THE LEGACY OF UNCERTAINTY

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Abstract

An historical account is given of the circumstances whereby the uncertainty relations were introduced into physics by Heisenberg. The criticisms of QED on measurement-theoretical grounds by Landau and Peierls are then discussed, as well as the response to them by Bohr and Rosenfeld. Finally, some examples are given of how the new freedom to advance radical proposals, in part the result of the revolution brought about by "uncertainty," was implemented in dealing with the new phenomena encountered in elementary particle physics in the 1930s.

1 Introduction

I must thank the organizers of this conference on Squeezed States and Uncertainty Relations for the kind invitation to speak here. For some years I have studied and written on the history of modern physics, and so I assumed that I was to speak on some topic in that field. Let me say why a talk on the history of physics may be relevant, and why I have chosen the title as I have. According to a Greek historian of the period just before the Christian era, Dionysius of Halicarnassus, "History is philosophy from examples." But why should physicists care anything about philosophy, by examples or otherwise? Because physics was and is natural philosophy, and never more so than when we deal with uncertainty relations.

I will begin by discussing the general significance of the Heisenberg uncertainty relations, how they entered physics, and what interpretational (i.e., philosophical) problems they were intended to solve. I will then mention the criticisms that Lev Landau and Rudolph Peierls addressed to the measurement problem in QED, criticisms which led Niels Bohr and Lon Rosenfeld to attempt to justify the real existence of quantized electromagnetic fields. But I will not be so foolhardy as to review this subject in technical detail, when I am in the presence of so many experts on quantum optics. Instead, I shall ask how the establishment of a quantum mechanics that accepts the impossibility of exactly describing an atomic system in classical terms, influenced the thinking of physicists as they tried to understand the phenomena of subatomic, i.e., nuclear and subnuclear, systems. For, after the introduction of "uncertainty," physicists felt permitted to advance hypotheses that would have been unthinkable before the quantum mechanical revolution of 1925-26.

In particular, I shall discuss some bold developments during the 1930s in quantum field theory and in nuclear and cosmic ray physics, three subjects whose confluence gave rise to the new field that is now called elementary particle physics. [1]

2 The Origin of the Uncertainty Relations and of Complementarity

In a recent biography of Heisenberg by David Cassidy, entitled Uncertainty, the author begins a chapter, which is called "Certain of Uncertainty," as follows:

> On March 22, 1927 Werner Heisenberg submitted a paper to the Zeitschrift fuer Physik entitled "On the perceptual [anschaulich] content of quantum theoretical kinematics and mechanics." The 27-page paper, forwarded from Copenhagen, contained Heisenberg's most famous and far-ranging achievement in physics–his formulation of the uncertainty, or indeterminacy, principle in quantum mechanics. Together with Bohr's complementarity principle, enunciated later that year, and Born's statistical interpretation of Schrdinger's wave function, Heisenberg's uncertainty principle formed a fundamental component of the so-called Copenhagen interpretation of quantum mechanics–an explication of the uses and limitations of the mathematical apparatus of quantum mechanics that fundamentally altered our understanding of nature and our relation to it ... [This] marked the end of a profound transformation in physics that has not been equalled since. [2]

The development of quantum mechanics by Heisenberg, Born, Jordan, Bohr, Schroedinger, Dirac, and others in 1925-26 marked the end of a period, beginning with Planck's introduction of the quantum of action in 1900, that was characterized by efforts, sometimes described as "desperate," to apply the well-established Newtonian particle and Maxwellian wave concepts, even if modified by Einstein's relativity and restricted by the quantum rules of Bohr-Sommerfeld. But quantum mechanics entailed a whole new epistemology. Common-sense notions of causality, separability, locality, visualizabilty, and measurability demanded, at the least, reinterpretation, and perhaps utter abandonment at the quantum level. Heisenberg's uncertainty principle lay at the very heart of all this consternation and excitement. How did it first appear?

