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W. Pauli's Scientific Work

Charles P. Enz

'Nur die Fülle führt zur Klarheit
Und im Abgrund wohnt die Wahrheit.'
SCHILLER

CONTENTS

1. PORTRAIT OF A GENIUS[1]*

It was at the turn of the century, indeed in 1900, that Max Planck's idea of the energy quantum was born in Berlin. Also in 1900, on 25 April, Wolfgang Pauli was born in Vienna as the son of Wolfgang Joseph Pauli, a medical doctor, and his wife Bertha Schütz.

The chances for young Pauli to become a physicist were remarkable: Ernst Mach was his godfather whose gift for Wolfgang's christening was a silver cup with the inscription '31 Mai 1900'. Much later Pauli commented on this event in a letter dated 31 March 1953 as follows:[2]

> ... Among my books there is a rather dusty container, in which there is a silver cup in fin de siècle style, and in this cup there lies a card. ... This cup, of course, is a christening cup and on the card is inscribed in old-fashioned flowery type:
>
> 'Dr. E. Mach, Professor an der Universität Wien'.
>
> It so happened that my father was very friendly with his family, and at that time was intellectually entirely under his influence. He (Mach) had thus graciously agreed to assume the role of my godfather....

Pauli's father became an associate professor at the medical school of the University of Vienna in 1907. In 1913, he was the first to lecture on physiological-chemical biology, and in 1922 he became director of the new institute for medical colloid chemistry,

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* This article has two series of references. Superior figures as [2] refer to notes and references listed on pp. 792–796. Figures in square brackets as [2] refer to Pauli's own works which are listed on pp. 796–799.

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a field in which he became well-known by his publication of a number of important works. This was the reason why young Pauli published under the name of Wolfgang Pauli *junior* until his nomination as full professor at ETH (Eidgenössische Technische Hochschule), Zurich, in 1928.

Although in later years Pauli sometimes used, half jokingly, harsh words like 'geistige Einöde' [spiritual desert][3] for his home town, Vienna was, at the time of Pauli's high school years, a remarkable place. In fact, Pauli's class, which graduated from Döblinger Gymnasium's humanistic section in 1918, has gone down in local history as the 'class of the geniuses'.[4] This class of 27 boys counted among its numbers two later Nobel prize winners (the second, Richard Kuhn, got the chemistry prize for 1938), two famous actors (one was Hans Thimig from the famous Viennese family of actors), three University professors, two directors of medical schools, a music historian, a politician and several industrialists. The graduation group picture,[4] however, does not show the whole class because some of the boys has already joined the Army; World War I was still haunting Europe.

But this did not prevent the class of geniuses from having its own kind of fun with school. Very typically, at the occasion of Empress Zita's birthday Pauli asked the family's girl cook to write a letter of vigorous protest to the school director complaining that Pauli's entire class was planning to demonstratively read *Die Arbeiter Zeitung* during the anniversary mass for the Empress.[5]

During the same year 1918, however, Pauli immediately attained full respectability by the submission on 22 September, still from Vienna, of his first published paper [1], on the energy components of the gravitational field. Eight months later, in his second paper [2], submitted on 4 June 1919 from Sommerfeld's institute in Munich, Pauli already acknowledges a correspondence with Hermann Weyl whose theory of gravitation was the subject of this note. The critical self-assurance for which the young Pauli became so well-known and which earned him the name of God's whip ('die Geissel Gottes') from Ehrenfest[6] is still restrained in the second paper. Indeed, in a footnote Pauli points out an error of sign in Weyl's calculation with the words: '... I should like to express, with due respect, the opinion that a small oversight has occurred in Weyl's paper.'

Algebraic signs haunted Pauli no less than any average physicist. Much later he would stand in front of the big blackboard in the lecture room 6c of the physics institute of the ETH nodding his head and murmuring 'Vorzeichen, Vorzeichen' [the signs, the signs!], while a damped noise would signal that the class had taken the opportunity to relax.

Pauli's third paper [3], submitted on 3 November 1919, again discusses the consequences of Weyl's theory and ends with a critical remark of more fundamental significance. Formulated in the best of Pauli's characterisic style, mature, precise and cautious, he observes that while Weyl's theory continually operates with the field strength in the interior of the electron it is only the force on a test particle which for the physicist is well-defined. And since there is no smaller test particle than the electron itself the notion of the electric field strength in a mathematical point seems to be an empty fiction.

And the 19-year-old Pauli continues: 'One would, of course, like to require that essentially only observable quantities be introduced into physics.' In this credo that physics should be formulated entirely in terms of observable quantities, the influence of Pauli's godfather Ernst Mach is undeniable. This credo arose to its full significance later on in the elaboration and interpretation of the new quantum theory (see Section 3). It conflicted, on the other hand, with the difficulties of renormalization theory of fields (see Section 4). With regard to the latter problem the closing sentence of Pauli's third paper [3] raised a question which is still unanswered:

> Sollten wir überhaupt mit den Kontinuumstheorien für das Feld im Innern des Elektrons auf einer falschen Fährte sein?

['Is it possible that we are on the wrong track with the continuum theories for the field in the interior of the electron?'] The problem of the notion of the field and the atomicity of the electric charge bothered Pauli during all his scientific life, as will be seen in Section 7.

These first three short papers of Pauli on relativity and gravitation stand isolated; it was only thirteen years later that he came back to this field of research. However, as is well-known, this activity of Pauli culminated in a momentous work, the famous chapter on relativity in *Encyklopädie der mathematischen Wissenschaften* [4] of which Einstein wrote:[7]

> No one studying this mature, grandly conceived work would believe that the author is a man of twenty-one. One wonders what to admire most, the psychological understanding for the development of ideas, the sureness of mathematical deduction, the profound physical insight, the capacity for lucid, systematical presentation, the knowledge of the literature, the complete treatment of the subject matter [or] the sureness of critical appraisal.

Although it is difficult to improve upon Einstein's eulogy of Pauli's essay on relativity, Pauli's own critical appraisal of the problem of charged particles deserves special mention. In fact in the last Section (Section 67 of ref. [4]) Pauli comes back to the questions raised in his third paper [3] concerning the concept of the electric field strength and the atomicity of electric charge: No modification of his earlier judgement was necessary.

The encyclopaedia article has withstood the test of 35 years of research so brilliantly that an English translation, updated with 'Supplementary Notes by the Author' written in 1956 [5], appeared in 1958, the year of Pauli's death. These short supplementary notes deal with some of the later developments of the theory but give in no way a complete picture. The most important omission is the problem of relativistic quantum theory, in particular field theory, to which Pauli has significantly contributed himself (see Section 4) and which had grown to proportions beyond the scope of such supplementary notes.

Of particular interest are the supplementary notes 7 and 23. In note 7 Pauli corrected his earlier omission of the Bianchi identities. In a conversation with J. Mehra in spring 1958 Pauli commented on this omission when Mehra observed to him that the original article was already so perfect that it was surprising that he should have thought of adding supplementary notes. He replied with a wide grin, 'You know, it was not all that perfect. I had not even mentioned the Bianchi identities.'[8]

In note 23, concerning unified field theories, Pauli comments on his earlier discussion of the problem of charged particles in the following words: 'The reader of the original text of Section 67 will see that I was already at that time very doubtful regarding the possibility of explaining the atomism of matter, and particularly of electric charge, with the help of classical concepts of continuous fields alone.'

Apart from this general introduction, note 23 discusses two types of attempts at unification of fields, namely theories with unsymmetrical metric tensor and five-dimensional theories. The latter are of particular interest here because Pauli's most important research papers in general relativity deal with the 'formulation of the laws of nature in five homogeneous coordinates' [6]. In the first of these two papers published in 1933, 'Pauli gave a beautiful account of this projective geometry and its tensor analysis, which were developed from first principles and he formulated the Einstein–Maxwell equations in projective coordinates.'[9] The second paper 'dealt with the incorporation of spinors and of Dirac's equation into this geometrical structure'.[9] Bargmann comments on it: 'In my opinion, this is by far the most satisfactory exposition of spinors in general relativity – quite independent of the problem of a unified field theory.'[9] For the sake of completeness it should be mentioned that Pauli had treated the problem of spinors and the formulation of the Dirac equation in 5 dimensions in two earlier papers published in 1932 with Solomon [7].

A particular result obtained in the second paper [6] concerns an additional term in the field equation, describing an anomalous magnetic moment which, however, is too small to be observed (except, perhaps, through its influence on terrestrial magnetism, as Pauli remarks at the end of this paper). In a recent analysis of the problem, Thirring [10] has shown that there is in addition a non-vanishing, although very small, electric dipole moment due to a very peculiar violation of parity and charge conjugation in this theory.

The number of research papers in general relativity by Pauli is not large. Apart from the already mentioned publications, there is only one more, written with Einstein in Princeton in 1943, 'on the non-existence of regular stationary solutions of relativistic field equations' [8]. But Pauli always took an active interest in this field, as is evident from his contribution as president of the conference on 'Fünfzig Jahre Relativitätstheorie' [9] which was held in Berne in 1955 shortly after Einstein's death.

2. THE OLD QUANTUM THEORY. SPIN AND EXCLUSION PRINCIPLE

Pauli studied six semesters at the University of Munich where his teacher in theoretical physics was Arnold Sommerfeld. In June 1921 he obtained his doctorate with a dissertation on a particular molecular model.[11] His doctoral diploma is dated 25 July 1921 and carries the mention 'summa cum laude'.[12] His dissertation, an improved and extended version of which was published [10], is Pauli's first contribution to the old quantum theory. The difficulty of this theory was that its rules were applicable only to conditionally periodic systems which excluded systems with more than one electron. The next more complicated one-electron system beyond hydrogen-like atoms is the hydrogen molecule ion which was the subject of this work.

In the winter semester of 1921/22 Pauli was Max Born's assistant in Göttingen. During this time Born and Pauli collaborated on a paper [11] on the systematic application of astronomical perturbation theory to atomic physics.[11] A short review on perturbation theory written by Pauli appeared two years later in the *Physikalisches Handwörterbuch* [12]. Pauli spent the following summer semester in Hamburg as an assistant of W. Lenz whom he had met in Munich, where Lenz had also worked with Sommerfeld.[11]

In Göttingen, Pauli had met Niels Bohr who asked him to come to Copenhagen for one year. Pauli was much surprised by this offer, and answered 'with that certainty of which only a young man is capable: "I hardly think that the scientific demands which you will make on me will cause me any difficulty, but the learning of a foreign tongue like Danish far exceeds my abilities" ... and I went to Copenhagen in the fall of 1922, where both of my contentions were shown to be wrong' [13].

After a first paper written in Copenhagen in collaboration with H. A. Kramers on the theory of band spectra [14] Pauli concentrated his efforts on the problem of the anomalous Zeeman effect. This problem appeared to him very unapproachable. When a colleague who met him strolling in Copenhagen observed that he looked unhappy Pauli answered: 'How can one look happy when he is thinking about the anomalous Zeeman effect' [13].

Pauli's first paper on the anomalous Zeeman effect [15] analyzed the multiplets in the case of strong fields that Landé had investigated for weak magnetic fields H.[13] From this analysis Pauli was able to rederive Landé's splitting factor g by using the rule that 'the sum of the energies of all states of a multiplet belonging to a given value of M remains a linear function of H, when we pass from weak to strong fields'.[14] In a second paper [16], submitted from Hamburg where he had returned in autumn 1923 as an assistant of Lenz, Pauli showed that Landé's association of the quantum numbers in the cases of weak and strong fields[15] (which are adiabatically connected) could be derived from the rules of the old quantum theory.

