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From the necessary to the possible: the genesis of the spin-statistics theorem

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Abstract. The spin-statistics theorem, which relates the intrinsic angular momentum of a single particle to the type of quantum statistics obeyed by a system of many such particles, is one of the central theorems in quantum field theory and the physics of elementary particles. It was first formulated in 1939/40 by Wolfgang Pauli and his assistant Markus Fierz. This paper discusses the developments that led up to this first formulation, starting from early attempts in the late 1920s to explain why charged matter particles obey Fermi-Dirac statistics, while photons obey Bose-Einstein statistics. It is demonstrated how several important developments paved the way from such general philosophical musings to a general (and provable) theorem, most notably the use of quantum field theory, the discovery of new elementary particles, and the generalization of the notion of spin. It is also discussed how the attempts to prove a spin-statistics connection were driven by Pauli from formal to more physical arguments, culminating in Pauli's 1940 proof. This proof was a major success for the beleaguered theory of quantum field theory and the methods Pauli employed proved essential for the renaissance of quantum field theory and the development of renormalization techniques in the late 1940s.

As witnessed by the title of the Solvay conference, the world in 1927 was considered to consist of electrons and photons (the term electrons here refers to charged particles in general and thus also to protons, which were at the time still frequently called "positive electrons"). No matter what precise ontology one ascribed to either, how one thought about their dual or complementary wave-particle nature, these entities formed the essential building blocks of microscopic nature[1].

Among other things, these two entities were distinguished by the statistics that was applicable to them. Photons obeyed Bose-Einstein statistics, as established after

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[1] There was of course space itself or the gravitational field, but this was empirically irrelevant in the microscopic world and hardly studied in the context of quantum mechanics at this time. It consequently plays only a small, albeit interesting, role in my story and I will return to it in due time.

Bose's successful derivation of Planck's law in 1924 [Bose 1924], while electrons (and protons) obeyed the Pauli exclusion principle, which, as Fermi and Dirac had shown in 1926, meant that they obeyed another type of novel quantum statistics, now known as Fermi-Dirac statistics [Dirac 1926; Fermi 1926].

These two statistics could be implemented in Schrödinger's wave mechanics by taking many-particle wave functions which were fully symmetric (Bose-Einstein) or fully antisymmetric (Fermi-Dirac) under the exchange of two particles. Dirac had further shown that these were in fact the only two possibilities of constructing many-body wave functions of "similar" particles. However, as he pointed out, "[t]he theory at present is incapable of deciding which solution is the correct one" (p. 662).

The first part of this paper is devoted to early attempts, in the years 1927 to 1931, at demonstrating why the assignment observed in the fundamental building blocks of nature, photons obeying Bose-Einstein statistics, electrons and protons obeying Fermi-Dirac statistics, is in fact the only possible assignment. Already at this time, quantum field theory began to play an essential role in discussing the statistics of the fundamental entities.

The difficulty of possible statistics assignments never formed a separate research strand, but was rather treated as an interesting aside in works dedicated to other problems. After the years 1932–1934, the rather limited question of why the three fundamental entities necessarily had to obey the statistics they did was replaced by a new one. These years saw the discovery and theoretical prediction of new elementary particles, the positron, the neutron and the neutrino. With the discovery of further new particles now a plausible, even anticipated, possibility, and thus the theoretical prediction of new particles a legitimate theoretical method, the question arose, which statistics one was allowed to assign to a newly discovered or hypothetical particle.

In order to map out such a general space of possibilities, it was necessary to associate the statistics obeyed by a particle with a different, independent physical property of the particle. Early on, this independent physical property was identified as the particle's spin. This was only possible through a shift in the meaning of the concept of spin: From a specific property of electrons and protons, it now came to be interpreted as a fundamental quantity, defined for any particle (even if its value was zero), closely related to that particle's transformation properties under Lorentz symmetry.

This development, which began in 1934, when Pauli and Weisskopf realized that particles without spin would necessarily obey Bose-Einstein statistics, culminated in Pauli's 1940 formulation of the spin-statistics theorem, which established the general principle that all particles with half-integer spins were necessarily fermions, while all particles with integer spin were bosons[2]. This development is described in the second, larger part of this paper.

There have been some treatments of Pauli's formulation of the spin-statistics theorem. All of them provide very useful background reading for this paper. [Duck and Sudarshan 1997] is mainly concerned with the validity of the different arguments for the spin-statistics connection and takes both the question and the concepts employed in these arguments for granted. Thus, although it gives an analysis of historical arguments, its approach is more philosophical and rather ahistorical, giving hardly any context. [Tomonaga 1997] has a whole chapter on the spin-statistics theorem and provides a few insightful historical comments, but its main focus is on a pedagogical presentation of Pauli's argument. [Massimi 2005], finally, treats the spin-statistics

[2] I will be using the anachronistic designations "fermion" and "boson" throughout, in order to avoid the cumbersome "particle obeying Bose-Einstein/Fermi-Dirac statistics", even though the terms were only coined after the spin-statistics theorem provided the theoretical foundation for cleanly dividing all the particles of the world into these two categories.

theorem only in its relation to the exclusion principle and Fermi-Dirac statistics. It thus focuses entirely on the statistics part of the theorem, and says very little about the spin.

This paper goes beyond these works, by addressing both aspects of the spin-statistics theorem and discussing in detail the conceptual developments that made possible and motivated its formulation. It thereby also allows a new look at the history of quantum field theory in the 1930s, which focuses not primarily on the conceptual difficulties, such as the question of divergences, but rather looks at some of the conceptual developments which led up to the formulation of renormalized quantum electrodynamics (QED) in the late 1940s, and thus forms a first step towards understanding those later developments not simply as few brilliant, pragmatic, post-war physicists realizing how one should have (and could have) been doing QED all along, but rather as the continuation of conceptual advances in quantum field theory, which continued even after the grave fundamental difficulties of the program were realized in the late 1920s.

1 Why photons are bosons and electrons are fermions

Dirac was not the only one who considered the undecidability between Bose-Einstein and Fermi-Dirac statistics as a defect of quantum mechanics. Werner Heisenberg stated at the 1927 Solvay conference:

> There is no reason, in quantum mechanics, to prefer one statistics to another. [...] We feel nevertheless that Einstein-Bose statistics could be more suitable for light quanta, Fermi-Dirac statistics for positive and negative electrons. The statistics could be connected with the difference between radiation and matter... [Bacciagaluppi and Valentini 2009, p. 500]

It should be noted that, knowing the statistics of the fundamental building blocks, it was of course possible to derive the statistics of composite systems from those of their constituents and thus make general statements concerning the connection between the statistics of composite particles and other physical properties. A spin-statistics theorem for composite objects thus directly followed from the fact that the elementary fermions all had a spin of 1/2 [3].

As Heisenberg also pointed out, the fact that both of the two elementary charged particles were fermions implied that all composite neutral particles would necessarily be bosons, and thus, since the one fundamental neutral particle, the photon, was a boson, all neutral particles would be bosons[4]. But this gave no indication as to how the necessity of a certain statistics for the fundamental particles might be formulated mathematically.

Such an indication had in fact, unwittingly, been given by Dirac earlier that year, in a first attempt at formulating a quantum mechanical theory of electrodynamics. His presentation of the new quantum theory of radiation [Dirac 1927] began with and was largely based on a novel way of implementing Bose-Einstein statistics into quantum mechanics, different from the use of symmetric Schrödinger wave functions in many-body configuration space. Dirac demonstrated that one could interpret the one-particle Schrödinger wave function as instead describing an ensemble of non-interacting particles (i.e., an ideal gas). The squared Fourier amplitudes then did not

[3] See for example Pauli's famous "neutrino" letter from 4 December 1930, in which such an argument is made explicitly.

[4] This idea of a "charge-statistics theorem" was of course not pursued any further, when developments in nuclear physics, in particular the discovery of the neutron, invalidated its basic presuppositions.

represent the probability of one particle being in the corresponding stationary state, but rather gave the fraction of particles in the ideal gas that occupied that state. The resulting ensemble was then, however, a Boltzmann ensemble. In order to implement Bose-Einstein statistics, one could quantize the theory again – the Fourier coefficients of the wave function now themselves became non-commuting q-numbers acting on a new wave function, defined not in space but in "occupation number space", i.e., its arguments were now the number of particles for each stationary state. Dirac could show that the new many-particle theory which emerged from this second quantization now described a Bose-Einstein ensemble. He showed that this second-quantized theory was in fact equivalent to the older theory of symmetrical wave functions.

This reformulation was not a means in itself for Dirac: It allowed him to construct a quantum theory of electrodynamics formulated as a theory of an ensemble of light quanta obeying Bose-Einstein statistics. Dirac's method was reinterpreted by Pascual Jordan. He read it not as a second quantization of a quantum theory, moving from Boltzmann to Bose-Einstein statistics, but as the (first) quantization of Schrödinger's wave mechanics interpreted as a classical field theory. One of his guiding principles was the necessity of a symmetric representation of matter and radiation. Consequently, he believed that if this method was to be applied to electrodynamics, it should also be applied to matter waves [Cini 1982; Darrigol 1986; Lehner 2011]. In order to account for the fact that electrons obeyed Fermi-Dirac statistics, he developed a novel formalism of field quantization based on anti-commutators rather than on the canonical commutators of quantum mechanics [Jordan 1927; Jordan and Wigner 1928]. This was possible in his transformation theory, which was less closely tied to classical mechanicss and thus allowed for a commutation algebra which was not perfectly analogous to classical Poisson brackets.

Dirac objected to Jordan's approach: One could not simply invent a new quantization method if the regular ("natural" in Dirac's words at the 1927 Solvay conference) method did not work because the particles involved obeyed Fermi statistics [Bacciagaluppi and Valentini 2009, p. 501]. The difference between photons, which corresponded to classical fields, and electrons, which corresponded to classical particles, had to be deeper than merely two different quantization procedures of classical field theories, a point which he repeated emphatically in another paper on quantum electrodynamics five years later [Dirac 1932][5]. For Jordan, on the other hand, and this was to be the dominating view in the next years, Dirac had a developed a method of moving from a classical field theory to a corresponding quantum (field) theory, and one could devise different quantization methods, depending on which statistics the (emergent) particles of the quantum theory were supposed to obey.

Initially, this did not much change the status of the two different statistics. Instead of (or rather, along with) two different symmetrization rules for the many-particle wave function, there were now two different quantization procedures for a classical field theory[6]. In either case, the only way how to tell which of the two was to be used was empirical input.

In the summer of 1927, Pauli and Jordan revisited the question of quantizing the electromagnetic radiation field, restricting themselves to the free field, but taking great care to conserve the relativistic covariance of the classical theory [Jordan and Pauli 1928]. It was of course clear that light quanta would have to obey Bose-Einstein

[5] See also Kojevnikov [2002].

