

Uhlenbeck found Pauli's arguments purely formal. There was no connection with the Bohr's atom: *It was then that it occurred to me that since ... each quantum number corresponds to a degree of freedom of the electron, the fourth quantum number must mean that the electron had an additional degree of freedom – in other words, the electron must be rotating!* [27]. As recalled by Uhlenbeck [27], Goudsmit had written that at that time he did not know what a degree of freedom was: Goudsmit had never passed his mechanics exam and had no right to teach mechanics in the Dutch high school, not even after getting his Ph.D., though he later was teaching the graduate course in mechanics at the University of Michigan.

Goudsmit immediately appreciated that if the angular momentum of the electron was $\hbar/2$ one had rotation of the electron with two possible orientations of its axis respect to the orbital plane. Assuming for the gyromagnetic ratio of the electron *twice* its classical value $e/(2mc)$, one got for the magnetic moment of the electron exactly one Bohr magneton. The hypothesis of the rotating electron also confirmed that the properties originally attributed by Landé to the *core* were in effect properties of the valence electron, in agreement with Pauli ideas. The existence of a rotating electron led also to the interpretation of j as the total angular momentum of the electron, namely $j = \ell + s$.

Goudsmit and Uhlenbeck were in an excited state, but they thought that such a new concept, a rotating electron, that no physics VIP had ever developed, could not be published. For some reason it had to be non-sense. On the contrary Ehrenfest was immediately interested.

The fact that one had to take a gyromagnetic ratio *twice* its classical value, as originally proposed by Landé to get agreement with experiments, was still disturbing. However, following a suggestion from Ehrenfest, Uhlenbeck found in an old paper by Max Abraham that the gyromagnetic ratio of a spherical electron having only surface charge was *twice* the classical gyromagnetic ratio. Abraham's calculations, non relativistic and old-style, clearly were only indicative, but they came as an encouragement. Ehrenfest thought that Goudsmit's and Uhlenbeck's idea was either very important or non-sense, and had to be published. He suggested that they wrote a short note for Naturwissenschaften and gave it to him: *and we will ask Mr Lorentz.* The note was prepared and, with Goudsmit's agreement, Uhlenbeck signed it as first author.

Lorentz was particularly negative. The surface velocity of an electron with radius $r_0 = e^2/(mc^2)$ rotating with angular momentum $\hbar/2$ would be ten times that of light! the magnetic energy of an electron having one Bohr magneton magnetic moment would be so large as to require a radius ten times its classical value to keep its mass m. Uhlenbeck, convinced that their idea was really non-sense, rushed to see Ehrenfest. It was too late. Ehrenfest had already sent out the note [29]: *You are both young enough to be able to afford a stupidity*! This is how Uhlenbeck and Goudsmit happened not to miss the discovery of the spin, but the story does not end here: they still had to convince the VIPs of Physics. The note was dated 17 October 1925. It appeared in the middle of November.

2.5 The discovery of the spin. – II

On 21 November 1925, about a week after the publication of the note [29], Goudsmit received a letter from Heisenberg. It was a letter of appreciation for their courageous idea which would remove all the difficulties of the Pauli theory. He noted that it led to the *Landé-Sommerfeld* formula for the alkali doublets except for a factor 2 and he wished to know how they got rid of this factor.

Uhlenbeck and Goudsmit did not know that early in the same year Ralph de Laer Kronig, aged 20, today known for the Kramers-Kronig dispersion relations, had also discovered the spin of the electron during a short visit to Landé in Tübingen. However his idea had been rejected by Pauli and shortly afterwards, in the course of a visit to Copenhagen, also by Heisenberg and Kramers [30,31]. Heisenberg was now coming back with Kronig's work!

Kronig had in fact calculated the energy levels of an electron having an intrinsic angular momentum $s = 1/2$ and a magnetic moment equal to one Bohr magneton. In its rest system the moving electron would be subject to the magnetic field resulting from a Lorentz transformation applied to the radial electric field, in addition to the electrostatic attraction from the (screened) nucleus. The rotation axis of the electron could then take two orientation, parallel or antiparallel to the magnetic field.

In order to get agreement with the experimental data on the fine structure of hydrogen-like atoms a compensation between the relativistic precession of the orbit in its plane and the precession of the spin axis induced by the magnetic field was necessary, in such a way to make levels with different ℓ but the same j coincide, giving the same number and pattern of energy levels as predicted by the Sommerfeld theory. Kronig found that both effects were proportional to the fourth power of the atomic number. This result was strongly encouraging at a time when the magnetic interaction between the orbit of the valence electron and a magnetic moment corresponding to an angular momentum of the electrons in the core gave a third power dependence on the effective nuclear charge, while the experimental data required fourth power dependence. However the spin-orbit coupling turned out a factor 2 too large.