The third paper on the anomalous Zeeman effect [17], submitted on 2 December 1924, is of fundamental importance because it introduces, still in disguised form, the spin quantum number of the electron. Pauli starts by arguing that the then widely accepted hypothesis that closed atomic shells (in particular the K-shell) carry magnetic moment and angular momentum has to be rejected for several different reasons. The main reason was that the relativistic velocity-dependence of the electron mass leads to a dependence of the gyromagnetic ratio of the K-shell on the atomic number Z, and hence to a Z-dependence of the Zeeman-splitting, contrary to observation. Pauli then draws the remarkable conclusion that the magneto-mechanical anomaly must be due to a 'strange two-valuedness' of the quantum-theoretic properties of the outer electron which is not describable classically. The original wording of this conclusion is the following (Ref. [17], p. 385):

Die abgeschlossenen Elektronenkonfigurationen sollen nichts zum magnetischen Moment und zum Impulsmoment des Atoms beitragen. Insbesondere werden bei den Alkalien die Impulswerte des Atoms und seine Energieänderungen in einem äusseren Magnetfeld im wesentlichen als eine alleinige Wirkung des Leuchtelektrons angesehen, das auch als der Sitz der magneto-mechanischen Anomalie betrachtet wir. Die Dublettstruktur der Alkalispektren, sowie die Durchbrechung des Larmortheorems kommt gemäss diesem Standpunkt durch eine eigentümliche, klassisch nicht beschreibbare Art von Zweideutigkeit der quantentheoretischen Eigenschaften des Leuchtelektrons zustande.

The remarkable feature of this conclusion is that only four months before, in a paper submitted on 17 August 1924 [18], Pauli had proposed the hypothesis that 'in general the nucleus must possess a non-vanishing resulting angular momentum.' The association of the electron spin to this 'strange two-valuedness' thus seems compelling. Pauli explains the reasons for his hesitation in his Nobel Prize Lecture of 1946 [19] as follows: 'Although at first I strongly doubted the correctness of this idea because of its classical mechanical character, I was finally converted to it by *Thomas*' calculation[16] on the magnitude of doublet splitting.' But he continues: 'On the other hand, my earlier doubts as well as the cautious expression: "classically non-describable two-valuedness" experienced a certain verification during later developments.'

It is interesting to note that there exists a completely forgotten footnote by Pauli which explains more closely Pauli's difficulty with the 'classical mechanical character' mentioned above.* This footnote, written in 1928, in fact, fully elucidates the reason why Pauli could at the same time propose the idea of the nuclear spin and reject the apparently identical idea of the electron spin. This reason is that due to the small mass of the electron the peripheral velocities of the spinning electron are much larger than the light velocity c, while nuclear masses are sufficiently large to make the peripheral velocities of rotating nuclei much smaller than c.[17] This reference is a footnote on p. 1794 of Ref. [26] (see Section 3) and reads:

In einer mehr die kinematischen Verhältnisse ins Auge fassenden Weise wird auch von einem 'rotierenden Elektron' (englisch 'spin-elektron') gesprochen. Die Vorstellung eines rotierenden materiellen Gebildes halten wir aber nicht für wesentlich und sie empfiehlt sich auch nicht wegen der Ueberlichtgeschwindigkeiten, die man dann mit in Kauf nehmen muss...

The subtle history of the electron spin has been recounted elsewhere.[18] So it is sufficient to record here the following two episodes concerning the history of nuclear spin. Sommerfeld's reaction to Pauli's proposal [18] was one of disappointment over its classical nature which he expressed on a postcard to Pauli.[19] The second episode is recounted by S. A. Goudsmit,[20] who writes: 'For a number of years, whenever I met Pauli, he would remark cryptically that he "could afford not to be quoted". It was only in the later thirties that I found out to what he referred.'

Pauli's work on the anomalous Zeeman effect culminated in the famous paper enunciating the exclusion principle [20], submitted on 16 January 1925. In an address given at a dinner in honour of Pauli's award of the Nobel prize held at the Institute for Advanced Study in Princeton on 10 December 1945 [13] Pauli said: 'The history of the discovery of the exclusion principle, for which I have received the honour of the Nobel Prize award this year, goes back to my student days in Munich.' And he continues

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*See also Pauli's letters to Bohr of 30 December 1925 and of 5 February 1926, Pauli-Correspondence, letters [114] and [119] (Springer, New York, 1979), Vol. 1.

later on: 'The series of whole numbers 2, 8, 18, 32, ..., giving the lengths of the periods in the natural system of chemical elements, was zealously discussed in Munich, including the remark of the Swedish physicist, Rydberg[21], that these numbers are of the simple form $2n^2$ if n takes on all integer values.' In his Rydberg Centennial lecture of 1955 [21] Pauli says, regarding this formula: 'This is the famous formula $2p^2$ (p integer) which Sommerfeld called "cabbalistic" in his book *Atombau und Spektrallinien* and which impressed me very much as a student.' These periods in the system of chemical elements obviously intrigued Pauli very much and in 1923 he gave his inaugural lecture as Privatdozent at the University of Hamburg on this subject (see Ref. [19], p. 133).

Of course, Rydberg's formula was immediately evident once the exclusion principle was enunciated. In the original paper [20] the latter has the following content:

> Es kann niemals zwei oder mehrere äquivalente Elektronen im Atom geben, für welche in starken Feldern die Werte aller Quantenzahlen n, k_1, k_2, m_1 (oder, was dasselbe ist, n, k_1, m_1, m_2)[22] übereinstimmen. Ist ein Elektron im Atom vorhanden, für das diese Quantenzahlen (im äusseren Felde) bestimmte Werte haben, so ist dieser Zustand 'besetzt'.

Admitting that he cannot give a closer justification of this rule Pauli then goes on to discuss the consequences. And he notes:

> Zunächst sehen wir, dass das Resultat von Stoner und damit die Periodenlängen 2, 8, 18, 32, ... im natürlichen System in unserer Regel unmittelbar enthalten sind.

Here reference is made to a paper by E. C. Stoner[23] which, in Pauli's own opinion, was an important step towards the discovery of the exclusion principle. He formulates Stoner's main observation as follows (Ref. [19], p. 133):

> For a given value of the principal quantum number is the number of energy levels of a single electron in the alkali metal spectra in an external magnetic field the same as the number of electrons in the closed shell of the rare gases which corresponds to this principal quantum number.

The divining of the exclusion principle thus appears as a magic act, an instant vision of things falling into place as in a phase transition. And one understands the fascination Pauli had for the act of creation in science, which he analyzed much later in his life in the example of Johannes Kepler (see Section 7).

3. THE NEW QUANTUM THEORY AND ITS INTERPRETATION

In spring 1925 Heisenberg tried by Fourier analysis to arrive at intensity formulas for the hydrogen spectrum, in the hope of being able to guess the correct quantum mechanical intensity relations. However, the Kepler problem (of the hydrogen atom) turned out to be too difficult for this task. So Heisenberg applied his ideas to the anharmonic oscillator, the result of which he communicated to Pauli in a letter on 24 June 1925. In view of the credo mentioned earlier in relation with Pauli's third paper [3], the following introduction in this letter is of particular interest:[24] 'The principle is: In the calculation of any quantities such as energy, frequency etc, only relations among essentially observable quantities should occur.' This principle, Heisenberg believed, was the basis of Einstein's relativity theory which is also recognized as the origin of the credo in Pauli's third paper [3]. Einstein, however, thought that this principle was wrong, even if he had applied it himself. His argument was that only theory decides what can be observed.[25]

The mathematical formulation of the new theory by Heisenberg[26], by Born and Jordan[27], by Dirac[28] and by Born, Jordan and Heisenberg[29] followed almost immediately. Already in October, Pauli surprised Heisenberg with the complete quantum mechanical solution of the hydrogen atom. Heisenberg writes in a letter to Pauli on 3 November 1925:[30] 'I clearly do not have to write you how glad I am about the new theory of the hydrogen atom and how much I admire that you have brought forth this theory so quickly.'

The work on the anomalous Zeeman effect described in the previous section made use of only modest mathematical tools. However it required all the logical and analytical powers of Pauli's genius. On the other hand, the paper on the hydrogen spectrum, from the point of view of the new quantum mechanics [22], required considerable mathematical intuition. The key to the solution of this purely algebraic problem was the Lenz vector[31] which is a constant of the motion of the Kepler problem. By eliminating the coordinates of the electron Pauli arrives at four algebraic equations relating the components of the Lenz vector and those of the angular momentum vector **l**. Choosing a representation where l_z and $\mathbf{l}^2$ are diagonal he then obtains the Balmer formula.

In March 1926 there appeared Schrödinger's first communication on 'Quantisierung als Eigenwertproblem'.[32] In a letter to Jordan, dated 12 April 1926, Pauli comments on this work[33]: 'I feel that this paper is to be counted among the most important recent publications.' And he immediately goes on to show the equivalence of Schrödinger's approach with the 'Göttingen Mechanics'.[33] This major contribution of Pauli to the development of the new quantum mechanics has been brought to the attention of the physics community by B. L. van der Waerden only in September 1972.[33] It had not been published by Pauli because Schrödinger had submitted the proof of the equivalence on 18 March 1926[34], and therefore had the priority. But Pauli must have attached some importance to his independent proof since, contrary to his habit of writing his correspondence by hand, this letter is typed, and Pauli kept a signed carbon copy in a special envelope.

While the electron spin was still missing in the paper on the spectrum of the hydrogen atom (Pauli's acceptance of the spin idea occurred in March 1926, and this paper was submitted on 17 January of this year), Pauli developed the non-relativistic quantum mechanics of the magnetic electron in his second published contribution to the new quantum theory [23], submitted on 3 May 1927.

The electron spin had been discussed in matrix mechanics earlier by Heisenberg and Jordan[35] in relation to the anomalous Zeeman effect, but its integration into the Schrödinger equation met formal difficulties. They stemmed from the fact that if the wave function ψ would depend on the angle φ, which is canonically conjugate to the z-component s_z of the spin, then, due to the 'strange two-valuedness' $s_z = \pm\frac{1}{2}$, ψ would have to change sign under rotation of φ by 2π. The simple way out proposed by Pauli was to let ψ depend on s_z itself, instead of on φ. This immediately leads to a two-component wave function and to the well-known Pauli spin matrices. Pauli emphasized that his theory was to be considered as provisional since the final theory should be relativistically invariant. He could not, at that moment, realize the important role his theory would play less than a year later when Dirac derived this final theory.[36]

This was the year when Pauli came to ETH with Paul Scherrer, the experimentalist, to succeed Peter Debye. About the same time Hermann Weyl left ETH and Schrödinger left the University of Zurich. The departure of these three eminent scholars was a painful loss for Zurich. Schrödinger was succeeded by Gregor Wentzel who, together with Pauli, represented the young generation. These two brilliant young professors brought to Zurich a fresh activity in theoretical physics for which the common seminar was the adequate platform for many years. From the beginning Pauli had at his disposal at ETH a postdoctorate assistantship which became the point of departure for many brilliant careers.