[6] It should be noted at this point that either way of looking at the problem entirely divorces it from any considerations relating to actual statistical mechanics. Already in 1928, Dirac critically remarked that the use of the term "statistics" to describe the behavior of systems composed of many identical particles was in fact misleading, since it has no immediate connection with the notion of a statistical ensemble [Dirac 1929].

statistics, so there was no ambiguity as to which quantization procedure to use. However, as Jordan later mentioned in a review article, he also soon (after finishing his work on fermionic quantization with Wigner) checked what would happen if one attempted to quantize classical electromagnetism to obey Fermi-Dirac statistics [Jordan 1928]. And he claimed to have found that this did indeed lead to a contradiction, i.e., that the classical field theory of electromagnetism could only be quantized according to Bose-Einstein statistics, or as Jordan put it: "...only the Bose quantization mathematically fits to the form of Maxwell's field equations" (p. 206). Jordan was extremely vague about how the classical field theory and the quantization procedure would clash. He only explained in a footnote:

> In that case [fermionic quantization] there would appear instead of the so-called relativistic Δ-function, which is an analogue of the Dirac δ-function, a different singularity, which is not expressible through well-defined, simple integral properties.

Jordan was referring to the Δ-function (nowadays known as the Pauli-Jordan function), which appears in the commutator of two field quantities, e.g., the commutator of two components of the electric field $\mathbf{E}$ at two different space-time points P and P', found by Jordan and Pauli to be of the form

$$[E_i(P), E_k(P')] = \frac{i\hbar c}{4\pi}\left(\partial_i\partial_k - \delta_{ik}\frac{1}{c^2}\partial_t^2\right)\Delta(P'-P) \tag{1}$$

where the Δ-function is defined, as mentioned by Jordan, through the "well-defined, simple integral properties":

$$\int_{V_4} f(\mathbf{r},t)\,\Delta(\mathbf{r},t)\,d^3r dt = \int_{V_3^+} f\left(\mathbf{r}, t=-\frac{r}{c}\right)\frac{1}{r}d^3r - \int_{V_3^-} f\left(\mathbf{r}, t=\frac{r}{c}\right)\frac{1}{r}d^3r \tag{2}$$

where f is an arbitrary function, V_4 is all of space-time and $V_3^{\pm}$ are the past and future light cones, respectively. The commutation relation for the field quantities is obtained by setting the commutation relations for the Fourier components to be the commutation relations for the raising and lowering operators of a harmonic oscillator.

Jordan offered no hint on how he had gone about constructing a fermionic anti-commutator and consequently, his statement does not amount to much more than the realization that one runs into difficulties when trying[7]. Jordan was anyway quite quick to jump to conclusions in this article – he also claimed that the matter field (described by the Dirac equation) could only be quantized fermionically, in this case offering no rationale at all.

This second statement of Jordan's was rebuffed by Pauli on several occasions: In his review of the issue of "Ergebnisse der exakten Naturwissenschafen" in which Jordan's article had appeared [Pauli 1929] as well as in his major work on interacting quantum electrodynamics with Heisenberg [Heisenberg and Pauli 1929]. He also mentioned Jordan's "großen Unsinn" in a letter to Oskar Klein (18 February 1929) [Hermann et al. 1979], in which he counted the fact that Jordan was wrong, i.e., that there was no

[7] In any case, Jordan's formulation of these difficulties is imprecise at best: When quantizing the electromagnetic field with anti-commutators there are two possibilities. Using an expansion of the field in complex plane waves, one gets instead of the Pauli-Jordan function the so-called Δ_1 function, which can be expressed by simple integral properties, as pointed out by Olivier Darrigol in his unpublished Ph.D. thesis. Using instead the expansion in sines and cosines, as done by Jordan and Pauli in their paper, one gets the Δ function, just as for the case of commutators, but with a different argument: $P+P'$ instead of $P-P'$. I would like to thank Olivier Darrigol for a very interesting discussion on this subject.

apparent way to choose between bosonic and fermionic quantization for matter waves (besides experimental evidence for the latter), as one of the four main conceptual difficulties in quantum electrodynamics (along with the electron's self energy, the difficulty of negative energies in the Dirac equation, and the fact that there were three "logically independent" fields: the matter waves of electrons and protons, and the electromagnetic field).

Implicitly, this meant that he accepted Jordan's first statement, that the electromagnetic field could only be quantized bosonically. But neither here, nor in his work with Heisenberg, did he make this statement explicit: He did however always mention that matter waves could be quantized either way, and since the specific proof that this was possible clearly failed for the electromagnetic field, it could also be read as a proof that the electromagnetic field could only be quantized bosonically.

This proof, that the matter field can be quantized fermionically (the general proof that all fields can be quantized bosonically had been given in the first three sections), can be found in the fourth section of Heisenberg and Pauli's first paper on QED. It is based on the premise that it is essential for the quantum field theory to be a consistent quantum theory. It is thus not a proof specific to quantum field theory or the fact that a theory of electrodynamics should be relativistic. It only needs to postulate that the following relation, well-known from quantum mechanics, holds:

$$\frac{\partial F}{\partial t} = \frac{i}{\hbar}\,[H, F] \tag{3}$$

where H is the Hamilton (energy) operator and F is an arbitrary function(al) of the field coordinates and momenta, and their derivatives. From this relation, one can derive both the conservation of energy and the time-independence of the canonical commutation relations, justifying its essential status: The former is derived from the fact that any operator (and hence H) commutes with itself, the latter is derived from the fact that the canonical commutators are c-numbers and hence commute with any operator. The above relation, however, holds in a fermionically quantized theory (where the canonical variables obey anti-commutation instead of commutation relations) only if the Hamiltonian H is of the form "linear function of the field momenta (and their derivatives) times linear function of the field coordinates (and their derivatives)". It therefore does not hold for the case of electromagnetism, where the Hamiltonian is quadratic in the field momenta. This latter fact is, however, not stated explicitly be Heisenberg and Pauli.

It was first stated explicitly in a paper by Leon Rosenfeld, who was Pauli's assistant at the time [Rosenfeld 1930]. In this paper, Rosenfeld was concerned with the quantization of the gravitational field according to the quantization procedure Heisenberg and Pauli had used for the electromagnetic and matter fields. The main issue was how to deal with the redundant (gauge) degrees of freedom, both in electrodynamics and general relativity, but Rosenfeld also briefly touched on the question of how the gravitational field should be quantized – after all, here there was, as opposed to QED, no experimental input as to which statistics the quanta of the gravitational field should obey. Rosenfeld concluded that, since the Hamiltonian of the gravitational field was quadratic in the canonical field momenta, the above relation (which Heisenberg and Pauli had shown to hold for fermionically quantized matter) would not hold for fermionically quantized "gravitons", and one had no choice but to quantize the gravitational field bosonically. This was stated as a theorem for general field theories in a series of lectures which he gave at the Institut Henri Poincaré one year later [Rosenfeld 1932].

At this point, Rosenfeld thus presented a criterion applied to the classical field theory (to its Lagrangian or Hamiltonian) with which it was possible to test whether that theory could be quantized fermionically or not. However, as he himself pointed out, this theorem was still lacking in two major respects: First, the criterion was purely formal, i.e., there was no physical interpretation of the required special form of the Hamiltonian. And second, the criterion only determined whether fermionic quantization was possible – it could not decide whether bosonic or fermionic quantization was to be applied in a given case. Rosenfeld noted that, apparently, whenever fermionic quantization was possible it was also realized in nature, but could not explain this fact. Pauli's main worry remained.

In the following years, matter described by the Dirac equation became more and more closely wedded to Fermi-Dirac statistics and the Pauli principle: Dirac's hole hypothesis [Dirac 1930] and the discovery of the positron in 1933 made the idea of a bosonic electron more and more unthinkable. Several works dealt with the description of electrons and positrons in a second-quantized framework in the years after 1930 [Fock 1933; Furry and Oppenheimer 1934; Heisenberg 1934; Weyl 1931] – none of them even mentioned the possibility that the electron field might also be quantized bosonically.

This is not to say that there was any notion that the negative energy states of the Dirac equation formally implied or were even equivalent to fermionic quantization – it was just that hole theory and thus the newly discovered positron could not be incorporated into a bosonic quantum field theory of matter, and hence the possibility of bosonic quantization was apparently not even entertained. The most explicit statement of what hole theory meant for the necessity of the assignment of statistics was given, unsurprisingly, by Pauli, in his comments on Dirac's talk on hole theory at the 1933 Solvay conference [Miller 1994, p. 142]:

> The theory of holes always seemed very interesting to me on account of the essential role played in it by the exclusion principle. Whereas this principle was formerly only an isolated rule, of which the validity was independent of those of the other bases of quantum theory, the theory of holes, introduced by Dirac in order to escape the difficulty of negative masses, would have been impossible if we had not wished to exclude all wave functions that are not antisymmetric.

At around the same time, in his celebrated *Handbuch* article [Pauli 1933, p. 258], Pauli also for the first time came out with an explicit argument for why the electromagnetic field needs to be quantized bosonically: Since here the field quantities themselves are observable, they need to fulfill commutation relations, just like any other quantum-mechanical observable. An analogous argument, he explained, did not hold for the electron, whose wave function, the field being quantized, was not a direct observable.

The story might end here: There are photons described by Maxwell's equations, which are necessarily quantized as bosons, and the fundamental massive particles, described by the Dirac equation, which necessitates fermionic quantization in order to get rid of the negative energy states. But a central tenet of this resolution was being called into doubt, namely that the Dirac equation was the one equation describing the fundamental particles. This doubt had three distinct origins:

1. The difficulties of the Dirac equation, i.e., the negative energy states. They were taken as an indication that it was not the final word on the equation describing electrons. This view became rarer after the discovery of the positron and the vindication of Dirac's hole hypothesis; but hole theory did leave several physicists, in particular Pauli, quite unsatisfied and searching for a different theoretical description of electrons (and, after 1933, positrons).
2. The actual discovery of new particles. While the positron naturally had to be described by the Dirac equation, and the neutron was also generally taken to be a spin 1/2 fermion in order to explain the properties of nuclei[8], the discovery

[8] Bethe and Bacher in their famous review article [Bethe and Bacher 1936, p. 91] state: "The neutron spin might, from experimental evidence, be just as well $\frac{3}{2}$ as $\frac{1}{2}$. However,

of these particles expanded the basic electron-photon horizon of 1927 and made the existence of further fundamental particles, possibly obeying different wave equations, thinkable.
3. The difficulties of nuclear physics. These were taken as an indication that in the nucleus quantum mechanics had to be if not entirely abandoned then at least heavily modified. A further indication of this was that the Dirac equation did not even seem to hold for the other fundamental charged particle, the proton, as it was discovered in 1933 that the proton had an anomalous magnetic moment [Tomonaga 1997]. In the attempt to describe nuclear phenomena, new theoretical entities, the neutrino and Yukawa's meson, were postulated, which were not necessarily described by the Dirac equation.

Although these three points can of course not be cleanly separated, they do roughly correspond to three independent heuristics, which demarcated the space of possible descriptions of a new particle, when such a particle, the cosmic ray meson, was discovered in 1937. We will discuss these three points in the next three sections, and then turn to the discovery of the meson, which in a way united all three strands, paving the way for Pauli's formulation of the spin-statistics theorem.

2 Improving the Dirac equation

The most prominent critic of the Dirac equation, and of hole theory in particular, was Pauli. He was unsatisfied with hole theory from the start. What bothered him most was the fact that it appeared to place the asymmetry between positive and negative charge in the initial conditions (i.e., the occupied negative energy states) rather than in the physical laws (Letter to Heisenberg, 16 June 1933 [Hermann et al. 1985]); after the discovery of the positron, he still hoped that it might turn out to be a boson (Letter to Peierls, 22 May 1933), thus contradicting Dirac's hypothesis[9]. And when his new assistant Victor Weisskopf pointed him to a curiosity in the behavior of the Klein-Gordon equation, the spinless (scalar), relativistic matter-wave equation, which had largely been abandoned in favor of the Dirac equation, he immediately saw this as a possibility to show that hole theory was not necessary to describe electrons and anti-electrons.