Uhlenbeck and Goudsmit had never derived this formula, and knew nothing about this factor *two*: ... they *set forth the idea of an intrinsic angular momentum of the electron, accompanied by a magnetic moment of one Bohr magneton without, however, realizing the nature of the coupling of this moment to the orbital motion* ldots [30]. Pauli, who did not believe in models, talked of *a peculiar not classically describable two-valuedness of the quantum theoretical properties of the valence electron.* Bohr introduced a new force, the *non-mechanical strain.* For this reason the Letter to Naturwissenschaften [29] had the curious title: *Replacement of the Hypothesis of the Non-mechanical Strain by an Assumption about the Internal Behaviour of Every Single Electron.*

Einstein came to Ulenbeck's and Goudsmit's aid. He happened to visit Leiden at the right time to explain them the relativistic nature of the coupling. This was sufficient. Uhlenbeck and Goudsmit computed the energy difference due to the two orientations of the spin by first order perturbation theory and got the Kronig formula including the mysterious factor *two*.

Shortly afterwards, at the beginning of December Bohr went to Leiden for the celebration of the fiftieth anniversary of the Lorentz doctorate. He knew the Letter to Naturwissenschaften [29] but he still did not understand the origin of the spin-orbit coupling. When he heard of the Einstein approach he was immediately convinced and accepted the idea of a rotating electron. He was not impressed by the difficulties raised by Lorentz (they will disappear when the real quantum theory is found) but took more seriously the factor 2. However, also in this case he was convinced that better calculations would solve the problem. He did not know the paper to Physica [28]. Goudsmit showed him that forgetting the factor 2 the combination of the Sommerfeld relativistic effect with the spin-orbit coupling gave also in that paper the (correct) answer for the spectrum of Hydrogen.

On the basis of their new experience Uhlenbeck and Goudsmit prepared a second letter, this time to *Nature* [32], with title *Spinning Electrons and the Structure of Spectra*. Dated December 1925 it appeared on 20 February 1926, with an approving postscript by Bohr. The word *spin* appeared for the first time in this Letter. However, still Bohr could not convince Pauli who kept talking of *erroneous teaching*.

Shortly afterwards also the problem of the factor 2 was solved, by Llewellyn H. Thomas [33]. It was a subtle relativistic effect, which surprised even Einstein, today well known as Thomas precession. When Pauli heard of it he finally accepted the idea of a rotating spin, as he mentioned later in his Nobel prize lecture.

The last word belongs to theory. In 1927 Pauli introduced his matrices to describe the electron as a two-component wave, to account for the two possible directions of the spin. In 1928 Dirac worked out the relativistic theory of the electron, linear in time derivatives. He had no thought of spin in mind: ... *this equation gave the particle a spin of half a quantum. And gave it a magnetic moment. It gave just the properties that one needed for an electron. That was really an unexpected bonus for me, completely unexpected* ....

In 1928 the spin had become a fundamental property of the electron, like charge and mass.

For the sake of completeness it should be recalled that already in 1900 G.F. FitzGerald raised the question whether magnetism is due to rotation of electrons, that in 1921 A.H. Compton considered a rotating electron in connection with ferromagnetism, and that in 1922 E.H. Kennard made calculations like those made by Abraham [6]. Kronig was certainly the one who anticipated the main part of the spin idea [27]; Pauli may deserve some criticism for discouraging him from publishing [31]. Uhlenbeck's and Goudsmit's *proposal came just at the right time ... and they had the luck and the privilege to be students of Ehrenfest* [27].

3 The spin of the elementary particles

3.1 The spin of the proton

Pauli proposed, already in August 1924, as an explanation of the hyperfine structure: *The nucleus possesses in general a non vanishing angular momentum ....* He later hinted that these words may have influenced Goudsmit and Uhlenbeck in their claim of an electron spin, but in 1961 Goudsmit wrote that they were not aware of Pauli's paper until five years later.

The proton spin was determined for the first time in June 1927 by David M. Dennison in the course of a stay in Copenhagen [34]. He interpreted a series of existing measurements of the specific heat of molecular hydrogen, from room temperature down to about 50 °K, assuming that the transition between ortho- and para-hydrogen was so slow as not to alter appreciably the equilibrium between the two components during the cooling down phase. He could interpret all data *if the nuclear* [ie proton] *spin is taken equal to that of the electron and only the complete antisymmetric solution of the Schroedinger wave function allowed* [6].

According to Tomonaga [1] Dennison got the idea of a very slow ortho-para hydrogen inspired by the work of Takeo Hori [35], a young Japanese who, also in Copenhagen, had just finished an experimental study of the band spectra of H_2, discovering the intensity alternation in the hydrogen rotational spectrum and determining the moment of inertia of the hydrogen molecule.