Besides the published contributions to the building of the new quantum mechanics mentioned earlier, Pauli took part with Heisenberg, Bohr, Born and others in leading the 'Aufbruch in das neue Land'.[37] The clarification of the logical and epistemological content of the new theory, with its entirely new notions of intrinsic uncertainty and complementarity, was a more strenuous march of the mind than can be realized in looking back. The last journey was covered at the 5th Solvay Conference in Brussels in the autumn of 1927, which was dominated by the discussions between Bohr and Einstein, in the outcome of which Pauli had an essential part.[38]

It was also Pauli who in later years gave the most accurate formulation of the controversy between the desire 'to complete quantum mechanics in a way so as to make it into a deterministic scheme with the aid of hidden parameters'[39], and 'the interpretation of quantum mechanics based on the idea of the complementarity'.[39] He made it clear that the given physical reasons which 'have nothing to do with philosophical prejudices'[39], led him to consider the second point of view of the above alternative as the only possible one [24]. He also clarified the fruitless discussion between Born and Einstein on the same subject in three letters to Max Born dated 3 and 31 March and 15 April 1954.[40]

There exist three extensive review articles on quantum theory by Pauli, two of which had the misfortune to have been written, essentially, in 1925. Consequently they soon became obsolete and are therefore not widely known. The first, 'Quantentheorie' [25] appeared in 1926 in Geiger and Scheel's *Handbuch*; the second, 'Allgemeine Grundlagen der Quantentheorie des Atombaues' [26] was part of Müller and Pouillet's treatise of 1929. While in the first, references to the electron spin are given in footnotes added in proof, the second contains an addendum (Nachtrag) on its consequences [where the footnote quoted in the previous Section can also be found]. It is a pity to see the enormous work of detail, so characteristic of all of Pauli's reviews, wasted in these treatises.

The third review of 1933, 'Die allgemeinen Prinzipien der Wellenmechanik' [27], which also appeared in Geiger and Scheel's *Handbuch*, however, competes in fame and durability with the review on relativity [4] of twelve years before. In fact, it was reprinted in somewhat shortened form in the same year 1958 as the English translation of the relativity [5] in Flügge's *Handbuch* [28]. The deleted sections had to do with the radiation field (Sections 6 to 8 of Part B of Ref. [27]), the theory of which had in the meantime grown into an enormous literature as will be seen in the following Section.

In the reprinted part, a footnote referring to the parity-violating neutrino (see Section 6 below) has been added (apart from a slight modification) in relation to Weyl's two-component equation (Ref. [28], p. 150). Also the discussion of the states of negative energy in Dirac's theory (Section 5 of Part B of Ref. [27]) has been modified and considerably shortened. This of course is due to the fact that at the time this review was written the only known particles were the electron and the proton (the positron was discovered in 1932, see note 47 below). As a consequence there was an asymmetry between positive and negative charge which had bothered Pauli already in his third paper [3].

In view of the fact that Pauli's *Handbuch* article on wave mechanics [27] was written at ETH in Zurich, it is astonishing that there did not exist a course on wave mechanics in the physics curriculum of ETH until 1956. Clearly, Pauli gave courses on selected topics of quantum mechanics, but it was only in the winter semester of 1956/57 that he started giving a regular course on the subject [29]. These lectures were marked by Pauli's liking for 19th century mathematics symbolized by Whittaker and Watson's well-known treatise[41], which gave his 'Wellenmechanik' the particular touch of solid handicraft. Pauli had inherited this love for wanderings in the complex plane from his teacher Arnold Sommerfeld. In fact, in his obituary note for Sommerfeld [30], Pauli writes: 'After the discovery of the new quantum mechanics (1927), Sommerfeld could now usefully employ in the theory of atomic structure his old mastery of the mathematics of wave theory in the byways of the complex plane so familiar to him.' On the occasion of Sommerfeld's 70th birthday in 1938 Pauli walked these 'byways in the complex plane' in a paper on the problem of light diffraction by a wedge [31], deriving new asymptotic formulas for Sommerfeld's exact solution.

4. QUANTUM FIELD THEORY. SPIN AND STATISTICS

From the moment that quantum field theory came into existence it became Pauli's main concern for the rest of his life. With respect to his own work quantum field theory started with Dirac's two papers of 1927[42] on the interaction of radiation with atomic matter. In these papers only the transverse (radiative) part of the electromagnetic field is quantized while the Coulomb part is included in the Hamiltonian of the material system, thus violating Lorentz invariance. This problem motivated the work of Jordan and Pauli [32] which develops a relativistically invariant form of quantum electrodynamics. Although it was a preliminary step since it treated only the free field case this work paved the way of a proper formulation of quantum field theory by introducing the famous 'invariant delta function'.

In the two important papers by Heisenberg and Pauli [33], which were the first ones that Pauli submitted from ETH in Zurich in 1928, the main aim was again quantum electrodynamics. But the canonical quantization method developed in the first of the two papers is much more general and has become a standard technique in quantum field theory.

The hard problem in the general part of this paper was the proof of the relativistic invariance of the canonical commutation relations, with regard to which Pauli used to say: 'Ich warne Neugierige'.[43] In fact, the commutation relations contain the two field quantities at equal time, and to obtain these relations for arbitrary space-times separation the canonical field equations have to be used.

The application of this general formalism to electrodynamics given in the second part of the first paper, and in the second paper, lead to difficulties related to the vanishing photon mass, that is, with the gauge group. While in the first paper these difficulties were overcome by adding a term to the Lagrangian density, the second paper was formulated in the radiation gauge in which the scalar potential vanishes. This choice of the gauge gives rise to a condition on the state vectors resembling the treatment in Fermi's paper[44] which had just appeared and is therefore mentioned only in a footnote of the second paper.

The matter which interacts with the electromagnetic field is taken, in both papers, to be the then known charged particles, electrons and protons. But in the first paper both Fermi and Bose statistics are discussed, the conclusion being that both statistics work. This is the first hint to the question of the spin-statistics relation which will be discussed below.

These early efforts in quantum electrodynamics and, in particular, the 'ultraviolet' divergence problem were reviewed in the discarded sections of the *Handbuch* article of 1933 [27], mentioned earlier. In the last of these discarded sections, Pauli also comments again on the questions raised in his third paper [3] concerning the concept of the electric field strength and the atomicity of electric charge.

Another question of historical interest discussed in the first of the discarded sections of Ref. [27] had to do with zero-point energy. Pauli made a distinction between the zero-point energy of material oscillators and of radiation, thus repeating an opinion already expressed in his paper on paramagnetism of 1927 [61] (see Section 5), namely 'that material systems (e.g. crystal lattices) quite generally are distinct from the radiation with respect to the occurrence of a zero-point energy'.

From his discussions with Otto Stern during his Hamburg years, Pauli knew that the zero-point energy of material oscillators was important for the understanding of the separation of isotopes.[45] On the other hand, he also knew from a calculation made in early years that the zero-point energy of radiation would have an unreasonably large gravitational effect.[45] Hence it must be discarded, thus freeing quantum electrodynamics from its most trivial divergence problem.

Another divergence problem that turned up in 1937 through the investigation of Bloch and Nordsieck[46] was the 'infrared' one which has to do with the soft photons emitted in Bremsstrahlung. Pauli and Fierz [34] reconsidered the problem in 1938 in order to see whether the logarithmic divergence of the integrated energy loss of a charged particle in a weak external field obtained by Bloch and Nordsieck was due only to inacceptable mathematical approximations or whether it concerned a deeper physical difficulty. In giving the particle a finite extension, restricting its velocity to non-relativistic values, and neglecting the recoil by photon emission Pauli and Fierz came to the conclusion that the infrared divergence was indeed an inherent difficulty of quantum electrodynamics. But the sensitive dependence of their result on the extension of the particle left Pauli somewhat pessimistic. Too pessimistic, indeed since this divergence problem is a fact with which the physicists around accelerators have become familiar with and which does not have the fundamental aspect of its 'ultraviolet' counterpart.

Anderson's discovery of the positron in 1932[47], which confirmed Dirac's reinterpretation of the states of negative energy[48], was for Pauli the point of departure into a new direction of research in quantum field theory. It eventually culminated in the important spin-statistics relationship. Curiously, the spin-$\frac{1}{2}$ positron first led Pauli to consider the spin-0 field, in a paper written with Weisskopf [35]. This is not so surprising however, since the main point of this paper was to show that the scalar relativistic wave equation of Gordon[49] and Klein[50] also admits particles of opposite charge and identical mass, thus giving rise to pair creation and annihilation and to vacuum polarization. This result had been obtained by applying the canonical quantization method of Heisenberg and Pauli [33].

The Pauli–Weisskopf paper was published in 1934, the year of Pauli's marriage to Miss Franca Bertram, who became Pauli's devoted and understanding spouse for the rest of his life.

In the paper of Pauli and Weisskopf [35], and even more explicitly in the detailed account that Pauli gave of this work in the *Annales de l'Institut Henri Poincaré* in 1936 [36], the question whether the spin-zero field could be quantized according to the exclusion principle (anti-commutators) is raised. The answer, which is explicitly derived in Ref. [36], is that in this case it is impossible to satisfy simultaneously the relativistic invariance of the theory and the condition that the charge density commutes in different points of space (or, more generally, for space-like separations). This was the third example of the general spin-statistics relationship. The first, obviously, was that of the photons which are spin-1 bosons, and the second that of the spin-$\frac{1}{2}$ particles of Dirac whose reinterpretation of the negative energy states was only possible with the exclusion principle.

The next important step was to establish the theory of free fields of arbitrary spin. After a first attempt by Dirac[51], this was, with the acknowledged guidance of Pauli, the work of Fierz in 1939[52] who at that time was Pauli's assistant. Fierz's result was that classically the energy is positive for integral spin and indefinite for half-integral spin. Quantization then was possible according to Bose statistics in the first case and according to Fermi statistics (exclusion principle) in the second.

Pauli, in his famous spin-statistics paper [37] submitted on 19 August 1940 from Princeton, where he had gone shortly before to escape the menace of Hitlerism, could build on Fierz's result as well as on those of two earlier publications, one with Fierz [38], the other with Belinfante [39]. He first gives a general 'proof of the indefinite character of the charge in case of integral, and of the energy in case of half-integral spin' (Ref. [37], Section 3). Then making use of the postulate 'that all physical quantities at finite distances exterior to the light cone ... are commutable' (Ref. [37], p. 721), he proves that 'for integral spin the quantization according to the exclusion principle is not possible' (Ref. [37], p. 722). Finally he remarks that quantization according to Bose statistics would, for half-integral spins, leave the energy indefinite which is unacceptable.

For this analysis Pauli had developed new mathematical methods concerning the classification of spinors of arbitary rank.[53] In later papers [40], [41], [42], he reviewed the particular cases of spin 0, $\frac{1}{2}$ and 1 with more conventional methods. Ref. [40] also gives the interaction describing an anomalous magnetic moment which has become known as the 'Pauli term'.

The appearance on the list of elementary particles of the mu-meson, discovered in 1937[54] and mistaken to be Yukawa's particle of the nuclear forces[55], gave field quantization a new impulse. Pauli took an active part in research on the meson theory of nuclear forces during the period until 1946 which he spent at the Institute for Advanced Study in Princeton. He published several papers, in collaboration with members of the Institute, on the strong coupling approximation. These were the papers with Dancoff [43] on the pseudoscalar case, with Kusaka [44] on the mixed pseudoscalar–vector case, and with Hu [45] on the scalar- and vector-pair theory. In autumn 1944 Pauli gave a series of lectures on meson theory and nuclear forces at MIT which subsequently were published in book form [46].

While this clearly indicates Pauli's strong interest in meson theory and nuclear forces, it was always the field-theoretic aspect, and not nuclear physics, that attracted him to this domain of research. Characteristically it was Pauli [47] who applied to meson theory the field-theoretic device of the 'λ-limiting process' of Wentzel[56] and Dirac[57], which will be discussed below in relation to an indefinite metric in Hilbert space and with Ref. [41]. This, however, was a weak coupling approximation.