Weisskopf had been working on a review article for "Die Naturwissenschaften" on the current difficulties of electron theory, in which he led from the Schrödinger equation to the Dirac equation via the Klein-Gordon equation [Weisskopf 1935]. In discussing the Klein-Gordon equation, he realized that the main reason it had been abandoned was that it predicted negative probability densities. It was, however, now widely accepted that a single-particle probability density could no longer be defined (due to pair annihilation and creation) in hole theory. The only sensible object one could talk about was charge density. And with electron-positron theory one actually wanted to have a charge density which could have both signs. He discussed this with Pauli[10]

simplicity is a strong argument in favor of the value $\frac{1}{2}$ which we shall, therefore, assume throughout this article." This appears to have been a general consensus, even before Julian Schwinger [1937] brought forth empirical arguments from neutron-hydrogen scattering for assigning a spin of 1/2 to the neutron.

[9] Interestingly, he immediately associated its being a boson with it having an integer spin, based on the evidence from atomic physics (the hydrogen atom) and thus on the type of proto-spin-statistics reasoning mentioned in the beginning of the last section.

[10] Interview of Weisskopf by Thomas Kuhn and John Heilbron on 10 July 1963, Niels Bohr Library & Archives, American Institute of Physics, College Park, MD USA, http://www.aip.org/history/ohilist/4944.html.

and out of this realization Pauli-Weisskopf theory developed [Pauli and Weisskopf 1934]. They found that not only could the charge density become negative, but also the energy density was always positive (if one treated the Klein-Gordon equation as a classical field equation, and not as a relativistic analogue of the Schrödinger equation). One could thus have a theory which needed no infinite Dirac sea (since the energy density was positive definite), but could still accommodate positrons and pair creation through second quantization (since the charge density was not positive definite, as opposed to the Dirac case). Pauli famously called their paper the "Anti-Dirac Theory" [Pauli 1936c].

Since one did not need the Dirac sea, it certainly was not necessary to quantize fermionically. But Pauli and Weisskopf realized that it was not even possible. Now actually, they could have immediately seen this by applying Rosenfeld's criterion to the Klein-Gordon field theory (The Klein-Gordon Hamiltonian is quadratic in field momenta and coordinates). Since they did not do this, we have to conclude that no one had really noticed that Rosenfeld had done more than restate the fact that the electromagnetic field could not be quantized fermionically. Sadly, just as Jordan had been six years earlier, Pauli and Weisskopf were initially terribly sketchy about how they had reached the conclusion that the Klein-Gordon field could not be quantized fermionically, both in their paper and in private communication (Letter from Pauli to Heisenberg, 7 November 1934).

I will try to reconstruct their original argument at least somewhat (they did offer a bit more information than Jordan had) and see how it relates to Rosenfeld's criterion. In fact, they give two separate ways of arguing, which they claim to be related. The first one is very close to Rosenfeld's argument. Due to the structure of the Dirac Lagrangian, the complex conjugate field ψ^* is the canonically conjugate variable to ψ. The two quantities therefore do not anti-commute. For the Klein-Gordon case, the field and the complex conjugate field are not canonically conjugate variables – they are two independent field coordinate degrees of freedom, and should therefore anti-commute upon fermionic quantization. Just as in the Dirac case, the field also anti-commutes with itself. One can quickly see that this leads to a contradiction. Starting from the conditions just outlined

$$\psi(x)\psi(x') + \psi(x')\psi(x) = 0 \quad \text{and} \quad \psi(x)\psi^*(x') + \psi^*(x')\psi(x) = 0 \tag{4}$$

one gets[11]

$$(\psi(x) + \psi^*(x))^2 = 0. \tag{5}$$

Thus $\psi(x) + \psi^*(x)$ is hermitian (by construction) and nilpotent. As a hermitian operator it can be diagonalized, as a nilpotent operator it only has zero eigenvalues, thus it is the zero operator and $\psi(x) = -\psi^*(x)$. The same rationale holds for the anti-hermitian operator $\psi(x) - \psi^*(x)$, thus $\psi(x) = \psi^*(x)$. It follows that $\psi(x) = 0$, i.e., the theory is trivial. This elementary contradiction is very closely related to Rosenfeld's criterion; it is the same structural property of the Hamiltonian which is responsible for the contradiction in both cases. Pauli and Weisskopf, however, did not try to make this realization into a general argument. Instead they tried to go beyond structural arguments and understand the physical reason why the Klein-Gordon field could not be quantized fermionically.

[11] This step, going from the field operators at different points to the field operators at the same point, is actually non-trivial and the singular behavior when taking this limit was not investigated by Pauli and Weisskopf. Their proof (and later proofs discussed in this paper, which employ a similar limiting argument) was thus not rigorous by the later standards of mathematical physics and axiomatic quantum field theory. These questions were thus revisited after the rise of axiomatic QFT in the 1950s.

And here it was the relativistic invariance of the theory that moved to the center of attention. Pauli and Weisskopf realized that if they started from a Hamiltonian expressed through the Fourier coefficients of the field (which was not manifestly relativistically invariant), then imposed anti-commutation relations on these Fourier coefficients and from here reconstructed the full field theory, the theory turned out not to be relativistically invariant[12]. This relativistic non-invariance first manifested itself in non-invariant field commutation relations.

Pauli and Weisskopf were, however, looking for an even more physical expression of the incompatibility. They claimed to have reconstructed from the anti-commuting Fourier coefficients not only the field strengths, but the observable charge-current density four-vector, and found that this quantity no longer had the right (Lorentz) transformation properties. Thus, not only were the more abstract commutators no longer invariant, but also observable quantities were no longer covariant[13].

Through Pauli and Weisskopf's work, the analysis of the fact that certain field theories cannot be quantized fermionically was taken to a new level: First, the incompatibility was moved from structural properties of the classical field equations to physical arguments, in particular relativistic covariance. And second, the question was now closely tied to spin: Even if the original reason for rejecting the Klein-Gordon equation (density not positive definite) was no longer valid in a world of quantized matter waves and pair creation, it was still deficient since it did not describe the electron's spin. So on the one hand one had Dirac's theory, which described spin and needed an infinite fermionic sea of negative-energy particles to describe pair creation, on the other hand one had the Pauli-Weisskopf theory, which did not describe spin, could describe pair creation without a Dirac sea and could only be quantized bosonically. Pauli wrote to Heisenberg: "An important and interesting point is the fact that our theory can only be performed with Bose statistics, because here a necessary connection between spin and statistics is beginning to dawn."

Pauli expanded on the arguments for the impossibility of fermionically quantizing the Klein-Gordon field in a talk he gave on Pauli-Weisskopf theory a few months later, in March 1935, at the Institut Henri Poincaré (as mentioned in a letter to Klein from 17 July 1935). In this talk he treated the question of fermionic quantization in detail [Pauli 1936b].

It is interesting to note at this point that as isolated elements of the spin-statistics theorem were discovered they were mostly treated as novelties, certainly not worthy of being treated in a publication of their own, or even of being expanded on at great length in a major publication: Jordan only mentioned his results in a review article, Pauli and Rosenfeld expanded on them only in lecture series or (Pauli) as a sidenote in a review article.

[12] This procedure would also already have worked for the electromagnetic field; in fact it would have been the natural starting point for Jordan in constructing a fermionic electrodynamics. Strangely, no-one seems to have considered it.

[13] It is not clear to me, how to reconstruct the charge-current density four vector from the Fourier coefficients of the *total* charge and current, and hence I have not been able to reproduce their result. However, one can quite easily reconstruct the total charge and current (i.e., the space integrals of the density vector) and find that they do not have the right transformation properties (scalar and three-vector, respectively). In any case, this reasoning never shows up in print again and is replaced by other arguments, possibly indicating that it was not as conclusive as Pauli and Weisskopf originally thought. Another argument Pauli and Weisskopf possibly pursued can be found on an undated manuscript in the Pauli Archives at CERN (5/486), where an argument is attempted that the impossibility of fermionic quantization arises due to the interaction with an external field. This argument is, however, never found in print and may even possibly be from a later date.

In beginning his talk, Pauli specified which properties one should expect from a quantum theory a priori. These considerations were inspired by Dirac, who had used such reasoning to reject the Klein-Gordon equation and derive his own relativistic wave equation. Dirac's a priori conditions (positive definite density, first order wave equation) were, as Pauli pointed out, however, only applicable to a one-particle theory. For a many-particle quantum matter-field theory, in which pair creation could be described, Pauli suggested positing two different conditions: A charge density with varying sign (to describe pair creation) and a positive definite energy density. Both of these conditions were conditions on the *classical* field theory. They were satisfied in the Klein-Gordon theory – the Dirac theory needed the additional postulate of the Dirac sea. Pauli only very briefly touched on the Dirac theory, but from his brief comments one can surmise that he was now thinking differently about hole theory, a change of view already hinted at by his 1933 Solvay comments cited above: No longer as a ludicrous hypothesis to save a flawed theory, but as a necessary consequence of having a quantum field theory using the Dirac equation obey his two axiomatic conditions[14]. This understanding was to form an important element in moving from theorems on the impossibility of quantizing certain field theories fermionically (as had been the case so far), to a general spin-statistics theorem, in which also the impossibility of quantizing the Dirac field and others bosonically could be demonstrated.

For the time being, however, Pauli was mainly concerned with expanding on his findings concerning the Klein-Gordon field. As already mentioned, the Klein-Gordon field fulfilled Pauli's two conditions, already as a classical field theory. Pauli then added a third condition referring to the quantum theory proper: The charge densities at two different points with a space-like separation should commute. Using this condition he then set himself to derive that the Klein-Gordon field could not be quantized fermionically.

The condition that the charge densities at different space points commute with each other is certainly a new type of argument, moving still further away from the formal (Lorentz covariance properties) towards the physical, most related (in its appeal to observability) to the argument in the *Handbuch*, but a lot more general in that it applies, in particular, to a complex scalar field, which by itself is just as unobservable as the electron wave function. In his talk, Pauli himself did not give much motivation for choosing this condition, merely stating that it was necessary in order to be able to speak of a measurable charge density at all. In his later papers on the connection between spin and statistics, where this commutation condition (applied now in general to all observable quantities) continued to play an important role, he got more specific:

> The justification for our postulate lies in the fact that measurements at two space points with a space-like distance can never disturb each other, since no signals can be transmitted with velocities greater than that of light [Pauli 1940, p. 721].

It was thus later identified as an argument based on the relativistic notion of causality [Weisskopf 1983] or microcausality [Massimi 2005], even though Pauli himself does not seem to have used that word at the time[15]. It is, however, a very specific argument from

[14] One difficulty still remained: At the time, there was still the conviction that one could incorporate hole theory and the Dirac Sea in the "first-quantized" theory, i.e., fulfill Pauli's conditions without second quantization, leading to the elaborate scheme of subtraction physics. This still made hole theory seem to be an additional hypothesis on the classical theory, rather than a necessary element for a sensible quantum theory. Only gradually did it become clear that talking about the Dirac Sea only made sense in the second-quantized theory.