After Born, Jordan, and Heisenberg set out the principles and methods of matrix mechanics, Schroedinger introduced wave mechanics, and soon proved that the two very different approaches would always lead to the same predictions. (The equivalence of wave and quantum mechanics was independently shown by Wolfgang Pauli.) This immediately raised the old spectre of the wave-particle paradox in a new context, as did the experiments of 1927 on electron diffraction. (However, histories of quantum mechanics emphasize the theory, and they do not seem to take much notice of the latter.) After 1927 it became necessary to take seriously the matter waves of Louis de Broglie, and to explain how the de Broglie-Schrdinger wavelike electron could be the same object that leaves a well-defined track in a Wilson cloud chamber. Bohr and Heisenberg, then Bohr's assistant in Copenhagen, had been very concerned about this paradox the previous year, and to help clear up the matter, Bohr invited Schroedinger to visit them. Accordingly, Schroedinger took the train to Copenhagen from his post in Zurich, in October 1926. The Austrian physicist still adhered to a "realist" view of electron waves, and rejected any notion of "quantum-jumping," that is, the transfer of energy in discrete amounts, rather than continuously.

By all accounts [4], poor Schroedinger was attacked so vigorously by the usually congenial Bohr that he became ill and took to bed. Bohr, however, pursued him even into the sickroom,

and would not allow him to rest. Nevertheless, Schroedinger left Copenhagen without giving up the reality of his waves and still refused to concede the existence of quantum jumps.[4] According to Heisenberg, the result of the visit was a continued preoccupation by Bohr and himself with the problem of interpreting the quantum theory. As Heisenberg described it:

> For all that, we in Copenhagen felt convinced toward the end of Schroedinger's visit that we were on the right track, though we fully realized how difficult it would be to convince even leading physicists that they must abandon all attempts to construct perceptual models of atomic processes. During the next few months the physical interpretation of quantum mechanics was the central theme of all conversations between Bohr and myself ... Since our talks often continued till long after midnight ... both of us became utterly exhausted and rather tense. Hence Bohr decided in February 1927 to go skiing in Norway, and I was quite glad to be left behind in Copenhagen, where I could think about these hopelessly complicated problems by myself. [5]

Recalling a conversation with Einstein, who had maintained that it was only the theory which decides what we can observe, Heisenberg began to question what we really see when we examine an electron track in a cloud chamber:

> In fact, all we do see in the cloud chamber are individual water droplets which must certainly be much larger than the electron. The right question should therefore be: Can quantum mechanics represent the fact that an electron finds itself approximately in a given place and that it moves approximately with a given velocity, and can we make these approximations so close that they do not cause experimental difficulties? A brief calculation ... showed that one could indeed represent such situations mathematically, and that the approximations are governed by what would later be called the uncertainty principle of quantum mechanics.[6]

Upon Bohr's return to Copenhagen, there was "a fresh round of difficult discussions," in which Bohr insisted that the correct solution was to be given by the principle of complementarity. "But he soon realized," said Heisenberg, " ... that there was no serious difference," and that the main problem remaining was how to convince other physicists of the new way of looking at the world. That would not be easy. Comparing the scientist's voyage of discovery with that of Columbus, Heisenberg said:

> In science, too, it is impossible to open up new territory unless one is prepared to leave the safe anchorage of established doctrine and run the risk of a hazardous leap forward ... When it comes to entering new territory, the very structure of scientific thought may have to be changed, and that is far more than most men are prepared to do. [7]

For a brief period, Bohr and Heisenberg had had a falling-out, since Heisenberg wished to base his uncertainty relations entirely upon the particle viewpoint of matrix mechanics, while to Bohr the indeterminacy was related to the necessity of including in the discussion the complementary wave aspect of matter and of radiation. However, the two had reconciled their views

by October 1927, when Bohr gave a major address at the Fifth Solvay Congress in Brussels, essentially repeating a speech that he had made a month earlier at Como, Italy at a conference on the centenary of Alessandro Volta. At the Solvay conference, there began the famous and long-lasting Bohr-Einstein debates on the interpretation of quantum mechanics, the forerunner of the Einstein-Podolsky-Rosen arguments and Bohr's reply. [8]

3   The Measurability of Quantum Fields

In the spring of 1929, Heisenberg gave a set of lectures on quantum theory at the University of Chicago, a major portion of the lectures being concerned with a critique of the wave and particle concepts in interpreting experiments on Wilson photographs, x-ray and electron diffraction, etc. He also analyzed the spreading of wave packets, and he obtained uncertainty relations for electromagnetic fields, e.g., those holding for the simultaneous measurement of a component of the electric and a compnent of the magnetic field, both being measured in the same volume element. His conclusion was that: "After a critique of the wave concept has been added to that of the particle concept all contradictions between the two disappear, provided only that due regard is paid to the limits of applicability of the two pictures." [9]