The theory of the molecular band spectra and specific heat had been worked out by Federick Hund [36], who finished it in February 1927, in Copenhagen. It is based on the relation between the statistics of a particle and its wave function.

The magnetic moment of the proton was determined in 1933 by R. Frisch and Otto Stern [37]. They found a moment between 2 and 3 μ_P, the present value being $\simeq 2.8\,\mu_P$.

3.2 The spin of the photon

As the history goes [6] the spin of the photon was acknowledged to value 1 by Bose in 1924, namely before one even used the words *spin* and *photon*. When deriving the Planck distribution (and the *Bose-Einstein* statistics), Bose simply *noticed* [38] the need for such a factor 2. As he explained later, he proposed that this factor came from the fact that the photon carried one unit of angular momentum and that this could only take two orientations. But Einstein removed the sentence from the paper and wrote to Bose that he did not see the need for introducing such a concept. He preferred to talk of two states of polarization of the light (at a time when the polarization of a particle did not make sense!). After the spin was discovered Bose did not bother to write to Einstein and claim priority: priorities did not matter to his eyes.

3.3 The spin of the neutrino

In his famous letter dated 4 December 1930 *Dear Radioactive Ladies and Gentlemen* to a group of physicists meeting in Tübingen (Hans Geiger, Lise Meitner et al.) Pauli [39] informally proposed the existence of a new component of the nucleus, a light particle having spin $\frac{1}{2}$ obeying the Fermi statistics. He was *desperately* trying to solve the problem of the energy-momentum conservation in β-decay raised by Bohr and the problem of the statistics of N^{14} and Li^6 (see section 3.4).

Very soon Pauli realized that the two problems had a different nature and in fact at the Pasadena meeting of the American Physical Society in June 1931 he presented only his ideas on the β-decay. Later, on the occasion of the *Convegno Volta* held in Rome on 11-18 October 1931, he discussed the same ideas with Fermi and Bohr. Fermi showed a positive interest in the new particle, Bohr insisted on his idea of energy non-conservation [39].

May I recall that it was at this time that Fermi jokingly suggested the name *neutrino* in the course of a conversation with other Rome physicists. The word *neutron*, more and more frequently used for the heavy component of the nucleus whose discovery was in the air, was creating confusion. Fermi was playing with the Italian endings *one* and *ino* used to differentiate big and small objects of the same kind, respectively [40].

The invention of new particles was not yet fashionable and Pauli's attitude was extremely careful. The first published account of his three year old idea can be found in the Proceeding of the 7th Solvay Conference [41] held in Brussels on 22-29 October 1933. Pauli presented his hypothesis of a new particle – *M. Fermi a proposé le nom "neutrino"* – in the course of the extensive discussion which followed Heisenberg's "Considérations Théoriques Générales sur la

Structure du Noyau": ... *Il me paraît admissible que les neutrinos possèdent un spin* $\frac{1}{2}$ *et qu'ils obéissent à la statistique de Fermi, bien que les expériences ne nous fournissent aucune preuve directe de cette hypothèse. ...*

Pauli explained later that his change of attitude was determined by the general clarification which took place at the Solvay conference after the discovery of the β^+-decay and of the neutron, and after the work of Heisenberg, Iwanenko and Majorana on the structure of the nucleus [39].

Fermi attended the 7th Solvay conference but he did not take part in the discussion that followed Pauli's intervention. It is possible that he had already developed his theory of β-decay. His first paper [42] (preceded by the letter to *Nature* rejected by the Editor [40]) was published in fact only two months after the Solvay Conference, in December 1933, and the full version of his theory [43,44] early in 1934. (the German version was received by *Zeitschrift für Physik* on 16 January 1934).

Pauli assigned spin $\frac{1}{2}$ to the neutrino tentatively. Fermi built his theory on the explicit assumption that electrons and neutrinos obeyed Pauli exclusion principle, in a world where conservation laws were valid. One could perhaps conclude that the spin $\frac{1}{2}$ assignment, due to Pauli's intuition, was put on solid ground by Fermi.

3.4 The spin of the neutron

The neutron was discovered in 1932 by Chadwick bombarding a Be^9 target with α-particles [45] (see section 4.2). Its discovery solved a problem of statistics and gave at the same time a first indication of spin 1/2.

It was already known that the N^{14} nucleus had spin 1 and obeyed Bose statistics. This fact was in contradiction with the Wigner's rule requiring half-integer spin and Fermi statistics in the case of a nucleus made up of 14 protons and 7 electrons, namely of an odd number of spin 1/2 particles. Iwanenko [46] pointed out that a nitrogen nucleus made up of 7 protons and 7 neutrons, namely of an even number of particles, satisfied the Wigner's rule provided the neutron had spin 1/2.