However, all these efforts left Pauli somewhat dissatisfied, since on the one hand the strong coupling theories gave quantitatively wrong results for the magnetic moments of proton and neutron [43] and of the deuteron [44] and led to unstable nuclei of high charge [44] or produced an unsatisfactory neutron–proton interaction for the deuteron [45]. The weak coupling approximation, on the other hand, had the deficiency 'that if the perturbation method (development in powers of the coupling constant) is valid and if the radius a of the nucleon is supposed to be smaller than the range of the resulting nuclear forces, the coupling constant must be so small that the nuclear interaction becomes much smaller than the empirical one' (Ref. [45], p. 267).

The finite radius a of the nucleon is a characteristic feature of all these theories [43], [44], [45], [47]. More generally, the heavy particle is described by a non-relativistic source function or form factor, as was the charged particle in the work of Pauli and Fierz [34], and also like the heavy particle in Källen and Pauli's treatment of the Lee model [52] to be discussed below. In subsequent years many attempts have been made to find a relativistic generalization of the cut-off method.[58] This led to the so-called non-local field theories in which the fields are coupled at different space-time points.

Pauli took up this problem, previously discussed by C. Bloch[59] and by Kristensen and Møller[60], in a paper published in 1953 [48]. According to Pauli,

> The characteristic difference between local and non-local lorentzinvariant field theories is the fact that in the latter *it is not any longer possible to define field quantities (observables) which commute with each other simultaneously in all space-like pairs of points.* Therefore, in any case, non-local theories have to be considered as an enrichment of the known mathematical possibilities for quantized lorentz-invariant field theories (Ref. [48], p. 650).

The last sentence is somewhat surprising in view of the fact that commutativity of observables for space-like separation, that is, micro-causality, was a key postulate for Pauli's spin-statistics relationship [37]. But it may, in a very concealed fashion, reflect the hope that this might be a possible way to answer one day his ever-present question concerning the concept of the electric field strength and the atomicity of electric charge, raised in his third paper [3]. That such a hope (if it existed at all) was far from realistic is evident from the criticism Pauli voices in Footnote 8 where he writes with regard to Bloch's form factors[59] that 'such form factors will, however, in general lead to a wrong time order of processes (acausality) even for macroscopic distances'. This violation of macro-causality is indeed a serious defect.

The year 1948 brought forth renewed endeavours in quantum field theory through the famous works of Tomonaga[61], Schwinger[62], Feynman[63] and Dyson[64]. Back at ETH Pauli joined this activity, supported by the younger people around him: Villars, Jost, then his assistant, Schafroth, succeeding Jost as assistant, and the visitors Rayski, Luttinger and Glauber. The Monday afternoon theoretical seminar in Zurich, at which Heitler and his associates from Zurich University, Fierz from Basel, and even Stueckelberg from faraway Geneva participated, was an event which attracted many prominent guests. Former Pauli assistants Kronig, Bloch, Peierls, Kemmer, Casimir, Weisskopf used to stop by. This activity is reflected in the notes on 'Feldquantisierung' [49] based on lectures given by Pauli in 1950/51 at ETH and which were widely used by the experts. The situation could not be better described than in the words of Villars[65]:

> Pauli's attitude with respect to the possibilities opened up by the new approach was characteristically one of critical optimism. His criticism was primarily focused on the claim of the unambiguity of the physical predictions of the theory (after isolation of the infinities); his optimism and vivid interest due to the hope that something might actually be learned from facing the remaining difficulties rather than by claiming total success before it was actually achieved.

The problem of the 'unambiguity of the physical predictions of the theory' mentioned above was systematically analyzed in the paper by Pauli and Villars [50] on invariant regularization. In this 'formalistic', as opposed to the 'realistic' procedure (Ref. [50], p.435), the invariant Green's functions, which depend on a single mass, were replaced by modified functions depending on a mass distribution ϱ. Regularization was achieved by the condition that the zeroth and first moments of ϱ vanish. ϱ could be a continuous or a discrete distribution. Regularization by a single auxiliary mass had been used before by Feynman[66], while Stueckelberg and Rivier[67] had used an arbitrary number of masses.

The most important problem of unambiguity analyzed in the paper by Pauli and Villars was the self-energy of the photon. Wentzel[68], by formal application of Schwinger's method, had obtained a non-vanishing result, in violation of gauge invariance. With the same technique, Feynman's result[66] for the electron self-energy and Schwinger's result[69] for the magnetic moment of the electron were obtained. In addition, an older result by Pauli and Rose [51] for the finite polarization effect on a point charge could be reproduced.[70]

As Pais and Uhlenbeck had shown[71], multi-mass regularization implies an indefinite metric in Hilbert space.[72] An indefinite metric was introduced for the first time by Dirac in 1942[73] as a new method to overcome, in the words of Pauli, 'all well-known convergence difficulties of quantized field theories if it is coupled with a quite different and logically independent method due to Wentzel and improved by Dirac, the so-called λ-limiting process' (Ref. [41], p. 175). However, Pauli showed [41] that pair creation and annihilation completely invalidated this result. The reason is that for a consistent application of the λ-process the electrons must not come closer to each other than the distance λ.

In a situation similar to Dirac's an indefinite metric was introduced by Källen and Pauli [52] in their paper on the Lee model.[74] By introducing a nucleon form factor, Källen and Pauli were able to express the coupling constant renormalization of Lee in finite form which gave rise to a finite critical coupling constant g_{crit}. For a value of the renormalized coupling constant $g > g_{\text{crit}}$, the normalization factor of the V-particle state became imaginary thus leading naturally to an indefinite metric and, in addition, to an non-unitary S-matrix.

A last short note on indefinite metric in an extended Lee model where the V-particle has two complex conjugate eigenvalues was presented by Pauli at the CERN conference of 1958 [53], five months before his death. In this note Pauli concentrates 'on the possibility of a temporal description of physical phenomena, since this is necessary in order to understand causality'. The rather artificial character of this note, so untypical of Pauli, hardly conceals a certain disillusionment with quantum field theory which Pauli had arrived at towards the end of his life.

5. PHYSICS OF CONDENSED MATTER. STATISTICAL PHYSICS

As a graduate student in Munich, Pauli wrote three short papers on magnetic and dielectric properties of gases. In the first paper [54] submitted on 18 June 1920 Pauli calculated the diamagnetic susceptibility of monatomic gases with the aid of Larmor's theorem (multi-atomic gases are excluded by the fact that Larmor's theorem is not necessarily applicable).

The result, which is known as Langevin–Pauli diamagnetism[75], was a temperature-independent expression proportional to the quadrupole moment θ along the field direction. Comparison with the sparse experimental data on helium and argon led to values of θ which were 10 times too small compared to what follows from the atomic dimensions. This led Pauli to suspect that the experimental values could be wrong. And he concludes 'Purpose of these lines is to incite further measurements which could decide this important question.' Pauli's doubts were justified since the now accepted values are indeed more than 10 times smaller.[76]

In the second paper [55], which was based on a talk given at a meeting of the natural scientists in Germany, Pauli discussed the magnetic moment as determined from the Langevin formula $\mu^2/3RT$ for the paramagnetic susceptibility.[75] He rejects the Weiss magneton (which is approximately 5 times smaller than the Bohr magneton μ_B) as a fundamental unit and explains how angular momentum quantization (according to the old quantum mechanics) leads to non-integer values of μ/μ_B. Applied to diatomic molecules he concludes by comparison with experimental data that O_2 and NO have angular momentum quantum number 2 and 1, respectively, and that the magnetic moment is perpendicular to the molecular axis.

The third paper [56], submitted on 30 July 1921, treats the analogous case of the dielectric Langevin formula where μ is the electric moment. The statistical average implied in μ is more complicated in this case. Taking the dumb-bell model of a diatomic dipolar molecule, the problem reduces to the spherical pendulum which is treated according to the old quantum mechanics. This leads to a time average of the direction cosine which is expressed in terms of phase integrals. Subsequent statistical averaging then produces a quantum mechanical moment which is 2.1471 times smaller than the classical one. No experimental data were available to Pauli to test this result.

In a sequel to this paper of 1926, Mensing and Pauli [57] rederived μ according to the new quantum mechanics which gave a result much closer to that of classical theory. In particular, in the limit of high temperatures there was an exact coincidence between quantum mechanical and classical moment. And the authors conclude

> It appears, therefore, that here again, as in many other cases, the new quantum mechanics joins classical mechanics more closely with respect to statistical averages, than the old quantum theory.

These early papers on the problems of statistical physics, which are not widely known, have been described here in some detail because they demonstrate Pauli's precise and eminently practical method of research in a field which is entirely different from what has been discussed so far, and because they show Pauli's very early interest in this type of problems.

In another early paper [58] written in Copenhagen, Pauli gives an interesting and not well-known derivation of Planck's radiation law, based on the energy-momentum conservation for the Compton effect.[77] He shows that in order to get Planck's law the number of Compton processes $\nu \rightarrow \nu_1$, for given initial electron momentum, must be proportional to $\varrho_\nu + \text{const} \cdot \varrho_\nu \varrho_{\nu_1}$, ϱ_ν being the spectral density, where the bilinear term is due to fluctuations of the radiation and gives the necessary modification leading from Wien's law to Planck's law. This shows explicitly the importance of fluctuations in radiation equilibrium. Pauli then remarks that a generalization of this procedure to other systems, such as atoms, allows one also to consider optical dispersion phenomena. As observed by Bohr[78], this generalization is very closely related to Kramers' dispersion relation. Pauli also wrote a review article on the theory of black-body radiation which appeared in 1929 in Müller and Pouillet's treatise [59], although it had been completed already in 1924. The above paper [58] is described in detail in this review.

Pauli's published contributions to solid state physics are contained essentially in two early papers. The first one of 1925 [60] deals with the problem of infrared absorption of dielectric crystals, and was a communication to the German Physical Society. It is treated in the model of a linear chain with nearest neighbour anharmonic forces which are quadratic in the elongations. The result, given without comments on the calculation involved, is controversial. In fact, Peierls[79] goes so far as to say that 'The published summary of this talk is probably the only incorrect formula published under Pauli's name.' Yet Pauli's formula does not look unreasonable, at least at temperatures T above the Debye temperature where an absorption proportional to T is to be expected. As Peierls remarks, the particular sinusoidal dispersion curve of a linear chain has no non-trivial solution to the energy and wave vector conservation equations for the three-phonon process involved in this problem, and therefore the result should be zero. But at finite temperature the thermal broadening, which was not taken into account in Pauli's calculation, relaxes conservation, leading to a non-zero result.[80] Anyway, 'Pauli was not satisfied and suggested a further study of the problem'.[79] And it was indeed to Peierls, his student and subsequently his assistant at ETH, that he made this suggestion in 1929.[81]

The three-dimensional problem turned out to be complicated by the existence of different phonon branches and, in fact, had a long history to which Peierls contributed significantly.[82] Pauli's dissatisfaction, on the other hand, may well have left a deeper trace in his mind, for he did not have the happy gift of getting rid of frustrations easily. And, perhaps, his well-known remark, that 'I don't like this solid state physics... I initiated it though'[83] may well have had this secret root.