In his Chicago lectures, Heisenberg gave three "proofs" of the relation

$$\Delta E_x \Delta H_y \ \geq \ \frac{hc}{(\delta l)^4}, \tag{1}$$

for the fields averaged over a cubic cell of side δl. However, as shown later by Bohr and Rosenfeld, due to the presence of a δ- function involving the time difference in the commutator of two field components, the inequality (1) is ambiguous. When the averaging is more appropriately done over a space-time region, rather than space only, the right-hand side of (1) becomes zero. [10] Bohr and Rosenfeld concluded: "From this it follows that the averages of all field components over the same space-time region commute, and thus should be exactly measurable, independently of each other." [11]

The work of Bohr and Rosenfeld was in large part a response to a criticism of QED, based on measurement theory, that had been made by two very young (and rather brash) theorists, namely Lev Landau and Rudolph Peierls. In 1929 Landau was visiting physics centers in Western Europe on a grant from the Soviet Union, spending some time with each of Ehrenfest, Pauli, Heisenberg, Rutherford, Kapitza, and Born. However, for the most part he stayed in Copenhagen with Bohr, who (we know from his correspondence) was at that time concerned and, rather uncertain, about the uncertainty relations for two electromagnetic field components. [12] Visiting Zurich at the beginning of 1930, Landau began working on problems of QED with Peierls, who was then Pauli's assistant. In December of 1930, Landau again visited Zurich, and he and Peierls wrote a paper arguing that QED was essentially meaningless, because a fundamental limitation made the measurement of electromagnetic fields impossible in the context of quantum theory. Obviously, this paper was intended to (and did) generate a major controversy. [13]

According to Rosenfeld:

There was indeed reason for excitement, for the point raised by Landau and Peierls was a very fundamental one. They questioned the logical consistency of quantum electrodynamics by contending that the very concept of electromagnetic field is not susceptible, in quantum theory, to any physical determination by means of measurements. The measurement of a field component requires determinations of the momentum of a charged test-body; and the reaction from the field radiated by the test-body in the course of these operations would (except in trivial cases) lead to a limitation of the accuracy of the field measurement, entirely at variance with the premises of the theory ... On the other hand, ... the occurrence of irregular fluctuations in the value of any field component ... was known to be responsible for one of the divergent contributions to the self-energy of charged particles. Landau and Peierls, somewhat illogically, tried to bring it into relation with their alleged limitation of measurability of the field, and this only further confused an already tangled issue. [14]

As noted above, after two years of soul-searching, and by the use (in thought experiments) of classical test bodies, the consistency of QED as regards measurements was proven, for, again according to Rosenfeld [15]:

So long as we treat all sources of electromagnetic fields as classical distributions of charge and current, and only quantize the field quantities themselves, no universal scale of space-time dimensions is fixed by the formalism. It is then consistent to disregard the atomistic structure of the test-bodies and there is no restriction to the logically admissable values of the charge density. [16]

Surely this is one of the few examples of a problem of physics reduced to one of mere logic. As in much of Bohr's work on measurement theory, a great deal of effort went into assuring readers that they need not worry further about the puzzling issues that gave rise to the paper. Abraham Pais quotes approvingly a friend's remark on Bohr-Rosenfeld: "It is a very good paper that one does not have to read. You just have to know it exists." [17]

4 The Legacy of Uncertainty: The Positron and the Neutrino Conjectured

After the probability interpretation and the (quite separate) measurement problems of non-relativistic quantum mechanics had been "solved," or at least put in abeyance for a time, most thoughtful physicists felt that the first order of business was to look at other fundamental issues of the theory, especially those related to the striking new phenomena then being revealed by experiment. At least one important era of research had been successfully concluded; Dirac in 1929 expressed it as follows:

The general theory of quantum mechanics is now almost complete, the imperfections that still remain being in connection with the fitting of the theory with relativity ideas ... The underlying physical laws necessary for the mathematical theory of a large part of physics and the whole of chemistry are thus completely known. [18]

Dirac admitted that there remained great practical difficulty in actually solving the complicated equations for atomic and molecular systems, but he failed to mention that there were also "fundamental" questions remaining even in nonrelativistic quantum mechanics; for example, the treatment of collective behavior like superconductivity. As the quotation above shows, the fundamental problem that concerned Dirac at this time was the relativistic theory of the electron itself, and this also appeared to be implicated in at least three other problematic areas, namely: quantum field theory, nuclear physics, and the cosmic rays. [19]