[15] In a talk he gave for the Zurich philosophical society in 1936 [Pauli 1936a], he certainly used the term causality in the sense of determinism.

causality: Rather than argue from the propagation velocities of the physical entities (be they particles or waves) and demanding that they not propagate faster than the speed of light, Pauli only invoked measurements at different space-time points which should not influence each other, if they are space-like separated. As he later explained in a letter to Dominique Rivier, a student of Stueckelberg's, he adopted this specific notion of causality in order to avoid problems concerning the localizability of particles in theories where those particles (be they electrons or photons) could be annihilated or created (Letter to Rivier, 3 August 1948 [von Meyenn 1993]).

In order to derive something from his causality condition, Pauli first had to deal with one problem: He and Weisskopf had realized that already the construction of a charge density with the correct transformation properties would be difficult in the fermionic theory. Such reasoning, however, was always based on the presupposition that observables would have the same form (either as functions of the field variables or as function of the Fourier coefficients) in a bosonic and a fermionic theory. It was, however, quite possible that the observable charge density would have to have a different form in either case.

This serves as an illuminating example of how one is to understand Pauli's program of moving from formal to physical arguments. One might, after all, be inclined to say that, e.g., it makes no sense to talk about physical inconsistencies if a theory is already shown to be quantized in the wrong manner and is thus formally inconsistent[16]. Pauli's method was, however, to initially loosen the formal requirements and then show that more physical arguments still lead to the desired conclusions. In this case, that meant dropping the very narrow definition of the charge-current density vector of the Klein-Gordon field, originally derived (or rather justified) by Gordon as the derivative of the Hamiltonian with respect to an external electromagnetic potential [Gordon 1926]. Instead, Pauli took a much looser formal definition of the charge-current density: All he demanded was that it be a hermitian vector fulfilling the continuity equation.This allowed him to give a more general form of the charge density and then narrow down the new realm of possibilities through the causality condition.

Pauli split the regular (Gordon) four-current density S of the bosonic theory into three summands, S_1, S_2, S_3, corresponding to the current due to particles, anti-particles and pair production[17]. All three summands are hermitian, covariant and satisfy the continuity equation by themselves. Pauli then generalized the four-current to the form

$$S = c_1 S_1 + c_2 S_2 + c_3 S_3. \tag{6}$$

In the bosonic theory, all three coefficients c_i are equal to 1. Pauli now allowed for them to be arbitrary real numbers and identified c_1 as the charge of the particle, c_2

[16] This is how Olivier Darrigol in his unpublished Ph.D. thesis interprets later criticism of Pauli's argument by de Wet.

[17] This splitting is quite problematic, as Pauli himself conceded. Pauli was heavily scolded for this by Duck and Sudarshan sixty years later [Duck and Sudarshan 1997]. It seems to me, however, that they are slightly missing the point: Pauli could easily have shown that the charge densities at different points do not commute, by simply assuming that the charge density has the same functional form in the fermionic theory, i.e., that the functional form of the charge density is entirely determined by the classical field theory and is independent of the quantization procedure used (using Gordon's justification). Pauli was trying to go beyond this, by allowing for alternate expressions for the charge density. That he had to use somewhat questionable mathematics to obtain such alternate expressions only goes to show how restricted one is in constructing the charge density from the classical field theory. A similar point is made in a review of Duck and Sudarshan's book by Arthur S. Wightman [Wightman 1999]. Wightman also points out that the problem is rather that Pauli by no means showed that he had exhausted the possibilities of alternate expressions for the charge density.

as the charge of the anti-particle and c_3 somewhat vaguely as "the frequency of pair production processes." On this generalized current, Pauli now imposed the condition that the commutator of the charge density at two different space points should vanish. He could show that this uniquely led to $c_1 = c_2 = c_3 = 1$ for the bosonic theory and to $c_1 = c_2 = c_3 = 0$ for the fermionic theory, furnishing the first proof (based on Pauli's conditions) that the Klein-Gordon field could not be quantized fermionically.

Pauli was aware that his conditions might still be too restrictive: He mentioned the possibility of abandoning the commutability of the charge density at different space points, but stated that this would lead to difficulties when coupling to an electromagnetic field. In his correspondence with Klein, he also discussed the possibility of having the charge-current density be non-hermitian, but felt that path to be "ziemlich phantastisch" (Letter to Klein, 7 September 1935). All in all, he was quite convinced of having conclusively proven his statement.

Pauli-Weisskopf theory was not at all meant to be a realistic theory. In his talk, Pauli stated: "One has to admit that it is not certain that one can apply the theory in question [Pauli-Weisskopf theory] to reality, since the particles without spin – like the α particle – are all complex [not fundamental]". To these complex particles it could not be applied, because their behavior was complicated by the unknown nuclear forces. And Pauli himself did not believe in the existence of elementary charged bosons – when the meson was discovered in 1937, he immediately concluded that it had to be a fermion (Letter from Pauli to Heisenberg, 14 June 1937) – a remainder of old notions of a correlation between charge and statistics. The theory remained for the time being a mere intellectual game, to prove that pair production could arise in a theory without a Dirac Sea. Weisskopf remembered:

> We thought that this theory only served the purpose of a nonrealistic example of a theory that contained all the advantages of the hole theory without the necessity of filling the vacuum. We had no idea that the world of particles would abound with spin-zero entities a quarter of a century later. That was the reason we published it in the venerable but not widely read *Helvetica Physica Acta*. [Weisskopf 1983, p. 70]

This did not prevent others from considering Pauli-Weisskopf theory as a possible starting point for a theory of the electron. Heisenberg thought that maybe the field quantization process had to be modified, so that, in quantizing the Klein-Gordon field, spin and Fermi statistics entered at the same time (Letter to Pauli, 16 June 1934), but did not pursue this thought much further.

A different modification of Pauli-Weisskopf theory was pursued by the Romanian-born French physicist Alexandre Proca, after attending Pauli's 1935 Paris talk[18]. The idea was simple: Could one not find a classical field theory that on the one hand fulfilled Pauli's demands (a positive definite energy density and a charge-current density with varying sign) but at the same time incorporated the electron spin? In order for his classical wave equation to include spin, he added another demand to Pauli's two: The wave field should have four complex components, just like the Dirac field. Since a spinor field was out of the question (this would lead to the Dirac equation, which did not have a positive energy density), he was left with only one possibility: A complex

[18] Proca was a researcher at the Institut Henri Poincaré [dos Santos Fitas and Videira 2007] at the time and later cited Pauli's lecture even before its publication. We thus have strong reason to believe that he attended the talk. And even if he did not, he was involved in the editorial decisions of the *Annales de l'Institut Poincaré*, where Pauli's talk was published, so he certainly knew of it before its publication. This last fact was kindly pointed out to me by Adrien Vila Valls, who is currently preparing an in-depth study of the history of the Proca equation.

vector field ψ_s[19]. He derived the relativistically invariant wave equations for such a field and presented them in a paper submitted in May 1936 [Proca 1936]. The central equations of this theory were (in the absence of an external electromagnetic field)[20]:

$$F_{rs} = \partial_r \psi_s - \partial_s \psi_r$$
$$\partial_r F_{rs} = \frac{m^2 c^2}{\hbar^2} \psi_s. \tag{7}$$

These equations are quite blatantly (even more blatantly if one considers the Lagrangian from which they can be derived) a generalization of Maxwell's equations to a "massive field" (in the sense that the quanta of the field will have a non-vanishing mass m). One would thus think that Proca should have immediately realized that, while his theory described massive particles with spin, this spin would have the wrong magnitude: It would be 1 instead of 1/2, since after all the quanta of the electromagnetic field, the photons, have an intrinsic angular momentum of 1. But, as we will see in the next section, the general theory of the one-to-one correspondence between representations of the Lorentz group and the physical quantity of spin was at this time only slowly developing. Proca's path to a correct interpretation of his wave equation was quite tortuous[21]; it was only one and a half years later that he published a paper identifying the particles described by his equation as possessing an integer spin [Proca 1938]. But even at this point, he did not simply state the analogy with the photon. He had to explicitly demonstrate the spin value of his field by taking the non-relativistic limit of the Proca equation and showing how many components remained (three) and how they coupled to an external magnetic field.

By this time, the fact that he had failed to produce a new relativistic theory of the electron was not that much of a tragedy (it may thus well be that he had discovered his mistake earlier, but only decided to publish now): With the discovery of the meson, his equation was now a candidate for the description of that particle, as was the Klein-Gordon equation. Thus the attempts to develop a new theory of the electron which avoided the difficulties of hole theory ended up giving candidate theories for the newly-discovered "heavy electron."

3 Generalizing the Dirac equation

A somewhat similar development was taken in the theory of generalized Dirac equations: When these were first studied by Bartel van der Waerden, he did not have in mind the description of new particles, but rather wanted to analyze how unique the Dirac equation was, and whether the presuppositions made by Dirac might also lead to a different relativistic wave equation. Van der Waerden's work was a response to the initial reactions upon the publication of the Dirac equation in 1928. The appearance of the four-component wave functions, which transformed non-trivially under the Lorentz group but were not four-vectors, caused quite some confusion. C.G. Darwin wrote

> Now the relativity theory is based on nothing but the idea of invariance, and develops from it the conception of tensors as a matter of necessity; and it is

[19] In fact, he mentioned the possibility of having a pseudo-vector field in a footnote, but did not pursue it any further.

[20] Basically all the statements on Proca's paper in Mehra and Rechenberg [2001] are false. They claim that the fundamental equation is simply the Klein-Gordon equation for a vector field and that this vector field can be derived from a scalar field by taking derivatives. They also imply that Proca quantized the field in his paper, which he did not.

[21] It will be described in detail in the forthcoming paper by Adrien Vila Valls, already mentioned in footnote 18.

> rather disconcerting to find that apparently something has slipped through the net, so that physical quantities exist which it would be, to say the least, very artificial and inconvenient to express as tensors. It does not seem possible to make anything further out of the matter until it has developed more... [Darwin 1928, p. 657]

and, in a similar vein, John von Neumann

> Dass eine Größe mit vier Komponenten kein Vierervektor ist, ist ein in der Relativitätstheorie nie vorgekommener Fall, im Diracschen ψ-Vektor tritt das erste Beispiel einer solchen auf. [von Neumann 1928, p. 876]

Von Neumann did not pursue the question much further, but did point out that this new representation of the Lorentz group was reducible into two two-dimensional representations. This observation was extended upon by Hermann Weyl: In his 1928 book "Gruppentheorie und Quantenmechanik" [Weyl 1928], he demonstrated that all representations of the Lorentz group could be constructed from two inequivalent (conjugate) two-dimensional representations, now known as Weyl spinors. In particular, Weyl showed how a four-vector X_μ could be constructed from a Weyl spinor $\chi = (\chi_1, \chi_2)$ as:

$$X_\mu = \begin{pmatrix} \chi_1^*\chi_1 + \chi_2^*\chi_2 \\ \chi_2^*\chi_1 + \chi_1^*\chi_2 \\ i\left(\chi_2^*\chi_1 - \chi_1^*\chi_2\right) \\ \chi_1^*\chi_1 - \chi_2^*\chi_2 \end{pmatrix} = \begin{pmatrix} \chi^\dagger\chi \\ \chi^\dagger\sigma_1\chi \\ \chi^\dagger\sigma_2\chi \\ \chi^\dagger\sigma_3\chi \end{pmatrix} = \chi^\dagger\sigma_\mu\chi \tag{8}$$

where the σ_i are the Pauli matrices, and in the Pauli matrix four-vector σ_μ the time component σ_0 is simply the identity matrix. The Dirac four-component wave function in turn could be decomposed into a Weyl spinor and a conjugate Weyl spinor, the two two-dimensional representations that von Neumann had identified.