This remark of Pauli also points to a highly positive fact. The allusion that he had actually initiated solid state physics refers to his second publication in this domain, the famous paper on the Pauli paramagnetism [61], submitted on 16 December 1926. Concerning this paper Peierls remarks that 'it is probably no exaggeration to say that the modern electron theory of metals was started by Pauli's paper on the paramagnetism of an electron gas.'[84]

While the exclusion principle had already proved its success in the understanding of the atomic structure*, its consequences for the statistics of identical particles were just beginning to be recgonized**. Fermi[85] had applied it to a gas of particles (electrons). Dirac[86] had shown that its application to the quantum mechanical solution of an ideal gas necessarily led to Fermi statistics and he argued that for a gas of material particles (as against photons), Fermi statistics, and not Bose statistics, should be applied.

Pauli adopted this point of view, though somewhat reluctantly. This is because the

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* W. Heisenberg, Z. *Physik*, **39**, 499 (1926): Helium spectrum.

** W. Heisenberg, Z. *Physik*, **38**, 411 (1926): Many-body problem.

analogy with the gas of light quanta, which was emphasized in his paper on Planck's radiation law [58], was thus destroyed. On the other hand, he points out the difference in the significance of the zero-point energy (mentioned in the previous Section) and of the velocity in the two cases. He then gives a general derivation of Fermi statistics, including the fluctuations, by the method of the grand canonical ensemble which at that time was not yet well known (Fermi had not used it). Pauli later took over this derivation of quantum statistics almost literally into his course on statistical mechanics [62].

After an application to atoms with arbitrary angular momentum he derives in this paper [61] the paramagnetic susceptibility bearing his name. But he emphasizes that the approximation of the metallic electrons as an ideal gas would have to be refined by taking into account their interactions. He observes that due to the exclusion principle the low-temperature susceptibility is strongly reduced from the Langevin value, and is of the same order as the diamagnetic contribution, which later was derived by Landau.[87]

At the 6th Solvay Conference in Brussels in 1930, Pauli presented an exhaustive report entitled 'Les théories quantiques du magnétisme. L'électron magnétique' [63]. In the first section Pauli reviews the paramagnetism and the diamagnetism of free electrons, in particular Landau's then new theory[87] which in the report is still quoted only as an oral communication. He then discusses the exchange integral and its application by Heisenberg[88] to the theory of ferromagnetism, as well as a number of other questions related to ferromagnetism. This section is Pauli's last written contribution to solid state physics, the second section being devoted to the relativistic theory of the electron.

But Pauli's interest in statistical physics had another important side, concerning the problem of Boltzmann's H-theorem in quantum mechanics. The first paper on this problem is contained in the volume dedicated to Arnold Sommerfeld by his pupils on the occasion of his 60th birthday [64], published in 1928. Pauli sets out to show that from the point of view of wave mechanics Boltzmann's H-theorem on the increase of entropy can be given a much more general form than is possible in classical mechanics. This is due to the fact that wave mechanics is already a statistical theory which gives much greater simplicity and generality to the laws describing the energy transfer between subsystems.

Pauli derives the 'golden rule' for the probability $W_m(t)$ to find the system in a state m at time t if it was in state n at time 0. The calculation of the inverse process from m to n is more complicated and necessitates the 'hypothesis of elementary disorder', according to which the initial phases are random. This leads to the detailed balance $A_{nm}=A_{mn}$ for the transition probabilities A_{nm} and to the rate equation $\Delta W_n = -\sum_m Z_{nm}\,\Delta t+\sum_m Z_{mn}\,\Delta t$ which later was baptized as the 'master equation' by Uhlenbeck.[89] Here $Z_{nm}=A_{nm}G_m W_n$ is the transition rate and G_m the weight of state m. The time interval Δt must be small enough so that the W_n do not vary appreciably over Δt. The H-theorem then is a simple consequence of the master equation.

Conditions for the existence of the H-theorem in quantum mechanics are investigated in a second paper by Pauli and Fierz in 1937 [65]. The derivation and the consequences of the master equation gave rise to a large number of publications[90] of which the work of van Hove[91] is of particular importance. In fact, the weakness of Pauli's derivation is that in order to integrate the master equation on a time scale long compared to the interval Δt the random phase assumption has to be made at every step Δt. Van Hove showed that the master equation can actually be derived with one initial random phase assumption only. This of course still destroys translational invariance in time and therefore leads to irreversibility.

It is noteworthy that this work of van Hove was among the contents of the last course that Pauli gave at ETH. It was in the middle of this subject that the lectures abruptly stopped on 5 December 1958, ten days before his death.

On the other hand, the last work Pauli published in a scientific journal was a paper on phenomenological thermodynamics [66], submitted on 19 July 1957. In this paper the chemical equilibrium of mixtures was studied with the aid of external force fields which allowed one to do away with the cumbersome van 't Hoff boxes. This paper was included in the second edition of Pauli's course on Thermodynamics [67].

6. NEUTRINO, CPT-THEOREM AND PARITY VIOLATION

Pauli has recounted the history of the neutrino in a conference given in the Zurich Society of Natural Sciences in the evening of 21 January 1957 [68]. This was an exciting day since that very afternoon Pauli had received from Telegdi in Chicago the first results of the three experiments by Madame Wu[92], by Lederman[93] and by Telegdi[94] on the parity violation in beta- and mu-decay. In the morning of the same day two theoretical papers, one by Yang, Lee and Oehme[95] and the other by Lee and Yang on the two-component theory of the neutrino[96], which had been proposed by Salam[97] several weeks earlier[98] were in Pauli's mail. And from Villars in Geneva came the *New York Times* article reporting the sensation of parity violation.[98, 99]

Understandably, Pauli was on the peak of excitement, which, however, was somewhat dampened by his embarrassment about not having believed that 'God is just left-handed'.[98] He gave an excellent talk that evening, and at the end he broke the news about parity violation, giving an improvised account of the problem and its importance. (An extended version of this account is contained in Section 5 of the published version [68] of this talk.)

Pauli's own brainchild, the neutrino, conceived 27 years earlier, had unquestionably played a trick on him! In a letter to Madame Wu written on the afternoon of the talk at Zurich, Pauli says[100] 'this particle neutrino, of the existence of which I am not innocent, still persecutes me'. Its capricious left-handedness had created considerably more publicity than its birth, which for Pauli had been the only possible way out of a dilemma.

This dilemma was the continuous beta-spectrum of Radium E (see Fig. 1 of Ref. [68]). Calorimetric measurements by Ellis[101] and by Lise Meitner[102] in 1930 had yielded an energy corresponding to the average energy of the beta-spectrum, and not to its maximum. These results ruled out Meitner's idea that the continuous character of the spectrum was due to secondary processes. In order to explain this situation Niels Bohr invoked his old idea of *statistical* energy conservation[103] in beta-decay.[104] But to Pauli's deeply rooted sense of symmetries and conservation laws such a compromise was unacceptable. The only logical alternative was to invoke a new particle so penetrating that it escaped all measuring apparatus.

The first written account of this shockingly bold idea is contained in a letter of 4 December 1930 that Pauli addressed to the 'dear radioactive ladies and gentlemen' at a physics meeting in Tübingen, which Pauli was unable to attend because of a dance in Zurich (see Ref. [68], p. 159). In this letter he characterizes this new particle as neutral spin-$\frac{1}{2}$ particle of mass smaller than 0.01 times the proton mass and calls it neutron. This name had been introduced by Rutherford in 1921[105] for a hypothetical nucleus consisting of one proton and one electron. After the discovery of the real neutron by Chadwick in 1932[106] Fermi baptized Pauli's particle 'neutrino', the little neutron, in seminars he gave in Rome (see Ref. [68], p. 162). In June 1931 Pauli gave an account of his ideas in a talk at Pasadena, and at the 7th Solvay Conference in Brussels in 1933 he discussed the entire problem of beta-decay including the neutrino hypothesis.[107]

Inspired by the discussions at the 7th Solvay Conference Fermi shortly afterwards developed his theory of the beta-decay.[108] From a comparison of the statistical factor of allowed decays at the upper end of the spectrum Fermi[108] and Perrin[109] concluded already in 1933 that the neutrino mass should be zero (see Ref. [68], p. 164).

Pauli was particularly well prepared for the theory of beta-decay by his two papers on the mathematical theory of the Dirac matrices of 1935 and 1936 [69]. But although he took an active part in the analysis of the general structure of the theory, one of the results of which was the rearrangement theorem for the permutation of the fields, he left the elaboration to his assistant Fierz.[110] Instead, Pauli looked into the field-theoretic aspect of beta-decay interaction. In a note presented to the Academy of Sciences of the U.S.S.R. in 1938, and published in Russian [70], Pauli showed that the Fermi theory of beta-decay leads to an infinite self-energy and that, therefore, the application of the perturbation theory in higher than first order (as had been done by Heisenberg[111] in his theory of cosmic showers) was unjustified. This problem has remained with field theory ever since and has led to the distinction between renormalizable and unrenormalizable interactions.[112]

The two papers on the mathematical structure of the Dirac matrices [69], on the other hand, have become of great use for the definition of the discrete symmetry operations of charge conjugation (C), parity (P) and time-reversal (T) for spin-$\frac{1}{2}$ fields. These operations had been analyzed in view of the spin-statistics problem by Schwinger[113], and in particular by Lüders.[114] Lüders made the remarkable discovery that the product CPT had more general invariance properties than each operation separately, when applied to specific interaction Hamiltonians.

Pauli looked into this problem himself, and the result was the famous paper on the CPT-theorem [71] which he dedicated to Niels Bohr's 70th birthday and which is part of the volume edited by Pauli (with the assistance of L. Rosenfeld and V. Weisskopf) for this occasion. In a dedication full of warmth and wit Pauli paraphrases Bohr's favourite verses of Schiller which were chosen to introduce this essay. He says that while trying to use a rigorous mathematical formalism 'to connect all mentioned features of the theory with the help of a richer "fullness" of plus and minus signs in an increasing "clarity" ', the epistemological analysis 'makes me aware that the final "truth" on the subject is still "dwelling in the abyss" '.

Pauli's original form of the *CPT*-theorem states that if the combined *CPT*-inversion, which he calls 'strong reflection', is properly defined for the original field quantities 'it also holds for all ordered products of them or their derivatives of finite order after application of the inversion' (Ref. [71], p. 35). The remarkable fact of the theorem is that it only supposes invariance with respect to the proper, i.e. continuous, Lorentz group and local character of the interactions, the proper spin-statistics relationship being assumed. For the proof Pauli relies heavily on the spinor analysis he had developed in his spin-statistics paper [37].

The 'final truth' of the theorem rose to full 'clarity' shortly afterwards through the work of Jost[115], who had realized at an early stage that the combined *PT*-inversion could be continuously connected to the identitiy by analytic continuation in the complex Lorentz group. While the *CPT*-theorem, together with the spin-statistics relationship, has become a corner stone of modern field theory[115], it has also played a decisive role in weak interactions for the analysis of both the *P*-violation of the neutrino [68] and the *CP*-violation of the neutral *K*-meson.[116]

Pauli's interest in the problem of the beta-decay was stimulated anew by two experimental events which occurred several months before the parity violation was established. On 15 June 1956 he received a telegram from F. Reines and C. Cowan from the Los Alamos nuclear reactor with the following content: 'We are happy to inform you that we have definitely detected neutrinos from fission fragments by observing inverse beta-decay of protons. Observed cross-section agrees well with expected 6×10^{-44} cm^2.' Pauli answered the same day by a night letter with the following content[117]: 'Thanks for message. Everything comes to him who knows how to wait. Pauli.'

So the existence of Pauli's brainchild was at last certified. Pauli announced the good news at the CERN symposium in July 'because otherwise everybody would ask me separately' [72]. This last excuse, of course, was superfluous, but typical for Pauli: his natural modesty did not permit him to show too openly that he was proud.