Other indications came from work on deuterium nearly at the same time. Deuterium was discovered a few weeks before the neutron, but indications that the deuteron spin could be 1 were available only in 1933 from measurements of band spectra of molecular deuterium [47]. This value was definitely confirmed one year later, in March 1934 by similar measurements [48]. With a deuteron spin 1 the neutron spin could be either 3/2 or 1/2. The latter value was confirmed only in 1937, from the analysis of data on neutron scattering by ortho- and para-hydrogen [49,50,51]. By that time the spin had already entered the domain of nuclear forces.

4 Spin and strong interaction

The forces binding together proton and neutron were first investigated in 1932 by Heisenberg [52]. He postulated that as in the case of the hydrogen molecule, these forces were produced by exchange of charge and spin, and were central forces. (Later non-central forces had to be introduced to account for the electric quadrupole moment of the deuteron [53], and, in the late forties, for the spin-orbit coupling [54,55].)

Five years after it was discovered through the study of atomic spectra and electromagnetic interactions, the spin appeared to have a role also in the domain of nuclear forces. But which nuclear forces ? For a while people thought that a single mechanism could be responsible for keeping together a nucleus, for large angle scattering off light nuclei, and for β-decay. As it has turned out, in all cases spin had to play a major role.

4.1 First nuclear models

Departure from the Coulomb Law was observed for the first time by Rutherford in 1919 in a scattering experiment of α-particles by hydrogen. Chadwick and Bieler found in 1921 that the force became very much stronger for distances between the α-particle and the proton $< 4 \times 10^{-13}$ cm. A similar behaviour was observed in magnesium and aluminium [56]. For scattering by heavy nuclei no deviation form the Coulomb Law was seen, at least with the α-sources available at that time.

In 1927 Rutherford pointed out a difficulty. Scattering by heavy nuclei implied a radius $< 3.2^{-12}$ cm. However, assuming that the slowest α-particles emitted by α-decay left the nucleus with nearly zero energy, the radius of the Uranium nucleus was expected to be $> 6.5 \times 10^{-12}$ cm. Under these conditions, unless the Coulomb Law broke down at this distance from the centre of the nucleus, α-particles could not go through the potential barrier around the nucleus and α-decay could not take place.

This difficulty was solved in 1928 by Gamow [57], and a bit later by Gurney and Condon [58] independently. Quantum mechanical tunnelling allowed α-particles to penetrate potential barriers. It was the first application of quantum mechanics to nuclei.

From his study of heavy nuclei and comparison with light nuclei Gamow suggested a dependence of the type $R \propto A^{1/3}$ for the nuclear radius and $\propto A$ for the volume. From his estimate $R = 7.35 \times 10^{-13}$ cm for RaEm, and Bieler's value $R = 3.4 \times 10^{-13}$ cm, he found the relation $R = 1.2\, A^{1/3} \times 10^{-13}$ cm for the radius of nuclei.

Gamow [59] imagined the nucleus made up of α-particles held together by strong forces rapidly decreasing with the distance and becoming strongly repulsive if they approached each other more closely than a critical distance. Weak Coulomb forces were also present. A nucleus looked similar to a drop of liquid where little hard spheres were kept together by surface tension. With such a model the volume is $\propto A$, and the binding energy is also $\propto A$, as observed. The forces are *saturated*. The relation *nuclear volume* $\propto A$ was generally accepted in 1932, when the neutron was discovered.

4.2 The discovery of the neutron

Search for a neutron consisting of an electron and an embedded proton started in the early twenties at the Cavendish laboratory, following a suggestion from Rutherford [6]. In 1929 W. Bothe and H. Becker found in Berlin that a hard radiation was emitted when bombarding beryllium with α-particles [60]. They assumed that this radiation consisted of γ-rays of nuclear origin. I. and F. Joliot observed emission of protons in a cloud chamber when the same radiation fell on hydrogenous substances [61]. They concluded that protons were emitted by Compton effect, confirming the Bothe and Becker hypothesis. This conclusion was received with scepticism at the Cavendish where Chadwick after some work promptly, on February 1932, realized that the radiation seen by Bothe and Becker consisted of neutral particles having mass close to that of the proton coming from a $Be^9(\alpha,n)C^{12}$ reaction [45].

It took quite some time before the idea of a neutron made of a protons and an electron could be dropped down. D.D. Iwanenko [62] was the first to propose that a neutron could be considered as an elementary particle like protons and electrons. Chadwick still believed a neutron embedded in an electron [63].

It is probably not widely known [64] that at a time when the interpretation of the Joliots experiment was not yet clear Majorana immediately realized the truth: *how silly! they have discovered the neutral proton and have not even realized it ....* Majorana immediately started working out a nuclear model made of protons and *neutral protons.* Fermi realized immediately the importance of Majorana's work and offered him to present it at the Paris conference in July 1932. Majorana declined Fermi's offer in a rather curious way [64]. Majorana's work was published much later, when others independently had worked on similar ideas.