It was Paul Ehrenfest who pushed for further clarification. He was at this time already quite desperate due to his inability to keep up with the developments in modern physics. One of his major worries were the unintuitive transformation properties of the Dirac wave function – for which he coined the name spinor – which seemed so at odds with the simplicity of the well-known vector and tensor calculus of relativity. At Ehrenfest's request, van der Waerden, a mathematics professor at the nearby Groningen university, provided a spinor calculus, to complement the well-known tensor calculus [van der Waerden 1929][22]. A Lorentz-covariant object now carried, according to its construction from Weyl spinors, spinor instead of tensor indices, which determined its transformation properties and the invariants that could be constructed from it. This allowed the treatment of tensorial objects (such as four-vectors) on the same footing as objects that could not be expressed as tensors (such as the Dirac wave function). The main difference to the tensor calculus was that one had to introduce two different types of indices, corresponding to the two inequivalent two-dimensional representations. Van der Waerden distinguished between the two by assigning dotted indices to the conjugate Weyl spinors.

Ehrenfest was still not satisfied and repeated his plea for a simple overview over spinor theory in his "Erkundigungsfragen" [Ehrenfest 1932], published in the year before his suicide. Van der Waerden's overview, unwieldy as it was at points, however, did form the basis for the generalization of the Dirac equation in the following decade.

[22] For a detailed discussion of van der Waerden's work, see [Schneider 2010].

It was van der Waerden himself who realized that the spinor calculus offered the possibility of constructing more general, relativistically invariant wave equations. However, as already mentioned, he only considered the possibility of alternate wave equations for the electron (the proton always being implied as well, the Dirac equation thus being understood as the universal equation describing the fundamental constituents of matter), i.e., he was dealing with the question whether one could reproduce Dirac's original reasoning in the spinor formalism. His generalization was thus quite limited: The only possibilities he allowed for the electron wave function were a Weyl or a Dirac spinor[23].

Van der Waerden found (whether independently of Weyl or not is unclear [Schneider 2010, p. 128]) that one could write down a wave equation for a two-component electron, but that this was only possible for a vanishing mass. In order to describe a massive electron, one was led to a (slightly generalized) Dirac equation, if one further assumed a first-order wave equation, in order to rule out a Klein-Gordon type equation for a Dirac spinor. This (slightly generalized) Dirac equation was of the form (in the absence of an external field)[24]:

$$\begin{aligned} \partial^{\dot{\alpha}\kappa}\chi_\kappa &= \frac{im'c}{\hbar}\psi^{\dot{\alpha}} \\ \partial_{\dot{\alpha}\kappa}\psi^{\dot{\alpha}} &= \frac{im''c}{\hbar}\chi_\kappa \end{aligned} \tag{9}$$

i.e., was written as two separate equations coupling the two Weyl spinors χ_κ and $\psi^{\dot{\alpha}}$ (κ and $\dot{\alpha}$ taking the values 1 or 2) that form the Dirac spinor. Here, $\partial_{\dot{\alpha}\kappa}$ is a 2×2 matrix differential operator, obtained by taking the relativistic scalar product of the four-divergence ∂_i with the four-vector of the Pauli matrices, while m' and m'' are arbitrary constants. As van der Waerden pointed out, the regular Dirac equation is retrieved if one sets $m' = m'' = m$.

The idea of more daring generalizations of the Dirac equation, to incorporate also particles with spin values different from 1/2, was apparently pursued for the first time by Majorana [1932]. It does not really belong into our story, as it did not use the spinor formalism and ended up using infinite-dimensional representations of the Lorentz group, which played no role for either the description of the meson or the development of the spin-statistics theorem. However, it serves as a perfect example for the change of perspective that took place after 1932. While Majorana made rather vague statements on the applicability of his generalized wave equations, and the paper reads rather as a study of hypothetical wave equations that could evade the problem of negative energies (and thus fits better into the last section), Dirac's 1936 paper on generalized relativistic wave equations [Dirac 1936] clearly states that these equations might be useful for describing new particles to be discovered in the future, a possibility that had become quite acceptable after the discovery of the positron and the neutron in the preceding years.

[23] His primary interest was to see whether one could simplify the Dirac equation by reducing it to two components [Schneider 2010, p. 125], and certainly not to complicate it by adding more. He apparently did not consider Proca's idea of a vector electron, which would not have been a simplification (also four components) and would have eliminated the need for the spinor calculus altogether, while the whole paper is of course based on the idea that a relativistic description of the electron needs a spinor description.

[24] I have written van der Waerden's equation using Dirac's later notation, to make the two more easily comparable. This is simply a relabeling.

At this time, Dirac was quite disillusioned by the great difficulties of quantum electrodynamics [Kragh 1990], and returned to a principle which he had first formulated in 1931 [Dirac 1931, p. 60]:

> There are at present fundamental problems in theoretical physics awaiting solution, e.g., the relativistic formulation of quantum mechanics and the nature of atomic nuclei (to be followed by more difficult ones such as the problem of life), the solution of which problems will presumably require a more drastic revision of our fundamental concepts than any that have gone before. Quite likely these changes will be so great that it will be beyond the power of human intelligence to get the necessary ideas by direct attempts to formulate the experimental data in mathematical terms. The theoretical worker in the future will therefore have to proceed in a more indirect way. The most powerful method of advance that can be suggested at present is to employ all the resources of pure mathematics in attempts to perfect and generalise the mathematical formalism that forms the existing basis of theoretical physics, and *after* each success in this direction, to try to interpret the new mathematical features in terms of physical entities...

This was precisely what he did: He took the existing basis of theoretical physics (relativistic invariance, the Dirac equation and its formulation using the spinor formalism), generalized it (to spin values greater than 1/2, both for massive and massless particles) and tried to interpret the new mathematical features in terms of physical entities ("the possible future discovery of an elementary particle with a spin greater than a half, or for approximate application to composite particles") in the above-mentioned paper, which he published in the summer of 1936. It was quite unsurprising that he should apply his generally stated research principle to this specific problem. After all, it was the original development of the Dirac equation (and hole theory) which was for him the paradigmatic example for the potential of this method. However, both the question of the generalization and of the subsequent interpretation need to be addressed in somewhat more detail, as they are not quite as straightforward as they may seem.

What did Dirac mean by a generalization to spins greater than one half? In fact, the question of the physical quantity of spin does not enter in this work at all. He did not deal with the question of how to extract the actual physical spin value from the relativistic wave equation, as had Majorana or, as we saw in the last section, Proca. Instead he simply assumed that spinors with more than one spinor index would correspond to spin values greater than that of the Dirac electron (which consisted of two Weyl spinors with only one spinor index each). What precisely was the correspondence between physical spin and spinor representation did not concern him. This thus directly also touches the question of interpretation: After he had written down the equations in a general form, while (in typical Dirac fashion) giving hardly any indication as to how he had arrived at them, Dirac discussed their physical implications no further – he had simply prepared them in a very general form (indexed according to the number of spinor indices of the wave field) in case they might come in handy at some future date.

The lack of further elaboration on where these equations come from is less disconcerting in this case than it is in others, because it can clearly be seen that the equations Dirac ended up with are a direct generalization of van der Waerden's formulation of the Dirac equation given above. Dirac simply allowed for both component fields A (a generalization of van der Waerden's χ) and B (generalized ψ) to now have an arbitrary number of spinor indices, both dotted and undotted[25], where A has one

[25] He adopted van der Waerden's convention, where undotted indices are left-handed Weyl spinor indices, and dotted indices are right-handed Weyl spinor indices.

dotted index less and one undotted index more than B. The generalized wave equation then reads[26]:

$$\partial^{\dot{\alpha}\kappa} A^{\dot{\beta}\dot{\gamma}\cdots}_{\kappa\lambda\mu\cdots} = \frac{im'c}{\hbar} B^{\dot{\alpha}\dot{\beta}\dot{\gamma}\cdots}_{\lambda\mu\cdots}$$

$$\partial_{\dot{\alpha}\kappa} B^{\dot{\alpha}\dot{\beta}\dot{\gamma}\cdots}_{\lambda\mu\cdots} = \frac{im''c}{\hbar} A^{\dot{\beta}\dot{\gamma}\cdots}_{\kappa\lambda\mu\cdots} \tag{10}$$

where both A and B are taken to be symmetrical in both their dotted and their undotted indices. With these equations, Dirac's paper basically ends. He did not go on to quantize these equations as field equations, rather regarding them still as one-particle equations of the Schrödinger type. This then remained the state of the art in equations for particles with general spin until the new particles Dirac had been hoping for were actually discovered – again just as fast as the positron had appeared after its original prediction by Dirac.

4 Nuclear physics

While the last two sections dealt with research concerned with the mathematical description of (possibly new) particles, I will now, before turning to the discovery of the meson, discuss research that was more concerned with the postulation of new particles in order to explain phenomena in nuclear physics. Initially, the description of these new particles hardly used any of the novel elements discussed in the last two sections, but was rather based on the tried and trusted equations used to describe the well-known particles. But as the nuclear theories involving new particles came to be studied in detail, the old equations often tended to be unable to explain the fine points and so these nuclear theories were combined with the new equations in order to fully exploit their potential.

A case in point is the neutrino: Originally postulated by Pauli in 1930 in order to explain the continuous β ray energy spectrum and then included by Fermi in his 1934 quantum field theory of β decay, it was initially assumed by default to have spin 1/2 and to be describable by the Dirac equation, just like the electron. When the basic Fermi theory, however, began to show some difficulties in reproducing the exact shape of β spectra[27], modifications were pursued. The most popular and successful one, was the Uhlenbeck-Konopinski theory, which changed nothing in the description of the neutrino itself, but rather modified the interaction term. Another possibility for modifying the Fermi theory was to instead change the equations for the free neutrino, so that it would no longer be described by the Dirac equation; there was after all the possibility that the neutrino could have a (half-integer) spin greater than 1/2 and still couple invariantly to a neutron, a proton and an electron. This possibility was pursued by a PhD student of Rudolf Peierls in Cambridge, Fred Hoyle (Letter from Rudolf Peierls to Hans Bethe, 13 February 1937 [Lee 2007])[28]. In doing this, Hoyle discovered a difficulty in constructing such a theory: A spin 3/2 field, the next

[26] In a similar, but somewhat more involved, way, Dirac's generalized equation for massless fields is a generalization both of the Weyl equation and Maxwell's equations, which had first been written in spinorial form by Laporte and Uhlenbeck [1931].

[27] These deviations were later found to be due to energy loss within the radioactive sources and could be removed by using thinner sources [Franklin 2005].