The experiment of Cowan and Reines[118] was a veritable *tour de force* since the penetration depth of neutrinos in lead is of the order of 100 light years! (see Ref. [68], p. 160). Cowan and Reines also determined upper bounds for the mass and the magnetic moment of the neutrino.[119]

The second experimental event of the year 1956, which stimulated Pauli, was Davis' negative result for the detection of the beta-transition of ^{37}Cl induced by neutrinos from a nuclear reactor.[120] The result meant that lepton charge, i.e. the number of leptons minus the number of anti-leptons, was conserved. It was in response to this result that Pauli undertook to analyze the consequences of the non-conservation of the lepton number.

Into this occupation exploded the news about parity violation, which Pauli swiftly integrated into his work. The result was the paper on the general form of the beta-decay interaction [73], submitted on 14 May 1957. The essence of this paper was that linear canonical transformations mixing right- and left-handed neutrinos and antineutrinos induced, for zero neutrino mass, 2×2 unitary unimodular transformations of the coupling constants. This group property then gave rise to four bilinear invariants of the coupling constants plus two pseudo-invariants transforming with a phase. Consequently, S-matrix elements can only depend on these six quantities which greatly simplifies the analysis of particular processes, such as the reaction of Davis, the detailed calculations of which Pauli left to his assistant.[121]

The Pauli group of transformations of the coupling constants describes nothing else than rotations for spin-$\frac{1}{2}$. This property led Gürsey[122] to its interpretation as isospin transformations. Heisenberg[123] incorporated this idea into his non-linear spinor equation which, combined with a degenerate vacuum, looked very fascinating because of the hope of being able to degenerate all elementary particles from one single equation.

For a while Pauli took an enthusiastic part in these endeavours. However, later, in spring 1958, he withdrew from this collaboration with Heisenberg because the theory had revealed too many inconsistencies. In a report published posthumously by Touschek [74] Pauli comes back once more to the problem that 'the fields of all particles are to be constructed from the minimum number of fields'. But the problem remained wide open.

7. ASPECTS OF PAULI'S SPIRITUAL LIFE

Pauli was more than just a physicist with exceptional analytical and mathematical skill: he was a natural philosopher.

Three men unquestionably left an imprint on Pauli's spirit: Ernst Mach, his godfather, Arnold Sommerfeld, his teacher, and Niels Bohr, his philosopher friend. With regard to Ernst Mach, Pauli writes in the continuation of the letter of 31 March 1953 quoted at the beginning of this essay:[2]

> He evidently was a stronger personality than the catholic priest, and the result seems to be that in this way I am baptized 'antimetaphysical' instead of catholic. In any case, the card remains in the cup and in spite of my larger spiritual transformations in later years it still remains a label which I myself carry, namely: 'of antimetaphysical descent'. Indeed, Mach considered metaphysics, somewhat simplifying, to be the origin of all evil in the world – that is, psychologically speaking, as the devil himself – and that cup with the card in it remained a symbol for the 'aqua permanens' which exorcizes the evil metaphysical spirits.

This letter is written in the middle of a period of intense philosophical and psychological activity stimulated by the 'extensive and essential disccussions' with C. G. Jung in Zurich (Ref. [75], p. 167).

At that time the label 'of antimetaphysical descent' imprinted on Pauli by the spirit of Mach had faded into 'larger spiritual transformations'.[2] And the two men who had most influenced these transformations were Sommerfeld and Bohr. Theirs were the only pictures that decorated Pauli's office at ETH.[124]

Of Sommerfeld, for whom Pauli had the veneration of a timid pupil, he writes in the obituary note [30] mentioned in Section 3:

> To his pupils it will remain unforgettable how in his fine sense for harmonies based on integral numbers he reconjured the spirit of Kepler.

And in the important essay on the ideas of the unconscious in relation to the sciences [76], written on the occasion of the 80th birthday of C. G. Jung on 26 July 1954, Pauli writes on p. 295:

> Through my teacher A. Sommerfeld I knew very well how these Pythagorean elements appearing with Kepler are still alive today.

Here Pauli refers to an article he had dedicated to Sommerfeld's 80th birthday on 5 December 1948 [77] where already the spirit of Kepler had been invoked.

This contagious fascination for Kepler transferred by his teacher inspired Pauli to a more serious investigation of Kepler's scientific method in his remarkable essay in the volume 'Naturerklärung und Psyche' published together with C. G. Jung [75]. In this essay the 'Pythagorean elements' in Kepler's thinking are the starting point of a profound analysis of the historical, philosophical and psychological motivation of science. Pauli writes of Kepler (Ref. [75], p. 114):

> To him as legitimate spiritual descendent of the Pythagoreans all beauty resides in the right proportion, for 'geometria est archetypus pulchritudinis mundi' (geometry is the primordial image of beauty). This principle of his is his strength and, at the same time, his limitation: his ideas about the regular polyhedra and the harmonic proportions clearly would not quite fit in the planetary system ....

The key word 'archetypus' mentioned explicitly in the title of the essay relates this work to Jung's psychology of the unconscious. In the essay for Jung's 80th birthday mentioned above, Pauli notes (Ref. [76], p. 295):

> In search for applications of the notion of archetypus outside the modern psychology of the unconscious I first came across the historical fact that Kepler extensively and regularly used the words 'archetypus' and 'archetypalis', and this in a similar sense as Jung, namely as primordial image ['Urbild'].

With regard to Jung's work and to the importance of the unconscious in the formation of scientific ideas, Pauli gives the following motivation for his Kepler article (Ref. [75], p. 113):

> My attention therefore was directed especially towards the 17th century in which the then quite new natural-scientific reasoning had grown out of the maternal soil of the magic-animistic conception of Nature as a consequence of a large spiritual effort. For the sake of illustration of the relation between archetypical conceptions and natural-scientific theories, *Johannes Kepler* (1571–1630) seemed to me particularly proper, since his ideas represent a remarkable intermediary stage between the former magic-symbolic and the modern quantitative-mathematical description of Nature.

The particular significance of the 17th century for modern science is also emphasized in a conference that Pauli gave in the Zurich Philosophical Society in February 1949 [78]. He says (Ref. [78], p. 1):

> The schism of the natural sciences and mathematics as autonomous partial disciplines from an originally unified but pre-scientific natural philosophy, which occurred in the 17th century, was, on the

one hand, a necessary condition for the further spiritual development of the Occident. But today the conditions for a renewed agreement of the physicists and philosophers on the epistemological foundations of the scientific description of Nature seem to me to be fulfilled.

Pauli then gives the reasons for this hope of a 'renewed agreement of the physicists and philosophers' in a formulation which he repeats almost verbally in his Kepler article (Ref. [75], p. 163–164):

> The development of atomistics and quantum theory since 1900 has indeed led to the fact that physics was gradually forced to give up its proud claim to be able, in principle, to understand the *whole* world ... Just this circumstance, however, [that today we do possess natural sciences but not any more a natural-scientific conception of the world] could, as a correction of the former one-sidedness, contain the germ of a progress in direction towards a unified global conception of the world, in which the natural sciences are only one part. Herein I would like to perceive the more general significance of the idea of complementarity, which, thanks to the Danish physicist Niels Bohr, has grown out of the soil of physics.

This idea of complementarity was the focal point of the inspiration that Bohr conveyed to Pauli on their 'long and still continuing common pilgrimage since the year 1922, in which so many stations are involved'. (See the dedication in Ref. [71].) The 'soil of physics' out of which this idea has grown is the quantum mechanics which, through the indeterministic character of the uncertainty relations, leaves the physicist the freedom to choose between mutually complementary experimental setups.

With regard to this freedom Pauli says in a conference given at the meeting of the Swiss Society of Natural Sciences in Berne in 1952 (Ref. [79], p. 116):

> Herewith observation acquires the character of the *irrational, unique actuality* with a not predictable result.

This irrational element built into quantum mechanics, which Pauli had already emphasized in his conference in Zurich in 1949 (Ref. [78], p. 8), was of importance to him because of its far reaching implications outside physics. In the last quoted reference he continues:

> This feature of complementarity within physics leads in a natural way beyond the more limited domain of physics to analogous situations with the general conditions of the human perception.

This leads to the problem of consciousness which, in analogy to the relation between the observing apparatus and the observed system in physical experiments, implies a relation between subject and object. The passage on this relation contained in the last quoted reference impresses by the vastness of Pauli's view. And one can only agree with Arthur Koestler writing about Pauli[125]: 'He may well have possessed an even deeper knowledge of the limits of the natural sciences than most of his colleagues.'

Pauli says (Ref. [78], p. 9):

> The notion of consciousness, in fact, requires a cut between subject and object, the *existence* of which is a logical necessity while, on the other hand, the *position* of the cut is, to a certain degree, arbitrary. Non-consideration of this fact gives rise to two different kinds of metaphysical extrapolations which themselves can be called mutually complementary. The one is that of the material or, more generally, physical object, the constitution of which ought to be independent of the way in which it is observed ... The complementary extrapolation is that of the Hindu-metaphysics of the pure subject of perception to which is opposed no object. Personally I have no doubt that this idea also must be recognized as

untenable extrapolation. The occidental spirit cannot recognize such a notion of a trans-personal cosmic conscience to which is opposed no object, and must keep the median prescribed by the idea of complementarity.

This attitude of mediation between extremes, quite typical of Pauli, played also a role in his relationship with Jung. In the course of his work Jung was gradually forced towards a less extreme view in his psychology of the unconscious by admitting non-psychical elements. In relation to this, Koestler quotes the following sentence by Jung written in 1947 and referring to para-psychological phenomena:[126]

> I frankly doubt that an exclusively psychological methodology and consideration can do justice to the phenomena in question. Not only the statement of parapsychology but also my own theoretical reflections, which I have sketched in the *Theoretische Ueberlegungen zum Wesen des Psychischen*, lead me to certain postulates which touch the domain of the atomic-physical conceptions, that is the space time continuum. Herewith the question of the trans-psychical reality is raised which is at the immediate basis of the psyche.

The influence of Pauli on Jung is obvious from these remarks. In fact, Jung openly acknowledges this influence in his contribution to the common volume *Naturerklärung und Psyche* (Ref. [75], p. 101; see also Koestler,[125] p. 110). Koestler, however, remarks:[127]

> ... apart from his function as auxiliary teacher in the domain of theoretical physics (which, after all, Jung made little use of) Pauli probably had only little influence on Jung's treatise.

Pauli explicitly refers to Jung's gradual change of attitude mentioned above in his essay on the occasion of Jung's 80th birthday in relation to Jung's encounter with alchemy (Ref. [76], p. 290–291):

> It does not surprise us that soon after the first occurrence of this encounter with alchemy the psycho-physical problem and also the problem of the inclusion of the observer in the course of Nature assumes actuality. Indeed, in order to cope with these fundamental problems *Jung*, in 1946, applied drastic changes to the notions used by him. He does it in particular in view of the phenomena of 'extra sensory perception' (ESP)....

The psycho-physical problem mentioned in these lines has much occupied Pauli, as is clear from other passages in the same essay [76]. One reason was that dreams are examples of psycho-physical processes (see Ref. [76], p. 287). But, more generally, it was related to his hope of reaching a synthesis between Science and Psychology. In his essay on Kepler he ventures into wishful thinking when he says (Ref. [75], p. 164):

> It would be most satisfactory if *physis* and *psyche* could be understood as complementary aspects of the same reality.

Admittedly, this remained an open question for Pauli.