4.3 Nuclear forces

In March 1932, just after the discovery of the neutron, the Joliot-Curies report [65] the first pictures, in a cloud chamber, of protons kicked off paraffin, and a few months later Heisenberg [52] makes the first theory of nuclear structure for a nucleus composed of protons and neutrons (avoiding however any consideration on the β-decay and the true nature of the neutron). The nucleus is kept together by *exchange forces*, like Van der Wals forces keep a molecule together. These forces are assumed to be very weak between like particles. This will be shown to be wrong by studies of pp scattering [66,67,68], in 1936 (hinting to charge independence), as was wrong the conclusion that his force gave a saturated structure to α-particles (not to the deuteron as proven later by Majorana [69]). Still Heisenberg work is important as he showed that quantum mechanics could be applied to systems of neutrons and protons, and that *exchange forces* were strong. Heisenberg introduced also the isotopic spin (it will be used by Fermi in his β-decay theory of 1934 and by Yukawa in his meson theory of nuclear forces of 1935) – a first spin-off of the spin!

Several types of exchange forces were then investigated as shown below.

- Heisenberg(1932) – spin and charge exchange,
- Wigner(1932) – no exchange,
- Majorana(1933) – only charge exchange,
- Bartlett(1936) – only spin exchange.

Majorana force was the simplest one to explain saturation. For the rest one had to admit that experimental data only fixed the strength and the range of the nuclear potential but did not tell us anything about its shape. Eventually, ten years later, Wigner and Eisenbud [70] wrote the most general potential consistent with the law of conservation of angular momentum and parity and the requirement of time reversal invariance. The most general potential which can act between identical nucleons and which does not involve the momentum to a power higher than the first is a linear combination of four basic types:

$$V(r) = V_1(r) + V_2(r)\,\boldsymbol{\sigma_1}\cdot\boldsymbol{\sigma_2} + V_{\mathrm{T}}(r)S_{12} + V_{\mathrm{S0}}\,\mathbf{L}\cdot\mathbf{S}$$

a combination of a Wigner force, a spin-dependent central force, a tensor force and a spin-orbit force. In the neutron-proton interaction each of these four types could also be combined with a Majorana exchange operator giving eight possible types altogether. The shell structure of nuclei shows that, if the interaction of a single nucleon with a complex nucleus could be represented by an average potential, then this average potential must have a spin-orbit component. Unfortunately the tensor force can also give a spin-orbit coupling, and we cannot argue back from the shell model potential to estimate the strength of the nucleon-nucleon spin orbit interaction. The most unambiguous evidence for a spin-orbit force comes from the analysis of high energy experiments.

4.4 From photon exchange to meson exchange

Since early in the twenties it had been recognized that photons can be created or destroyed when they interact with matter. Dirac then showed that in the scattering of photons by electrons the incident photon is absorbed by the electron and the scattering photon is created. In 1932 Fermi [71] showed that the Coulomb force between charged particles could be thought as an exchange interaction involving photons. From here in 1933-1934 he showed [42,43,44] that electrons and neutrinos emitted in β-decay could well not exist in the nucleus before the decay, and be created at the instant of the decay, just as in the emission of photons. Tamm and Iwanenko [72] then pointed out that the creation and subsequent annihilation of an electron and a neutrino in the field of a proton and a neutron would lead to an exchange interaction between the nucleons with an exchange potential

$$U(r) \approx g^2/\hbar c r^5$$

where g was Fermi's β-decay coupling constant. The latter however was too small to account for the binding energies of neutrons and protons in the nucleus.

Building up on these ideas Yukawa [73] suggested in 1935 that the field of force responsible for the nuclear interaction is accompanied by a new quantum obeying Bose statistics and having a mass ≈ 200 times the mass of the electron. What about spin? for simplicity it is neglected. However, the spin of the deuteron is determined by the sign of the potential,

$$U(r) = -g^2 exp(-\lambda r)/r.$$

To obtain agreement with the experimental results one needed a value of λ between 10^{-11} and 10^{-12} cm^{-1}, and for g a few times that of the elementary charge e. In 1939 Bhabha [74] named the new quanta *mesons*. And soon Kemmer [75], and Yukawa and Sakata [76] noticed that exchange forces could be made charge independent by introducing neutral mesons with the same mass as the charged meson,

$$U(r) = -g^2 \boldsymbol{\tau_1} \cdot \boldsymbol{\tau_2}\, exp(-\lambda r)/r,$$

with one qualification: the force turns out to be attractive in $T = 1$ states, repulsive in $T = 0$ states and the deuteron would not be bound!