[28] This work appears to have been done initially in total isolation from Dirac, even though Peierls and Hoyle were both also in Cambridge at the time. But as Peierls wrote to Bethe, Dirac was quite distracted by his marriage.

simplest possibility, had neither a positive-definite energy density (and could thus not be quantized bosonically according to the Pauli-Weisskopf procedure), nor a positive-definite (charge) density and could thus also not be quantized fermionically using hole theory. Hoyle extended his research to study particles with arbitrary spin and found this to be a generic problem (Letter from Pauli to Peierls, 19 March 1938) for particles with spin greater than 1.

We thus observe how a study of the new particles postulated in nuclear physics led to research questions closely related to those discussed in the last two sections. Hoyle's research (or rather dissatisfaction with it) was an important starting point for Pauli's research on higher spins, as we will see later on. But much more prominent in bringing such questions to the forefront of attention was another nuclear particle, for the simple reason that it was (believed to be) discovered within a few years after being postulated.

I refer of course to the Yukawa particle, envisioned by Hideki Yukawa in 1935 as the quantum of a new U field, which mediates the interaction between protons and neutrons, in analogy to the electromagnetic field, the only difference being that the interaction would in this case lead to a proton being transformed into a neutron and vice versa, in accordance with Heisenberg's conceptualization of the nuclear force as resulting from a "Platzwechsel" [Yukawa 1935]. The quanta of this U field, as Yukawa pointed out, necessarily had to be bosons, in order to be responsible for such an exchange force[29] between two fermions (the proton and the neutron)[30]. As for the spin of these quanta, Yukawa did not comment on that at all: He envisioned the force field to be vectorial, in full analogy to the electromagnetic field, but in his paper only considered the scalar potential (i.e., the time component) of the force field, as he was treating the nucleons non-relativistically and thus neglected their interaction through the vector potential of the U field. However, he did make quite specific statements on both the charge and the mass of the U quanta. The charge had to be $\pm e$, in order to account for the change in charge of the nucleons involved in the interaction. And the mass could be determined from the range of nuclear interactions to be about 200 times the mass of the electron.

Even more than Fermi's theory, Yukawa's theory still had a number of shortcomings in describing the nuclear interactions; among other things, it could not yet describe the the spin dependence of these forces. These difficulties were however not addressed until Yukawa's U quanta came to be associated with the newly confirmed intermediate mass (between electron and proton) particles, responsible for the penetrating component of the cosmic radiation, the mesons or mesotrons.

How this association came to be has been studied in detail in [Rechenberg and Brown 1990] and need not further concern us here. What is important for our purposes is that when this connection had been made, making Yukawa's general approach highly relevant and plausible, work immediately began on improving and extending Yukawa's rudimentary non-relativistic field theory to a fully relativistic theory of mesons, which would describe all aspects of the nuclear interaction. Here now, the resources whose development was described in the last two sections became essential,

[29] See [Carson 1996] for an insightful analysis of the problematic term "exchange force," and how the concepts of Heisenberg and Yukawa relate to modern ideas on forces arising from the exchange of virtual quanta.

[30] Yukawa had actually originally disregarded this point in earlier attempts to describe the nuclear interaction through the exchange of electrons. Apparently it was pointed out to Yukawa by Nishina that the exchange particle should be a Bose particle [Hayakawa 1983, p. 85]. More important was, however,Yukawa's move to a stronger analogy with the electrodynamic force, where the mediating particle, the photon, was of course known to be a boson Darrigol [1988].

and as we will see in the next section, our three research strands combined very quickly with ongoing research on cosmic rays to form a whole new field of research: meson physics.

5 Meson physics

That the work on meson physics, beginning in 1937, involves the intersection of different programmatic approaches was already stated some time ago by Cassidy [1981]. He differentiates between two distinct traditions, the cosmic ray physicists, intent on applying an admittedly flawed QED in order to describe cosmic ray phenomena, in particular energy loss, and the field theorists, intent on exploring the theoretical limits of QED in order to find the final theory of the interaction of matter and radiation.

In the last three sections, I have shown that the "field theorists" identified by Cassidy by no means had a shared program: While Pauli, Weisskopf and Proca were looking for a new QFT in which hole theory could be avoided, Dirac was basically doing one-particle quantum mechanics without any direct aim at reforming QED.

Also, Cassidy is somewhat dismissive of the role played by Yukawa and thus of the role of nuclear theory as a separate tradition in its own right, rather than as a mere application of quantum electrodynamical methods. Partly his dismissal is based on a misunderstanding of Yukawa's original work (which he wrongly identifies as being based on scalar Pauli-Weisskopf theory, when in fact it is the non-relativistic limit of a vector field theory), partly on the neglect of Yukawa's later work: In fact, Yukawa was a trailblazer for meson physics, for the simple reason that he believed in a relation between his U quanta and the penetrating cosmic radiation before most others did, already in the fall of 1936.

Since the cosmic ray data gave very little input as to the construction of such a theory (it really did not offer much more than compatibility with the mass and charge values derived by Yukawa already in 1935), the range of possibilities was wide open, and Yukawa consequently followed a many-pronged approach. In manuscripts mentioned by [Hayakawa 1983] and [Rechenberg and Brown 1990], he further pursued the development of his vectorial theory, also incorporating the work of Proca. At the same time, he was investigating the possibility of a scalar theory along the lines of Pauli and Weisskopf – such a theory of spinless bosons could of course also describe the interaction between two spin 1/2 fermions. And finally, he also studied the general wave equations that Dirac had presented, even publishing a paper on this subject, in which he investigated in more detail the physical observables, such as velocity and spin [Sakata and Yukawa 1937a]. By the summer of 1937, however, Yukawa was focussing specifically on the scalar theory [Sakata and Yukawa 1937b]. A simple reason for this narrowing of focus is probably the fact that the scalar field theory had already been formulated as a full quantum field theory by Pauli and Weisskopf. Yukawa's focus on the scalar theory was shared by those theorists in the west who first adopted the identification of the cosmic ray meson and Yukawa's particle [Fröhlich and Heitler 1938; Oppenheimer and Serber 1937; Stueckelberg 1937].

However, as already pointed out in 1937 by Oppenheimer and Serber [Oppenheimer and Serber 1937], a scalar Pauli-Weisskopf theory of the meson as the source of nuclear interactions seemed incapable of explaining all the empirically ascertained characteristic properties of that force. In particular, these difficulties arose due to the lacking spin dependence of the nuclear force mediated by a scalar meson. Heitler and Fröhlich in Bristol attempted to solve this by making the coupling between the nucleons and the mesons depend on the kinetic angular momentum of the (scalar) meson Fröhlich and Heitler [1938]. The decisive step, however, which really kickstarted meson physics, was to quantize Proca's massive vector field theory and thus obtain a quantum theory of mesons with nonzero spin. This quantization was presented in four (more or less)[31] independent papers that appeared in early 1938, by Nicholas Kemmer [1938], who had briefly been Pauli's assistant in the summer of 1936 and was now working at Imperial College, London, by the Cambridge physicist Homi J. Bhabha [1938][32], by the Swiss physicist E.C.G. Stueckelberg [1938] and by Yukawa and his group [Yukawa et al. 1938][33,34].

It is Kemmer's work which is of interest to our story. While Bhabha, Stueckelberg and Yukawa confined themselves to constructing a quantum theory of vector (Proca) mesons, Kemmer took a more a general approach; he wanted to map out the entire space of possible meson quantum theories spanned by the two main restrictions: The mesons had to be bosons and have an integer spin[35]. According to his reminiscences, Kemmer had in fact constructed a quantum theory of vector particles, based on the work of Pauli, Weisskopf and Proca, even before hearing of Yukawa's theory and before the discovery of the meson, but had not published it [Kemmer 1983].

As a first step towards obtaining a general quantum theory of integer spin bosons, Kemmer combined the results of Pauli, Weisskopf and Proca with Dirac's formalism. In Dirac's scheme particles with different spin are classified according to two half-integer or integer (nonzero) numbers, k and l, which determine the number of dotted and undotted indices of the generalized Weyl spinors A and B, where A has $2k$ undotted and $(2l-1)$ dotted indices and B has $(2k-1)$ undotted and $(2l)$ dotted indices (cf. equation 10). The transformation properties of a field were thus fully defined by the numbers k and l, which can be integers or half-integers. Naturally, the four-component Dirac spinor is obtained when k and l both take the smallest possible value of $1/2$. Kemmer now showed [Kemmer 1938], how Dirac's scheme included Proca's massive

[31] Certainly Bhabha and Kemmer, both in England, were in rather close contact with each other and with Heitler [Kemmer 1983]. Also, in a footnote of his paper, Stueckelberg acknowledged having communicated with Kemmer.

[32] Not to be confused with the contemporary Post-Colonialist Homi K. Bhabha.

[33] It may seem surprising that neither Proca nor Dirac were directly involved in this development. While Proca simply was too slow (his paper on the vectorial nature of his field was submitted around the same time as these four, but did not yet include any work on the quantization of that field), Dirac had recently been burned when emphatically embracing experimental results by the American experimentalist Robert Shankland, which seemed to indicate energy non-conservation in the nucleus. These results were quickly disproven, leaving Dirac wary of novel experimental results [Farmelo 2009]. Along with his general distrust of matter-wave quantization and his growing distrust of quantum field theory in general, this appears to have kept him from applying his wave equations to the meson, even though he had expressly prepared them for just such an occasion.

[34] Maybe Robert Serber in Berkeley needs to be included in this list: He claims to have been the first one to have used Proca mesons [Serber and Crease 1998, pp. 45–46], but his only publication on the matter is a short abstract, which does not indicate whether he actually developed a quantum theory of such a field, which he called "dynaton" [Serber 1938].

[35] Again, we encounter here something that looks like an application of a spin-statistics theorem, but is not. It is simply the fact that if one wants to describe the interaction between the two nucleons by a field, that field has to be bosonic and of integer spin for the simple reason that the nucleons are fermions and have half-integer spin. If the spin-statistics connection is thus established for a few fundamental building blocks, it spreads to all the particles and fields these elementary building blocks couple to (nuclei, atoms) and with (mesons). Pauli in fact was initially quite opposed to existence of elementary charged bosons (Letter to Heisenberg, 14 June 1937) and expected the mesons to be fermions. But their association with Yukawa's U quanta put the bosonic nature of the mesons beyond doubt for the time being.

vector field (now identified as a spin 1 field) as the next-simplest case[36], namely $k = 1$ and $l = 1/2$. $A_{\kappa\lambda}$ is then identified with Proca's field strength tensor F_{rs}, while $B_{\lambda}^{\dot{\alpha}}$ is identified with Proca's vector field ψ_r. He further showed that there was a second way of transcribing the spinor theory with $k = 1$ and $l = 1/2$ into vector notation: Since the spinor notation said nothing about the transformation properties of the field under parity transformations, $B_{\lambda}^{\dot{\alpha}}$ could also be identified as a pseudo-vector. He further showed that Dirac's framework could be used to describe a Klein-Gordon field – one only had to loosen the condition that A and B were totally symmetric in their undotted and dotted indices, and allow for fully antisymmetrized spinor indices as well. Since the spinor indices can only take the values 1 and 2, antisymmetry is only an additional option for fields with exactly two indices[37]. Allowing for antisymmetric indices thus gives exactly one additional option, with the same index numbers as the Proca theory, $k = 1$ and $l = 1/2$. $A_{\kappa\lambda}$ with antisymmetrized indices then has only one degree of freedom, which is identified with the scalar (or pseudo-scalar) Klein-Gordon field, while $B_{\lambda}^{\dot{\alpha}}$ is identified with the divergence of that scalar field. B can be eliminated from the field equations, and one obtains the Klein-Gordon equation. Kemmer had thus identified four additional cases in Dirac's formalism: Vector (Proca), Pseudo-Vector (new), Scalar (Klein-Gordon-Pauli-Weisskopf) and Pseudo-Scalar (new).