Problems associated with the theory of the electron had been present almost since the turn of the century. The existence of a finite-sized concentration of electric charge appeared to require a new stabilizing force to prevent its explosion. As a constraint on the structure, physicists (notably, H.A. Lorentz) advanced the hypothesis that all the mass of the electron was electromagnetic in origin. In classical models, this required the (spherical) electron to have a radius

$$r_0 = a\frac{e^2}{mc^2},$$ (2)

e and m, being the mass and charge of the electron, c the velocity of light, and a a dimensionless constant of order unity, whose value depended on the assumed structure of the electron. (We shall assume in what follows that $a = 1$). Letting the radius tend to zero gave the electron an infinite self-energy, i.e., an infinite mass. There was difficulty in reconciling a finite electron with the theory of relativity, and Lorentz had suggested that within the electron radius r_0, physical laws that were different from the usual ones might apply. [20]

The problem became acute with the advent in 1925 of quantum mechanics, in which the electron was treated as a point particle. The most obvious relativistic generalization of the Schroedinger equation, the Klein-Gordon equation ("the equation of many fathers"), did not give the correct fine structure of the hydrogen spectrum, which Arnold Sommerfeld had somehow managed to obtain (without electron spin!) by using the Bohr-Sommerfeld "old" quantum theory. The problem in quantum mechanics was that the electron spin was not properly taken into account. Dirac set out to find an equation that would give the right spin and magnetic moment to the electron (he referred to these as "duplexity phenomena") by remedying the "incompleteness of the previous theories lying in their disagreement with relativity, or alternatively, with the general transformation theory of quantum mechanics." [21]

Dirac's new electron theory was spectacularly successful in treating the fine structure of hydrogen, Compton scattering, the electron's magnetic moment, and other important physics–but it also gave rise to new puzzles. The chief difficulty was the presence of negative energy states, which were meaningless in a relativistic theory, since an electron in such a state would have a negative mass. Dirac tried to prevent electron transitions to these negative energy states by declaring that they were all filled, and hence, by the exclusion principle, unavailable in practice. If occasionally "holes" did occur, they would act in every way as positive electrons.

Later, the one-electron theory of Dirac, with filled vacuum states, was supplanted by a quantum field theory, which was then combined with the quantum field theory of the electromagnetic field that Dirac (and also Pascual Jordan) had pioneered in 1927. The theory of the two fields in interaction became known as quantum electrodynamics (QED). [22] However, this completely

relativistic theory was itself plagued by serious inconsistencies, of which the most egregious were the so-called "divergences," namely, infinite predictions for the physical mass and charge of the electron. These divergences arose when virtual (i.e., energy-nonconserving intermediate) states were summed over, according to the rules of perturbation theory. The lowest approximations did give finite results, and were in surprisingly good accord with experiment. It was, therefore, assumed that the theory was correct at lower energies, but that it broke down above some critical interaction energy. In a suitably modified QED, it was argued, the small value of the expansion parameter (the dimensionless fine-structure constant, $\alpha = 1/137$) would validate the perturbation expansion.

Quantum mechanics can be expressed either in configuration space or in its complementary energy-momentum space, the two spaces being related by the Fourier transform theorem. Thus, a critical *high energy* can be related to a critical *small distance*. QED was working well at the energy scale that corresponds to the Compton wavelength, but it was assumed that it would very likely break down at the classical electron radius r_0, which is 137 times smaller than the Compton wavelength. That might account, it was thought, for the apparent contradictions to accepted laws of physics that were puzzling physicists around 1930, especially in the higher energy nuclear and cosmic ray phenomena, since $r_0 = 10^{-13} cm$ is almost identical with the known range of nuclear forces. [23] This distance was also a "natural" fundamental length at which to expect a breakdown in the classical theory, as Lorentz had, in fact, predicted at the beginning of the century. One of the principles guiding the development of quantum theory had been that classical physics is a limiting case of quantum physics (Bohr's Correspondence Principle); it was not forgotten in the 1930s.