Next step was to consider the dependence of the interaction on the spin and parity of the mesons exchanged. Beside scalar mesons, the original ones with $J^P = 0^+$, one can have pseudoscalar mesons with $J^P = 0^-$, vector mesons with $J^P = 1^-$ and pseudovector mesons with $J^P = 1^+$. In the last three cases the spin does play a role. For example, in the case of pseudoscalar mesons, a virtual meson in the field of a nucleon is in a P-state. This state is not spherically symmetric and its orientation in space is determined by the spin of the nucleon. The interaction between two nucleons depends on the relative orientation of their meson fields, hence it is spin dependent.

The interaction potential is

$$V(r) = 1/3(f^2/4\pi)\hbar c\,\boldsymbol{\tau_1}\cdot\boldsymbol{\tau_2}[\boldsymbol{\sigma_1}\cdot\boldsymbol{\sigma_2} + S_{12}(1 + 3/\mu r + 3/(\mu r)^2]exp(-\mu r)/r,$$

where S_{12} is the tensor force operator. This results in an attractive force in both singlet and triplet spin states as found from experiments [77,78] and a sign consistent with a positive deuteron quadrupole moment [53]. For convenience I summarize here the sequence of the experiments:

- (1934) np bound state (deuteron) has spin one (from d-d band spectra analysis [48]).
- (1936) np unbound state (virtual) has $J = 0$ (from thermal neutron cross-section in hydrogen for n + p $\rightarrow \gamma$ + d – energy dependence indicate a magnetic dipole transition [79]).
- (1937) thermal neutrons (Ra-Be source, cooled in liquid methane) scattering cross-section from ortho- and para-hydrogen (liquid hydrogen)[50,51].
- (1939) deuteron quadrupole magnetic moment [53].

By the end of the thirties the theory had gone far, and more experimental input was badly needed, *but pressure of other work now makes that impossible for some time* [80].

5 Postwar years

The prewar line of experiments of thermal neutrons was continued with new more powerful techniques:

- (1947) thermal neutrons (d from cyclotron on Be) scattering cross-section from ortho- and para-hydrogen (gaseous hydrogen) [81].
- (1948) thermal neutrons (reactor) scattering cross-section from hydrogen atoms coherently oriented in NaH crystals [82].
- (1950) scattering cross-section of neutrons from hydrogenous mirrors [83].
- (1953) same as ref. [81] but target mainly parahydrogen [84]

Methods of polarizing beams [85–90] and targets [91] are considered, as well as double scattering experiments to detect polarization states [92]. Wolfenstein attempts a *theory of proposed reactions involving polarized protons* [93]. The study of spin effects moves from thermal neutrons to high-energy beams [94,95]. At the Summer School of 1954 in Varenna Fermi lectures on experiments with polarized nucleons [96]; another review is given by Wolfenstein [97]. The formalism describing the collisions of particles with spin is formulated for any spin and in a relativistic way [98].

What about pions ? A particle had already been observed in 1937 by Anderson and Neddermayer [99] which had about the mass proposed by Yukawa, but it scattered too little to agree with the coupling constant of the nuclear forces. A great deal of effort was put to explain this discrepancy. Only very slowly the idea of a second meson made its way. The experiment of Conversi, Pancini and Piccioni [100] in the interpretation of Fermi *et al* [101] proved conclusively in 1947 that the muon is not responsible of the nuclear forces.

In the same year 1947 a new candidate, the pion, was observed [102]. Within a few months it was copiously produced by a new generation of accelerators [103]: it had the right mass, it was pseudoscalar, it was neutral or charged. Following a suggestion by Kroll and Rudermann [104] the pion-nucleon coupling constant was measured in a pion photoproduction experiment at threshold to be $f^2/4\pi = 0.073 \pm 0.07$ [105]. It turned out to be the same as that obtained from pion-nucleon scattering experiment [106]. A similar value $f^2/4\pi = 0.080 \pm 0.002$ was obtained from a phase-shift analysis of pp and pn scattering experiments between 217 and 350 MeV [107]. Measurements of $f^2/4\pi$ are also needed to explain the deuteron quadrupole moment and binding energy. In all cases one tests the long range part of the interaction, i.e. the one-pion exchange.

By the end of the fifties there was sufficient experimental information and the theoretical tools were ready to test the nucleon interaction at shorter distances. One way was to exchange more than one pion, or heavier mesons. In 1959, Regge poles came on the stage [108]: the spin acquired a key role. At about the same time one learned how to prepare polarized nucleon targets and polarized nucleon beams, clean and intense.