He then claimed that this was in fact all: These were all the sensible field theories one could formulate for the meson. Fields with a spin greater than 1 would lead to serious difficulties, since their energy densities were not positive definite, which was of course, according to Pauli's criteria, a deathblow for any bosonic theory. Kemmer therefore stuck with these four, analyzed the corresponding quantum theories in analogy to Pauli and Weisskopf, and finally concluded that of these four only the vector (Proca) theory (which had also been studied by Bhabha, Stueckelberg and Yukawa) would be able to correctly reproduce the correct sign and spin-dependence of the proton-neutron interaction. It was thus this theory of the meson which he further pursued, together with Heitler and Fröhlich, and which became the initial theoretical basis for meson theory.

The further development of meson physics is again described by Cassidy and also in Kemmer's reminiscences. It is no longer of immediate relevance to our story. Instead, it is again time to assess the status of the spin-statistics connection in the year 1938, as it presented itself to Wolfgang Pauli, who now once again becomes the central figure of this paper. He was aware of the results of both Kemmer and Hoyle (the latter being unpublished), and he was in direct contact with the former and also exchanged letters with the latter's supervisor, Rudolf Peierls. Similar results had been obtained by Pauli's own diploma student, Joseph Maria Jauch [Jauch 1938]. These results seemed to indicate that there was only a very limited number of possible relativistic quantum field theories, describing particles of spin 0, 1/2 or 1. For the scalar case, he himself had shown that only bosonic quantization was possible, while for the spinor case the necessity of fermionic quantization was a result of Dirac's hole theory[38]. Finally, no formal proof had been brought forth concerning the necessity of bosonic quantization for the Proca (i.e., the massive vector) equation, but the existence of

[36] There is always the possibility of switching k and l, which gives the complex conjugate representation, which is however equivalent in all cases considered.

[37] Fields with less than two indices have no symmetry properties, while fields with more than two indices must always have at least two identical indices and can thus not be antisymmetrized.

[38] As pointed out by Duck and Sudarshan, the latter argument was turned into a more formal argument, akin to the original reasoning of Pauli and Weisskopf, by Iwanenko and Socolow in 1937, using a charge-symmetric theory of electrons and positrons. Their paper appears to have been ignored by Pauli, who continued to prefer the seemingly more physical argument based on the Dirac Sea.

that necessity was certainly generally believed in, especially due to the close relation of the Proca theory with both the Maxwell and Pauli-Weisskopf theories. The latter analogy had, in fact, been pointed out to Proca immediately after he first published his equation in 1936, in a letter from Rosenfeld (August 1936):

> [I]t seems to me that the quantization of your complex vector will cause, as far as the exclusion principle is concerned, the same difficulties as the scalar of Pauli-Weisskopf; Fermi statistics will not be obtained for the same reasons as those given in the work of these authors. (Archives de l'Académie des Sciences de Paris, fond Alexandre Proca, correspondance scientifique)[39]

To conclude, in 1938 the space of possible particle descriptions, both concerning the allowed underlying classical field theories and the respective allowed quantization procedures, seemed to be entirely mapped out.

Pauli, however, believed that the arguments by Hoyle and Kemmer which excluded spins greater than 1 were not conclusive, and that it should be possible after all to construct quantum field theories from Dirac's (classical) wave equations for an arbitrary spin. Together with his new assistant Markus Fierz, he set himself to constructing general quantum field theories for arbitrary spin, thus in turn opening up the possibility of a general spin-statistics theorem, which did not simply categorize a limited number of possible field theories, but provided a general classification of an infinity of relativistic QFTs according to their spin and the statistics they necessarily obeyed.

6 Quantum field theories for an arbitrary spin

How is it that Pauli started to distrust the arguments of Hoyle and Kemmer? After all, they were based on the conditions he himself had formulated in 1935, namely that a sensible quantum field theory would need either a positive energy density (bosonic quantization) or a positive charge density (fermionic quantization). But, in discussing Hoyle's work with Peierls, he began to wonder whether these conditions might not be too restrictive, if they really ruled out quantum field theories with a spin greater than 1. As he would later write to Heisenberg (28 May 1938):

> It also seems logically satisfying to me, that in a theory in which the rest masses of the particles are arbitrary, the spin should be arbitrary as well.

Pauli argued (Letter to Peierls, 19 March 1938) that one could well have a (bosonic) theory with an energy density that was not positive definite, as long as the total energy, i.e., the integral of the energy density over space, was positive definite[40]. And he claimed that this was the case for integer spin quantum fields with spin greater than 1. Hoyle's results seemed to indicate the opposite, and a debate between Pauli and Peierls/Hoyle ensued. Hoyle had been working with zero-mass fields, most probably since his original interest had been in describing higher spin neutrinos, and Peierls originally hoped that this fact might explain the discrepancies (Peierls to Pauli, 24 March 1938), but it turned out that Hoyle had neglected the effect of the auxiliary

[39] Many thanks to Adrien Vila Valls for making this letter available to me and helping with the translation.

[40] It was not the first time that Pauli had brought forth such an argument: In his review article on relativity theory [Pauli 1921], which had made his fame as a young man, Pauli had already (with greater rigor than Einstein before him) made an analogous argument concerning the energy of the gravitational field. It is uncertain whether he already realized or suspected at this point that the difficulties in general relativity and in field theories with arbitrary spin were related: This only became evident when Fierz and he identified the massless spin 2 field with the gravitational field somewhat later.

(gauge) conditions (Peierls to Pauli, 14 May 1938). Similarly, the total charge of half-integer spin fields with spin greater than 1/2 also turned out to be positive definite. There was thus nothing in the classical field theory that prevented spins greater than 1. Peierls and Hoyle briefly claimed that new difficulties would arise after quantization (Peierls to Pauli, 14 May 1938), but this claim could also be rebuffed: Hoyle had been calculating with canonical (equal-time) commutation relations of the sort introduced by Heisenberg and Pauli; these were however not relativistically invariant. When Fierz performed the same analysis with relativistically invariant commutators (analogous to those first employed by Jordan and Pauli for the free electromagnetic field in 1927), the problems encountered by Hoyle disappeared (Pauli to Peierls, 25 May 1938). Peierls and Hoyle finally conceded, and Hoyle had to give up his Ph.D. thesis, as his results had turned out to be deeply flawed. He ended up never obtaining his PhD.[41] Fierz, on the other hand, published his results on the quantization of field theories with arbitrary spin using manifestly Lorentz-invariant commutators as his *Habilitationsschrift* [Fierz 1939], handed in on 28 July 1938 [Enz et al. 1997, p. 83]. This work included a first formulation of the spin-statistics theorem for arbitrary spins.

As was to be expected, the proof of the necessity of fermionic quantization for half-integer spin was again a one-line proof: By explicitly constructing energy-momentum tensors from the spinor fields, Fierz could show that theories with half-integer spins greater than 1/2 shared the property of the Dirac equation of having a non-definite total energy. The exclusion principle was thus necessary in order to implement hole theory. Concerning the converse proof for bosonic fields, on the other hand, Fierz presented a new derivation, which was however closely related to the original proof in the 1934 Pauli-Weisskopf paper, referring directly to an inconsistency in the formulation of the commutation relations and thus lacking a clear physical interpretation.

Why did not Fierz attempt a generalization of Pauli's 1935 method, which had been the most physical proof of a connection between classical field theory and bosonic quantization so far? The simplest explanation seems to be the following: Pauli's proof as it stood appeared to rely essentially on varying the expression for the charge-current density, and this variation, which, as has been pointed out, was problematic from the start, was very difficult to generalize to the more complicated expression for the charge-current density of a field with arbitrary integer spin. Fierz thus instead opted to retreat to a purely formal proof, which made no use of observable quantities, not even of the energy.

A first step was to take a new look at Dirac's 1936 equations, which were really equations for fields with an arbitrary number of spinor indices, and investigate how they related to the actual physical quantity of spin. The connection between the vectorial field and spin 1, about which Proca had been confused a few years earlier, was now common knowledge, thanks to meson theory. Fierz, however, was the first to systematically establish a general relation between spinor index configurations and the physical spin. Physical spin here means mechanical spin only, since the magnetic spin is only defined for fields coupled to an electromagnetic potential; Fierz's paper, however, dealt only with free fields, for reasons which I will discuss later on. The mechanical spin could be determined by going to the rest frame of a classical wave and determining the number of linearly independent plane wave solutions (i.e., polarizations); a spin of f then corresponds to $2f + 1$ possible polarizations[42]. Fierz could thus identify many redundancies in Dirac's equations, where different index configurations corresponded

[41] The question whether his later claims that this was mainly for tax reasons are accurate is beyond the scope of this paper.

[42] The existence of a rest frame of course necessitates a nonzero mass for the quanta associated with the field. Fierz dealt with the special case of vanishing rest mass, which had so troubled poor Hoyle, in an appendix.

to the same physical field: While Dirac had categorized his fields according to two integer or half-integer numbers k and l, Fierz could show that their physical properties only depended on the sum $k+l$ and that the spin f was equal to $k+l-\frac{1}{2}$. He could then, from demands of relativistic invariance (generalizing the work of Jordan and Pauli), construct a general (i.e., for arbitrary spin) commutation bracket between the (Fourier coefficient of the) field $a(k)$ and its complex conjugate, which in the rest frame took the simple form:

$$[a(k), a^*(k')] = C\delta_{kk'}\omega_k^{2f-1} \tag{11}$$

where C is a positive constant, ω_k is the frequency of the wave (which in the rest frame is only periodic in time and not in space), and the bracket on the left-hand side can (a priori) signify either a commutator or an anti-commutator. The wave equations allow for two possible frequencies for the plane wave solutions, namely $\pm mc^2/\hbar$, as was well known from the Klein Gordon and Dirac equations. This meant that the right-hand side was positive definite only for half-integer spin. On the other hand, if the bracket on the left-hand side was taken to be an anti-commutator it was certainly positive definite for $k = k'$[43]. Consequently, Fierz deduced that fermionic quantization with anti-commutators was not possible for integer spin theories and concluded:

> The above considerations appear to furnish a proof of the long-conjectured connection between spin and statistics.

Our story is now almost done: Fierz had proven the spin-statistics connection for arbitrary spins; all possible quantum field theories with which microscopic particles could be described had been mapped out. There were of course still major desiderata: In particular the simple but major fact that Fierz's results applied only to free fields. Setting up the general field equations for the interaction with an electromagnetic field was the next major focus of research in Zurich – and the difficulties encountered thereby again put into doubt for a while the existence of spins greater than 1. These developments, which culminated in a joint paper by Pauli and Fierz [Fierz and Pauli 1939], nowadays mainly famous for identifying the spin 2 field with the gravitational field of general relativity, are already beyond the scope of this paper.