In closing this essay the other open question has to be mentioned again which bothered Pauli during all his scientific life, and which also brings another man into the picture of Pauli's spiritual life: Albert Einstein. This open question is the one which was raised for the first time by the 19-year-old Pauli in his third paper [3] (see Section 1):

> Sollten wir überhaupt mit den Kontinuumstheorien für das Feld im Innern des Elektrons auf einer falschen Fährte sein?

It was Einstein's hope of his lifetime to integrate the atomistic constitution of matter into a unified theory. He never accepted the finality of the indeterministic character of quantum theory, and his well-known argument was that 'Gott würfelt nicht', 'God does not throw dice'. This had the effect that Pauli's relation to Einstein soon became one of respectful disagreement. But Einstein's admiration for Pauli was not affected by this disagreement. In fact, at a dinner in honour of Pauli held at the Institute for Advanced Study in Princeton, when Pauli left for a visit to Europe in 1946, the old and ailing Einstein quite unexpectedly rose to give an address which was moving and fascinating in its clear simplicity. In this address Einstein designated Pauli as his successor at the Institute and called him his spiritual son, of whom he hoped that he would carry on his, i.e. Einstein's, work.[128]

In 1958, in an article on Einstein Pauli writes [80]:

> Since 1927 Einstein was disappointed by the development of physics. Unyielding, he retreated into his spiritual solitude. His further work on field theory although written with the same mathematical mastery as before, seems to lack the close contact with Nature. It is doubtful whether these last theoretical attempts of Einstein will in fact find an application in physics.

This article, written in the year of Pauli's death, then takes an unexpected turn in its closing paragraph which voices a challenge to the address of the younger generation, including Pauli himself:

> Had we been able to present Einstein a synthesis of his general relativity theory with quantum theory then the discussion with him would have been considerably easier. But the duality between the field and its means of measurement, although latently present in today's quantum theory of fields, is conceptually not clearly expressed. The relation of the applicability of the ordinary space-time concept in the small with the properties of the smallest physical objects, the so-called 'elementary' particles is not disclosed. Einstein's life ended with a question [posed] to the science of physics and with a behest for synthesis to us.

These remarks dramatically reflect the fact that the question which had been posed by the 19-year-old Pauli, retained its urgency to the end of his life. It is the clash between the continuity of the notion of the field and the atomicity of the electric charge (and, more generally, all the coupling constants of elementary particle interactions) which bothered Pauli to a point that he raised the question time and again throughout his work, including the introduction to his course on electrodynamics [81].

This fact is substantiated by the following passages. The first is contained in a conference given at the Zurich Philosophical Society in 1934 [82]:

> But these [classical field] theories ... are unable to interpret the additional fundamental property of the [electric] charge to be atomistic .... Only a new formulation of quantum theory would be satisfactory which, just as it juxtaposes as complementary the momentum and energy conservation laws and the description in space and time, would also juxtapose the classical conservation law of charge and its atomicity in a quantum theoretical correlation by an interpretation of the numerical value of the dimensionless number $hc/(2\pi e^2) = 136.8 \pm 0.2$ (see also Ref. [35], p. 713).

Another place where this problem surfaces, not less urgently in its explicit wording, is the closing paragraph of Pauli's Nobel lecture delivered in Stockholm on 13 December 1946 [19]:

> From the point of view of logic, my report on 'Exclusion principle and quantum mechanics' has no

conclusion. I believe that it will only be possible to write the conclusion if a theory will be established which will determine the value of the fine structure constant and will thus explain the atomistic structure of electricity, which is such an essential quality of all atomic sources of electric fields actually occurring in nature.

The last reference in which the fine structure constant is explicitly mentioned is in note 23 of Ref. [5], written in 1956 (see Section 1), where Pauli writes:

> In this connection it should be remembered that the atomicity of electric charge has already found its expression in the specific numerical value of the fine structure constant, a theoretical understanding of which is still missing today.

The number 1/137, the fine structure constant which his teacher Sommerfeld had introduced into physics, was Pauli's link to the 'magic-symbolic' world with which he was so familiar. Pauli spent the last few days of his life in the Red Cross Hospital in Zurich, where he died on 15 December 1958. A fact which had disturbed him during these last days was that the number of his room was 137.

NOTES AND REFERENCES

1. Two biographical as well as scientific portraits of Wolfgang Pauli deserve special mention. The first is an article by R. E. Peierls in *Biographical Memoirs of Fellows of the Royal Society* (The Royal Society, London, 1959), Vol. 5, pp. 175–192. Entitled 'Wolfgang Ernst Pauli' it is the only reference which explicitly mentions Pauli's second name Ernst, which Pauli himself never used. Apart from supplementary biographical information this article also contains a selection of the well-known Pauli anecdotes. The second portrait is an article by W. Thirring in *Oesterreichs Nobelpreisträger*, edited by F. G. Smekal (Wilhelm Frick, Wien-Stuttgart-Zürich, 1961), pp. 147–157 and p. 191. While giving some additional biographical elements it contains two inaccuracies (see p. 152); in fact, Pauli became full professor (Ordinarius) at ETH in 1928 and Swiss citizen in 1948.
2. '... Unter meinen Büchern befindet sich ein etwas verstaubtes Etui, in diesem ist ein Silberbecher im Jugendstil und in diesem wiederum ist eine Karte ... Dieser Becher nun ist ein Taufbecher und auf der Karte steht in altmodisch verschnörkelten Buchstaben:

   "Dr. E. Mach, Professor an der Universität Wien".

   Es kam so, dass mein Vater sehr mit seiner Familie befreundet war, damals geistig ganz unter seinem Einfluss stand und er (Mach) sich freundlicherweise bereit erklärt hatte, die Rolle des Taufpaten bei mir zu übernehmen .... Er war wohl eine stärkere Persönlichkeit als der katholische Geistliche, und das Resultat scheint zu sein, dass ich auf diese Weise antimetaphysisch statt katholisch getauft bin. Jedenfalls bleibt die Karte im Becher und trotz meiner grösseren geistigen Wandlungen in späterer Zeit bleibt sie doch eine Etikette, die ich selber trage, nämlich: "von antimetaphysischer Herkunft". In der Tat betrachtete Mach die Metaphysik, etwas vereinfachend, als die Ursache alles Bösen auf Erden – also psychologisch gesprochen: als den Teufel schlechtweg – und jener Becher mit der Karte darin blieb ein Symbol für die "aqua permanens", welche die bösen metaphysischen Geister verscheucht. ...'

   A copy of this letter is exhibited, together with the silver cup and Mach's card, in the Pauli archive at CERN, Geneva. I am grateful to Mrs F. Pauli for having drawn my attention to these objects.
3. Pauli's remark on the occasion of Walter Thirring's acceptance of the chair of theoretical physics at Vienna University in 1958.
4. *Neue Illustrierte Wochenschau*, 46. Jahrgang, Nr. 22, Wien (29. Mai 1955); *Hamburger Abendblatt*, Jahrgang 15, Nr. 53 (3. März 1962).
5. See the second reference of note 4, the accuracy of the content of which has been confirmed to me by Mrs F. Pauli.

6. See Wolfgang Pauli, *Collected Scientific Papers* (ed. by R. Kronig and V. F. Weisskopf), Interscience, New York–London–Sydney (1964), Preface, vol. **1**, p. viii.
7. See Wolfgang Pauli, *Collected Scientific Papers*, vol. **1**, p. x, which is a translation of a review by Einstein published in *Naturwiss.* **10**, 184 (1922).
8. J. Mehra, 'Einstein, Hilbert, and the Theory of Gravitation', in this volume, see note 242 on p. 170.
9. See V. Bargmann, 'Relativity', in *Theoretical Physics in the Twentieth Century, A Memorial Volume to Wolfgang Pauli* (ed. by M. Fierz and V. F. Weisskopf), Interscience, New York–London (1960), p. 197.
10. W. Thirring, *Acta Physica Austriaca*, Supplementum 1972, and this volume.
11. See Pauli's own biographical note written in Hamburg in 1926 after having obtained the title of professor, reproduced in Wolfgang Pauli, *Collected Scientific Papers*, vol. **1**, pp. v–vi (English translation, pp. x–xi).
12. Pauli's doctor's diploma is exhibited, together with the diplomas of various honours he had obtained later, in the Pauli archive at CERN, Geneva. The following gold medals are deposited at the same place: Lorentz medal (1931), Nobel medal (1945), Franklin medal (1952), Matteucci medal (1956), Planck medal (1958).
13. A. Landé, *Z. Phys.* **15**, 189 (1923).
14. See B. L. van der Waerden, 'Exclusion Principle and Spin', in *Theoretical Physics in the Twentieth Century* [see note 9], p. 202.
15. A. Landé, *Z. Phys.* **19**, 112 (1923).
16. L. H. Thomas, *Nature* **117**, 514 (1926); *Phil. Mag.* **3**, 1 (1927). Compare also J. Frenkel, *Z. Phys.* **37**, 243 (1926).
17. An angular momentum of $\hbar/2$ leads classically to a velocity $v=\hbar/(2mr)$ for a mass m rotating at a radius r. Taking for m the electron mass m_e and for r the classical electron radius $r_e=e^2/(m_e c^2)$ leads to $v/c\simeq 70$. For a nucleus the radius is at least of the same order as r_e and the mass is at least 2000 times larger than m_e so that $v/c\lesssim 0.04\times I$ where I is the nuclear spin.

    For the electron this result was first derived by Goudsmit and Uhlenbeck at the end of September 1925. See Uhlenbeck's address delivered at Leiden in 1955 on the occasion of his acceptance of the Lorentz Professorship, quoted in B. L. van der Waerden, 'Exclusion Principle and Spin', in *Theoretical Physics in the Twentieth Century*, p. 213. I am grateful to Professors G. E. Uhlenbeck, B. L. van der Waerden and J. Mehra for interesting conversations on this point.
18. See R. Kronig, 'The Turning Point', in *Theoretical Physics in the Twentieth Century*, pp. 5–39, and B. L. van der Waerden, 'Exclusion Principle and Spin', same reference, pp. 209–216.
19. I am grateful to Professor J. Mehra for having drawn my attention to the existence of this postcard.
20. S. A. Goudsmit, *Physics Today* **14**, No. 6, 18 (1961).
21. J. R. Rydberg, *Lunds Univ. Arsskrift* **9**, No. 18 (1913). *J. Chimie phys.* **12**, 585 (1914).
22. In modern notation $k_1=l+1$, $k_2=j+\frac{1}{2}$, $m_1=m_j$, $m_2=m_j\pm\frac{1}{2}$. See B. L. van der Waerden, 'Exclusion Principle and Spin', in *Theoretical Physics in the Twentieth Century*, p. 206.
23. E. C. Stoner, *Phil. Mag.* **48**, 719 (1924).
24. Translated from B. L. van der Waerden, *Sources of Quantum Mechanics*, North-Holland, Amsterdam (1967), p. 25. See also W. Heisenberg, 'Erinnerungen an die Zeit der Entwicklung der Quantenmechanik', in *Theoretical Physics in the Twentieth Century*, p. 43.
25. See W. Heisenberg, *Der Teil und das Ganze*, Piper, München (1969), pp. 91–92. I am grateful to Professors B. L. van der Waerden and C. F. von Weizsäcker for an interesting conversation on this point.
26. W. Heisenberg, *Z. Phys.* **33**, 879 (1925).
27. M. Born and P. Jordan, *Z. Phys.* **34**, 858 (1925).
28. P. A. M. Dirac, *Proc. Roy. Soc.* **109**, 642 (1925).
29. M. Born, P. Jordan and W. Heisenberg, *Z. Phys.* **35**, 557 (1926).
30. Translated from W. Heisenberg, 'Erinnerungen an die Zeit der Entwicklung der Quantenmechanik,' in *Theoretical Physics in the Twentieth Century*, p. 43.
31. W. Lenz, *Z. Phys.* **24**, 197 (1924). For the Coulomb potential $-Ze^2/r$ Lenz' vector takes the form $(Ze^2m)^{-1}\cdot(\mathbf{l}\times\mathbf{p})+\mathbf{r}/r$.
32. E. Schrödinger, *Ann. Phys. (Leipzig)* **79**, 361 (1926).