In 1956 spin proves to be a fundamental tool to understand weak interaction when it is discovered that this interaction does not conserve parity. Helicity and handness (or chirality) are introduced. Nuclei are concerned as well as leptons (electrons, and muons, and the corresponding neutrinos and antineutrinos) and photons. Nearly at the same time, as for strong interaction, it becomes important to consider statistical 'ensembles' of particles in a definite spin state, polarized beams and targets. Electron polarization methods (theory and experiments) are reviewed by Tolhoek [109].

6 Towards modern polarization experiments

6.1 Detection of the azimuthal polarization of the n-p interaction at 150 MeV

A double-scattering experiment carried out at the 184-inch synchro-cyclotron of Berkeley was reported in 1951 to measure the polarization due to non central n-p interaction at high energy [94]. First scattering (pn) takes place inside the machine at variable angles and fixed energy. The neutrons are well collimated by the machine shielding. The protons of the second scattering at 30° (np) are recorded by liquid-scintillator telescopes at four azimuthal angles. A left-right asymmetry of the order of 12% was expected. The overall electronic resolution is 5 ns. The effects observed by Wouters are only a few percent, and within the experimental uncertainties.

In Europe at that time high-energy meant cosmic rays, that do not show prominent spin effects, detected by Geiger counters with μs resolution, or emulsions or cloud chambers. The only attempt I know to measure polarization effects in cosmic rays was by the emulsion group in Rome at the time when parity non-conservation was under scrutiny: they determined the polarization of cosmic-ray muons in emulsions by measuring the angular distribution of the decay electrons with respect to the muon line of flight in the rest system of the pion [110].

6.2 The Rochester experiment – other experiments up to 400 MeV

The first really successful experiment in the 200 MeV region, is that of Oxley *et al*, who worked at Rochester with protons [95,111]. Their experiment is typical of the methods used by later workers, aside from practical modifications rendered necessary by different geometrical conditions in different laboratories [112–116]. Fermi, followed by other authors, gave a simple interpretation of these effects in terms of spin-orbit coupling [117–120].

As mentioned above, nucleon polarization was one of the two topics on which Fermi lectured in Varenna in 1954 [96], the other was pion physics. For two more years an intense activity on polarization took place around different accelerators in the West, mainly Berkeley, Chicago and Harwell, and the possibilities of the field seemed soon exhausted.

6.3 Dubna polarized proton beam at 660 MeV

A jump ahead in the quality of the experimentation was taking place however in USSR where a polarized proton beam was scheduled as early as 1954 for the highest energy syncro-cyclotron in the world [121]. Western physicists learned for the first time about this important progress (and the existence of this accelerator) at the first High Energy Conference organized in Geneva by CERN in 1956 [122]. The use of polarization data in pion physics was also discussed at this conference [123,124]. First results were already available [125]. Nurushev has recently recollected this glorious period [126].

On that July 1956 the magnet of the CERN 600 MeV synchro-cyclotron was on its way to the Meyrin site, and Giancarlo Wick was lecturing on pions and their pseudovector coupling to nucleons in Geneva. Berkeley physicists had just observed the antiprotons produced at the 6 GeV betatron, and were busy with the recently developed bubble chamber: little room was left on their side for spin effects.

The spin came back in a spectacular way by the end of the year with Co^{60} nuclei beautifully aligned in the target of Mrs. Wu, and for a while in weak interaction it was only a question of spin. In this context even the old equations describing the spin motion in a magnetic field and the Thomas precession were refreshed [127]. For a few years the interest for spin effects in strong interaction remained in the shade: physics reasons, but also experimental limitations to do double and triple scattering experiments. Novel experimental methods were needed. They were new especially to conventional particle physicists as most of us were. One of the early experiments at the CERN synchro-cyclotron was a search for parity violation in pion production by polarized neutrons [128].

6.4 Forerunners of polarized targets

Around 1945 two groups working on the East Coast (Harward) and on the West Coast (Stanford) simultaneously detected the NMR (Nuclear Magnetic Resonance) response of protons, in paraffin and water respectively. The two groups used different language to describe their discoveries, and had different instrumentation. The former talked of *absorption by nuclear magnetic moments of long wavelength photons*, the latter of *electromotive force induced in a coil by the forced precession of nuclear magnetization.* The Harward group was lead by Edward Purcell, the Stanford one by Felix Bloch who became in 1954 the first Director General of CERN. He imported a heavy magnet from Stanford, to continue his experiment in Geneva, and even invited as a guest Anatole Abragam to help him. However, no attempt was made to use their expertise for the experiments being planned at that time with a view to the future CERN accelerators. Abragam was mostly known for his work on RPE (Resonance Paramagnetic Electronique) following his stage in Oxford.