I will concern myself with one last development: Pauli's proof of the spin-statistics theorem, also limited to the case of non-interacting fields. Pauli's proof went beyond that of Fierz in the same way as Pauli's considerations of his 1935 Paris talk went beyond those stated in the paper with Weisskopf: Instead of presupposing the precise form of the commutators, the energy densities, etc., Pauli deduced his proof from very general properties of the different spin field strengths and also returned to the physical reasoning of his 1935 talk, namely to the commutability of space-like separated observables.

7 Pauli's proof

Even Pauli's celebrated proof was not originally intended as the subject of a standalone paper on the spin-statistics theorem. It was originally only written as part of a joint presentation of Pauli and Heisenberg for the 1939 Solvay Conference. The topic of the conference was to be the theory of elementary particles, and Heisenberg had been asked to give a report on "general questions, limits of the current theory, the notion of elementary particle" (Letter from Heisenberg to Pauli, 20 April 1939). In preparing this report, Heisenberg asked Pauli whether he might not be willing to take

[43] The same statement about lacking rigor by modern standards that was made for the analogous Pauli-Weisskopf proof applies here – the singular behavior for equal arguments of the operators necessitated a more involved treatment in later years.

on the first part of the report, which Heisenberg envisioned as dealing with the "general properties of elementary particles", in particular, mass, spin, statistics and wave equations (with and without interaction), a subject area which Heisenberg rightly felt Pauli to be an expert on (Heisenberg to Pauli, 23 April 1939). Pauli grudgingly accepted:

> As much as it goes against my laziness to write such a report myself, I do believe that there are no objective reasons which would allow me to decline your suggestion [...] I do, however, want to make the counter-suggestion that I do *not* deal with interactions [...] and call the whole section [...] "Relativistic Wave Equations of *force-free* Particles and their Quantization." I would then also treat the connection between spin and statistics [...]

So Pauli set to work. We must now briefly address the question whether the proof of the spin-statistics theorem he thus provided was predated and possibly influenced by other demonstrations of the spin-statistics connection which appeared around the same time. The first was by Fredrik Jozef Belinfante [1939], at the time PhD student of Hendrik Kramers in Leiden. Since Belinfante's proof of the spin-statistics theorem, based on the novel conception of charge conjugation invariance, which his PhD supervisor had recently proposed [Kramers 1937], was published before Pauli's, it has been assumed [Schweber 1994] or at least implied [Duck and Sudarshan 1997] that Pauli's work was influenced by Belinfante. However, a closer look at the chronology shows that this is highly improbable[44].

On June 10th, Pauli sent a preliminary table of contents for his part of the report to Heisenberg and remarked on the paragraph containing the "connection between spin and statistics" that the proofs contained therein (he was already finished with that part) "ought to be new in this form." Three days later, he wrote to Kemmer that he had finished the Solvay report. The report was then sent out at some point before the official deadline (July 1st) and a copy of the manuscript, which was forwarded to Niels Bohr, either by Pauli himself or by the conference committee, is extant and reprinted in [von Meyenn 1993]. It differs from the final published version of Pauli's proof [Pauli 1940] only in minor details. Belinfante did not submit his paper until a few weeks later, on July 15. It was published in the October Issue of Physica. There is absolutely no indication that Pauli was aware of Belinfante's work prior to its publication.

It thus seems legitimate to end this paper by only discussing Pauli's proof, while leaving Belinfante's proof to a future work on the history of the spin-statistics connection after Pauli, where the connection with charge-conjugation (and CPT) invariance takes center stage. However, Belinfante's work is relevant to our story in an indirect way: After its publication, Pauli wrote to Kramers that Belinfante's proof was incorrect (sketching his own, as yet unpublished, proof as the correct one) and asked him to have Belinfante write a retraction. Instead, Belinfante (according to his recollections [Enz 2002, p. 334]) wrote a paper in which he clarified the relation between the two proofs, which was then published under the name of both Belinfante and Pauli [Pauli and Belinfante 1940]. It was only in this paper (or rather in the letter to Kramers that had led to it) that the two axioms on which Pauli's proof was based were explicitly stated, in order to distinguish Pauli's proof from Belinfante's, which was based on the axiom of charge conjugation invariance – I will return to Pauli's axioms in due time.

The other demonstration of the spin-statistics theorem which appeared around this time, again in the context of a Ph.D. thesis, was by Jacobus Stephanus de Wet

[44] This is not to say that Pauli's decision to publish his proof in a separate paper was not influenced by the appearance of Belinfante's proof. That, I would say, is highly probable, but the evidence is entirely circumstantial.

[1940], who got his PhD in Mathematics at Princeton in 1940, though he was in close contact with the local physicists, in particular Eugene Wigner. Again, we can basically exclude an influence of de Wet on Pauli; de Wet's results weren't published until 1940, and there is no indication that Pauli was aware of his work until he himself moved to Princeton in the same year.

So, again it seems legitimate to delegate a full discussion of de Wet's proof to a future paper. Also de Wet's work, however, is relevant to our discussion in one aspect: Pauli addressed de Wet's proof in a footnote added to his own proof when it was – the Solvay Conference 1939 having been cancelled because of the outbreak of the war – translated to English and published in the Physical Review after Pauli's move to Princeton in the summer of 1940. Pauli criticized de Wet for his use of canonical equal-time commutation relations. These, Pauli argued, did not apply to spins greater than 1, as he had learned in his discussions with Peierls and Hoyle. From a contemporary viewpoint, de Wet's proof may seem superior to the one of Pauli [Duck and Sudarshan 1997]: Physicists have learned to accept the special importance of field theories with spin equal to or less than 1, due to considerations of renormalizability, and feel comfortable with a proof of the spin-statistics theorem which confines itself to the small spin values and then generalizes by constructing the higher spins from the smaller ones. This criticism, however, misses the point of what Pauli's proof is all about in its historical context: It is the capstone in the development of a general quantum field theory for arbitrary spins, with which any conceivable new particle might be described and categorized.

And indeed it is this universality which Pauli himself saw as the main merit of his work: The principles Pauli's proof was based on offered no new insights with respect to the deliberations of the preceding decade; the two cases still had to be proven by two separate lines of argument, and in a letter to Heisenberg from 7 August 1939, Pauli consequently spoke of "proofs" rather than "proof." The major advance, as Pauli stressed in the same letter, was the "generality" of the proof. Since Fierz had already explicitly constructed a "relativistic theory for particles with spin > 1, fulfilling all physical requirements" (Letter from Pauli to Weisskopf, 10 March 1940), Pauli could, in order to get as simple, elegant and general a proof as possible, return to Dirac's original conception, where the field quantities were only defined by the number of dotted and undotted spinor indices, and did not have to concern himself with questions of redundancy and the actual physical spin, except in some introductory remarks.

With this classification alone, he was able to determine general properties of tensors constructed from the fields, in particular of second-rank tensors. He was thus able to prove the essential properties of the energy-momentum tensor without having to construct it explicitly, as Fierz had done. To this end, he first identified a transformation that would leave any homogeneous and linear field equation (i.e., describing a field interacting neither with an external source nor with itself) invariant. Although Pauli did not do so explicitly, this transformation can be identified as a simultaneous spatial and temporal reflection at the origin, a TP transformation in modern parlance[45]. Since this transformation left the field equations invariant, it transformed solutions of the field equations (i.e., physically allowed field configurations) into other solutions. Pauli then showed that any second-rank tensor constructed from fields with half-integer spin would change sign under such a transformation. This implied that for any physically allowed classical field configuration with a positive total energy there was another physically allowed field configuration with negative total energy. The necessity of fermionic quantization then followed in the usual way, through appeal to hole theory and the first of the axioms that Pauli had identified in his paper with Belinfante, namely the demand for positive total energy.

[45] See [Duck and Sudarshan 1997], who refer to this transformation as strong-reflection.

The second axiom, which was needed to prove the integer spin part of the theorem, was identified by Pauli as the commutability of space-like separated observables. This was a generalization of the axiom postulated in his 1935 Paris talk, where he had singled out the commutability of the charge density at points with space-like separation. While his motives for this step most certainly included the wish for generality, there is also an indication that he might have been uncomfortable focussing on the charge density: This quantity was really only observable when there was an external electromagnetic field, and, as already indicated, there were still substantial issues to be solved in such an interacting theory (see in particular a letter from Pauli to Kemmer from 24 November 1939). This axiom allowed Pauli to derive the general structure of the field commutation brackets, again avoiding the kind of explicit construction that Fierz had performed in his paper. The general structure he obtained for the commutation bracket of an integer spin field U with his complex conjugate was

$$[U(x), U^*(x')] = \text{even number of derivatives of } D(x - x') \tag{12}$$

where the D function is the generalization of Jordan and Pauli's original Δ function, which had been only applicable to massless fields such as the electromagnetic field, and, as with Fierz, it is a priori undefined whether the bracket denotes a commutator or an anti-commutator. However, using arguments akin to those used originally by him and Weisskopf and later by Fierz, Pauli could show that the even number of derivatives of the D function (for half-integer spin, he found an odd number of derivatives) was only compatible with interpreting the bracket as a commutator, and thus with Bose statistics.

8 Conclusions

Pauli's paper famously concludes with the words

> In conclusion, we wish to state, that according to our opinion the connection between spin and statistics is one of the most important applications of the special relativity theory.

When Gregor Wentzel presented Pauli's result at the end of his highly influential textbook on quantum field theory three years later [Wentzel 1943], he almost copied Pauli's appraisal, but with a telling shift of emphasis:

> It is doubtless one of the most beautiful successes of the quantum theory of fields that it – in connection with the postulates of the theory of relativity – delivers a general theoretical explanation for the connection between spin and statistics.

Pauli's proof was seen as a great achievement for quantum field theory – a theory which in the first decade or so of its existence had been almost universally distrusted and dismissed (see for example [Rueger 1992]). Building on Fierz's explicit construction of quantum field theories for arbitrary spin, it established quantum field theory as a universal framework, able to describe any particle imaginable that might pop up in cosmic rays, and more than that, it was even able to function as a guide by limiting and organizing what could be considered imaginable in the first place. Most succinct, even in its understatement, is perhaps Pauli's own assessment of the relevance of the spin-statistics theorem and its impact on the status of quantum field theory, which he expressed in a letter to Heisenberg on 28 May 1938 (that is, after Fierz's proof):

> It is rather interesting that by itself the integer spin has to be quantized according to Einstein-Bose, the half-integer according to Fermi-Dirac [...] *for that has not been put in by hand.* (Emphasis by the author)

By setting aside for the time being the difficulty of field interactions, these results also appeared untarnished by the divergence difficulties of quantum field theory, allowing Pauli to also use manifestly covariant methods, usually set aside in favor of the more tractable rough-and-tumble canonical methods when calculating actual physical processes. This return to manifest covariance, furthered then by Wentzel's textbook, was an important element in the reinvention of quantum electrodynamics by Tomonaga and Schwinger a few years later.

Certainly Pauli's proof was lacking in one fundamental aspect, not just by modern standards: It basically consisted of two separate proofs for half-integer and integer spin particles. This established a dichotomy between fermions and bosons, which has persisted despite more modern proofs in the context of axiomatic QFT which derive the entire spin-statistics theorem in a unified manner [Massimi 2005]. The enduring status of Pauli's proof and the importance that has been assigned to it ever since, can only be appreciated if one sees it as what it was in its historic context: A triumph for quantum field theory and, after a decade of grave difficulties, that theory's (re-)establishment as a candidate for a theory of everything.

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