33. B. L. van der Waerden, in this volume. It is interesting to note that in this letter to Jordan, Pauli derives Schrödinger's equation starting from a relativistic form which, for free particles, is not, however, the Klein–Gordon equation

$$\Box\psi = \frac{m_0{}^2c^2}{\hbar^2}\,\psi$$

(see notes 49 and 50 below) but

$$\Box\psi = -\frac{m_0{}^2c^2}{E^2}\frac{\partial^2\psi}{\partial t^2}.$$

Pauli must have felt that this equation looked rather strange.
34. E. Schrödinger, 'Ueber das Verhältnis der Heisenberg–Born–Jordanschen Quantenmechanik zu der meinen', *Ann. Phys. (Leipzig)* **79**, 734 (1926).
35. W. Heisenberg and P. Jordan, *Z. Phys.* **37**, 263 (1926).
36. P. A. M. Dirac, *Proc. Roy. Soc.* A **117**, 610 (1928).
37. W. Heisenberg, *Der Teil und das Ganze*, Piper, München (1969), p. 101.
38. W. Heisenberg, 'Erinnerungen an die Zeit der Entwicklung der Quantenmechanik,' in *Theoretical Physics in the Twentieth Century*, p. 47. Unfortunately, these discussions at the 5th Solvay Conference ('Electrons et Photons') have not been included in the *Collected Scientific Papers* of Pauli [see note 6]. They are listed, however, in C. P. Enz, 'Bibliography Wolfgang Pauli', in *Theoretical Physics in the Twentieth Century*, p. 306.
39. Quotations translated from Ref. [24], pp. 37 and 42.
40. Albert Einstein, Max Born, *Briefwechsel 1916–1955*, kommentiert von Max Born, Nymphenburger, München (1969), pp. 289–299.
41. E. T. Whittaker and G. N. Watson, *A Course of Modern Analysis*, Cambridge University Press (first edition, 1902).
42. P. A. M. Dirac, *Proc. Roy. Soc.* A **114**, 243, 710 (1927).
43. See G. Wentzel, 'Quantum Theory of Fields (until 1947)', in *Theoretical Physics in the Twentieth Century*, p. 51.
44. E. Fermi, *Rendiconti Acad. Lincei* **9**, 881 (1929).
45. See C. P. Enz and A. Thellung, *Helv. Phys. Acta* **33**, 839 (1960).
46. F. Bloch and A. Nordsieck, *Phys. Rev.* **52**, 54 (1937).
47. C. D. Anderson, *Phys. Rev.* **43**, 492 (1933); P. M. S. Blackett and G. P. S. Occhialini, *Proc. Roy. Soc.* A **139**, 699 (1932).
48. P. A. M. Dirac, *Proc. Roy. Soc.* A **133**, 60 (1931).
49. W. Gordon, *Z. Phys.* **40**, 117 (1926).
50. O. Klein, *Z. Phys.* **41**, 407 (1927).
51. P. A. M. Dirac, *Proc. Roy. Soc.* A **155**, 447 (1936).
52. M. Fierz, *Helv. Phys. Acta* **12**, 3 (1939).
53. See R. Jost, 'Das Pauli-Prinzip und die Lorentz-Gruppe,' in *Theoretical Physics in the Twentieth Century*, pp. 114–115.
54. S. H. Neddermeyer and C. D. Anderson, *Phys. Rev.* **51**, 884 (1937); J. C. Street and E. C. Stevenson, *Phys. Rev.* **51**, 1005 (1937).
55. H. Yukawa, *Proc. phys.-math. Soc. Japan* **17**, 48 (1935).
56. G. Wentzel, *Z. Phys.* **86**, 479, 365, (1933); **87**, 726 (1934).
57. P. A. M. Dirac, *Proc. Roy. Soc.* A **167**, 148 (1938).
58. See F. Villars, 'Regularization and Non-Singular Interactions in Quantum Field Theory,' in *Theoretical Physics in the Twentieth Century*, pp. 94–98.
59. C. Bloch, *Kgl. Danske Vidensk. Selsk. Mat.-Fys. Medd.* **26**, No. 1 (1950); **27**, No. 8 (1952).
60. P. Kristensen and C. Møller, *Kgl. Danske Vidensk. Selsk. Mat.-Fys. Medd.* **27**, No. 7 (1952).
61. S. Tomonaga, *Progr. Theor. Phys.* **1**, 27 (1946).
62. J. Schwinger, *Phys. Rev.* **74**, 1439 (1948); **75**, 651 (1949); **76**, 790 (1949).
63. R. P. Feynman, *Phys. Rev.* **76**, 749, 769 (1949).
64. F. J. Dyson, *Phys. Rev.* **75**, 486, 1736 (1949).
65. F. Villars, 'Regularization and Non-Singular Interactions in Quantum Field Theory', in *Theoretical Physics in the Twentieth Century*, p. 82.
66. R. P. Feynman, *Phys. Rev.* **74**, 1439 (1948).
67. E. C. G. Stueckelberg and D. Rivier, *Phys. Rev.* **74**, 218, 986 (1948).

68. G. Wentzel, *Phys. Rev.* **74**, 1070 (1948).
69. J. Schwinger, *Phys. Rev.* **73**, 416 (1948).
70. See F. Villars, 'Regularization and Non-Singular Interactions in Quantum Field Theory', in *Theoretical Physics in the Twentieth Century*, pp. 89–92.
71. A. Pais and G. E. Uhlenbeck, *Phys. Rev.* **79**, 145 (1950).
72. See F. Villars, 'Regularization and Non-Singular Interactions in Quantum Field Theory', in *Theoretical Physics in the Twentieth Century*, pp. 83–84.
73. P. A. M. Dirac, *Proc. Roy. Soc.* A **180**, 1 (1942).
74. T. D. Lee, *Phys. Rev.* **95**, 1329 (1954).
75. See e.g., J. H. van Vleck, *The Theory of Electric and Magnetic Susceptibilities*, Oxford (1932).
76. The experimental susceptibility of one gram atom in 10^{-6} c.g.s. units is -1.88 and 19.6 for He and Ar, respectively, as compared to 46 and 233 to 252 quoted in Ref. [54]. See *Handbook of Chemistry and Physics*, Cleveland (1962), pp. 2731, 2735.
77. A. H. Compton, *Phys. Rev.* **21**, 483 (1923); P. Debye, *Physikal. Z.* **24**, 161 (1923).
78. Niels Bohr, 'Foreword', in *Theoretical Physics in the Twentieth Century*.
79. R. E. Peierls, 'Quantum Theory of Solids', in *Theoretical Physics in the Twentieth Century*, pp. 154–155.
80. Peierls' conclusion is based on the fact that he actually had seen Pauli's notes with the detailed calculation. In this calculation Pauli made use of exact conservation (which holds true in his first order calculation) but obtained a non-zero result by error (his solution was an Umklapp process which he did not recognize as giving a zero result).

    As seen from today, the problem is complicated by the fact that the rate of the 3-phonon process responsible for the absorption simultaneously determines, and is determined by, the thermal broadening and hence is selfconsistent, i.e. non-linear. I am grateful to Professor Peierls for an illuminating conversation on this problem. I was unable to find any trace of Pauli's notes in the Pauli archive at CERN. Very likely, Pauli discarded them after Peierls had clarified the problem.
81. R. E. Peierls, *Ann. Phys. (Leipzig)* **3**, 1055 (1929). This was Peierls' doctoral thesis which he started in Spring 1929 when he came to work with Pauli. After finishing his thesis Pauli asked him to become his assistant in October 1929.
82. R. E. Peierls, 'Quantum Theory of Solids', in *Theoretical Physics in the Twentieth Century*, pp. 155–159.
83. H. B. G. Casimir, 'Pauli and the Theory of the Solid State', in *Theoretical Physics in the Twentieth Century*, p. 137.
84. R. E. Peierls, 'Quantum Theory of Solids', in *Theoretical Physics in the Twentieth Century*, p. 140. In fact, in this paper, Pauli made the first application of Fermi–Dirac statistics to the theory of metals. See A. Sommerfeld and H. Bethe, 'Elektronentheorie der Metalle', in *Handbuch der Physik* (ed. by H. Geiger and K. Scheel), 2nd ed., Springer, Berlin (1933), Vol. 24, Part 2, p. 2. Sommerfeld's paper on the electron theory of metals, based on Fermi statistics, was published only in 1928 (*Z. Phys.* **47**, 1).
85. E. Fermi, *Z. Phys.* **36**, 902 (1926).
86. P. A. M. Dirac, *Proc. Roy. Soc.* A **112**, 661 (1926).
87. L. D. Landau, *Z. Phys.* **64**, 629 (1930).
88. W. Heisenberg, *Z. Phys.* **49**, 619 (1928).
89. See A. Nordsieck, W. E. Lamb, Jr., and G. E. Uhlenbeck, *Physica* **7**, 344 (1940), p. 353. I am grateful to Professor Uhlenbeck for information on this point.
90. See, e.g., *Fundamental Problems in Statistical Mechanics* (edited by E. G. D. Cohen), North-Holland, Amsterdam (1962).
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99. The first message of parity violation was contained in a letter of John Blatt to Pauli written in Princeton, which essentially read: 'We are all very shocked by the sudden death of parity'. I am grateful to Res Jost for this information. Neither this letter nor an accompanying letter of Villars are listed in the file of the Pauli correspondence in the Pauli archive at CERN.
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103. See N. Bohr, H. A. Kramers and J. C. Slater, *Z. Phys.* **24**, 69 (1924) where on p. 77 the following sentence can be found: 'Diese Unabhängigkeit [der Uebergangsprozesse] reduziert nicht nur die Erhaltung der Energie zu einem statistischen Gesetz, sondern auch die Erhaltung der Bewegungsgrösse....'
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116. See, e.g., T. D. Lee and C. S. Wu, *Annual Rev. Nucl. Sci.* **16**, Chapter 9 (1966).
117. The sheet of paper with Pauli's handwritten text and acknowledgement of execution ('erl.[edigt] 15.6.56/15.35 h als night letter') is contained in the folder with Pauli's notes for his neutrino talk, Ref. [68], in the Pauli archive at CERN.
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124. In the late forties Mach's picture also hung in Pauli's office in No. 4c of the physics institute of ETH. I am grateful to Res Jost for this information. In later years Pauli moved to room No. 3e and Mach's picture was moved to the discussion room of the institute for theoretical physics on the same floor. This and the adjacent room of Pauli's assistant were decorated by historic group pictures of physics meetings. All these photographs are now in the Pauli archive at CERN.
125. Arthur Koestler, *Die Wurzeln des Zufalls*, Scherz, Bern-München-Wien (1972), p. 98.
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128. Private communication from Mrs F. Pauli. Unfortunately, this address has left no trace in the published literature.*

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*See, however, the footnote on p. 213 of Ref. [13] which mentions Einstein's address and gives the date of this dinner: 10 December 1945.

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