6.5 *The CERN project for a polarized proton beam at 600 MeV*

Towards the end of the fifties W. Gentner and G. Bernardini were responsible for the research to be carried out at the CERN 600 MeV synchro-cyclotron while waiting for the 25 GeV Proton Synchrotron. Gentner brought to the lab flavours of physics from Germany: production of polarized protons for subsequent acceleration to the desired energy. No significant proton depolarization was expected when the atoms were ionized by electron collisions or by light, or during acceleration in a nearly constant magnetic field. Energy levels and Zeeman splitting of the hyperfine structure states of atomic hydrogen were already well known. The CERN project was based on work done at Bonn in the early fifties [129,130] and at Erlangen in 1955 [131] adapted to the CERN synchro-cyclotron.

Robert Keller, back to CERN from Pittsburg in 1956 with polarized proton experience, was invited by Gentner to study polarized proton sources. He drafted in 1957 a proposal in one of the first CERN Yellow Reports [132]. He was joined the following year, first by Maria Fidecaro, and then, a few months later, by Louis Dick and by Gregoire Kantardjan as technical assistant. There was a lot of enthusiasm but hardly any experience at all.

The polarized source consisted of a beam of atomic hydrogen and a long tapered magnetic quadrupole outside the synchro-cyclotron. Focusing took place in the initial high field region, separation of the hyperfine component in the weak field towards the far end. The emerging atomic beam was then sent along a radius to a Penning source at the centre of the machine where it was ionized providing polarized protons for acceleration.

Tests of a prototype polarized source were made using a small 5 MeV cyclotron specially built by the group for this purpose by equipping the model magnet of the 600 MeV synchro-cyclotron with *Dees*, RF generator and vacuum chamber. The polarization was measured by scattering 4.4 MeV protons on carbon and was found to be about 50%. The work progressed steadily until the time of the Basel Conference in 1960 where the results were presented [133]. However, the long way to 600 MeV was never covered for different reasons.

It was not the last of these reasons the one that materialised one day in 1960, when Abragam entered the rooms near the SC where all the stuff was sitting - the huge permanent magnet quadrupoles, the 30 cm magnet, the home-made RF generator, the pumps and the Laval nozzle - announcing that he had in Saclay a few cm^2 target with a 6% nucleon polarization.

By the time of the 2nd Int. Symposium on polarization phenomena (Karlsruhe, 1965) the whole apparatus had been dismantled. By the end of the same year a high-energy polarized proton beam had been constructed at the

CERN synchro-cyclotron, by sending the extracted 600 MeV proton beam onto an external target. The scattered beam could be polarized vertically in both directions and had an intensity of $2 \cdot 10^5$ protons per second with a 40% polarization [134]. At the Karlsruhe symposium a number of sources was reported to work at various low energy accelerators [135]. Among these one from Aukland (New Zealand) was noted for its important yield [136]. In the following year results on pion charge exchange came from a polarized target experiment at the CERN Proton Synchrotron, which contradicted expectations from the Regge theory [137].

6.6 The Saclay suggestion to separate hyperfine states

At the time the CERN project was started in 1957 the separation of the hyperfine component by magnetic fields looked by far the simplest solution. A method based on the Lamb shift of the $2S_{1/2}$ and $2P_{1/2}$ levels did not look promising because of intensity problems [138,139]. The use of RF transitions was also suggested [140], but the solution proposed by Abragam and Winter came too late to be considered.

It consisted in the separation of the two electronic states $m_j = \pm 1/2$ in a strong Stern-Gerlach multipole field and the elimination by means of a diaphragm of all atoms in the states 3 and 4 (electron spin down). Thus the beam contained only atoms in states 1 and 2 polarized according to the electronic moment but unpolarized in the proton moment. Abragam and Winter [141] proposed to transfer all atoms in state 2 into state 4 by means of a radiofrequency field $H_1 \cos \omega t$ parallel to a constant field H_0 (adiabatic passage methods). Then the proton spin direction becomes the same for all the atoms. A field H_1 of 1 Gauss was required for a 240 MHz transition frequency. They expected at least a proton polarization of 50%.

6.7 The Basel Conference in 1960

The first conference specialized in polarization phenomena was held in Basel in summer 1960 [142]. W. Paul summarized the status of the polarized proton sources. Keller presented the results from the prototype polarized proton beam at CERN. Projects along the same principles were going on for low energy linear accelerators, at Erlangen (Fleishmann), at Minneapolis (Clausnitzer), at Harwell (Craddock), at Basel (Huber). The Saclay project of a polarized proton source was for a cyclotron and made use of a high frequency transition as suggested by Abragam and Winter [141]. Results on polarized targets were also presented, mainly based on the *solid effect* or *dynamic polarization* (Abragam, Borghini, Proctor) [143,144]. Physics was limited to low-energy reactions.

It should also be recalled that the well known agreement to define the positive polarization of spin $\frac{1}{2}$ particles, the *Basel Convention*, was reached at this conference.

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