Bohr suggested just such a breakdown of known laws in his Faraday lecture to the British Association in London in 1930, and repeated the idea at a conference in Rome in October 1931. [24] To Dirac he wrote: "I ... believe firmly that the solution to our present troubles will not be reached without a revision of our general ideas still deeper than that contemplated in the present quantum mechanics." [25] Heisenberg, who adopted the same belief as Bohr, tried to make a theory involving a minimum length, introducing a space that was a lattice-world, rather than a continuum, a concept to which he returned several times later on in his life. As the appropriate lattice spacing he proposed the distance h/2cMc, where M is the mass of the proton. Thus this distance is about 2000 times smaller than the electron's Compton wave length. He motivated his choice by the argument that distances smaller than the uncertainty inherent in a measurement with the most massive known elementary particle, the proton (i.e., the uncertainty in position determination by an ideal hypothetical proton microscope) were meaningless. This, then, was one legacy of the uncertainty relations. [26]

Let us now leave aside the problems of QED and consider the conventional picture of the structure of the nucleus around 1930. In 1930 it was believed that there were only two elementary particles, the proton and the electron (described in an Encyclopedia Britannica article by Robert Millikan as negative and positive electrons). These particles interacted according to the laws of Maxwell and of quantum mechanics to produce ordinary matter. Thus all matter, atoms *and* their nuclei were supposed to be electrical in nature. (The only additional fundamental interaction was gravity–curved space-time perhaps–although if all mass were truly electromagnetic, then perhaps gravity itself was intimately entangled with electromagnetism. (The notion of a unified field, was

considered by Einstein, Hermann Weil, Theodor Kaluza and Oskar Klein, and others.)

One of the most immediate difficulties with the electron-proton nuclear model was β-decay. Without the neutrino, not yet postulated by Pauli, any theory of β-decay inevitably violated energy and momentum conservation. These days we may find it surprising that the generation of quantum revolutionists did not insist upon the preservation of the basic conservation laws. (Indeed, Bohr rather preferred the idea that energy was not conserved in individual elementary processes, but only statistically. He argued that in that case, the first and second laws of thermodynamics would have a comparable statistical foundation. [27])

Some other difficulties of the electron-proton model were [28] :

- The symmetry character of the nuclear wave function depends upon the parity of the atomic mass number A, not Z, as the model predicted. [The number of fermions in the nucleus in the model is 2A-Z]; when A-Z is odd the spin and statistics of the nucleus were given incorrectly. For example, nitrogen (Z = 7, A = 14) was known, from the molecular band spectrum of N_2, to have spin 1 and Bose-Einstein statistics. In the e-p model, it was composed of 21 fermions–so it should have had half-integer spin and should have obeyed Fermi-Dirac statistics.]

- No potential well is deep enough and narrow enough to confine a particle as light as an electron to a region the size of the nucleus. [The argument for this is based on the uncertainty principle and on the relativistic electron theory.]

- It is hard to see how to "suppress" the very large (on the nuclear scale) unpaired magnetic moments of the electrons in the nucleus, which would conflict with the data on the hyperfine structure of atomic spectra.

The great *attraction* of the electron-proton model was that it was a unified theory. Indeed, no more unified theory has existed between that of Thales of Miletus (who is said to have believed that everything is made of water) and modern string theory. The only problem was that the electron-proton model could not coexist with quantum mechanics. But could it be that quantum mechanics was the correspondence limit of some more general dynamical theory that might relinquish even more of measurability than quantum mechanics did? For example, the observables in the new theory might be represented by operators that were non-associative, as well as non-commutative.

Such was the thinking as the thirties began: A new physics *was* in the offing, a new revolution in physics as one penetrated below some minimum distance. In part that thinking was correct–a new physics was in the offing. But it was not to be a physics of new laws, but one of new particles! The particles were new, but they obeyed the known laws of relativity, quantum mechanics, and quantum field theory.

The first of the new particles, the neutrino, was proposed by Pauli in a famous letter, dated 4 December 1930 and addressed to a meeting on radioactivity in Tuebingen (via Hans Geiger and Lise Meitner). The letter began: "Dear radioactive ladies and gentlemen." The new proposal had probably more a conservative than a radical character. One of the suggested neutral fermions was supposed to sit with each electron in the nucleus, thus solving the spin-statistics difficulty. In β-decay, it would accompany the emitted electron, thus permitting the conservation of energy and momentum. Pauli called the particle a "neutron," and indeed it was meant to accomplish

a part of what was later done by the neutron *and* the neutrino together. (Of course, it still did nothing to help the "confinement" and the hyperfine structure difficulties of the electrons in the nucleus. It should also be noted that Pauli's neutron was a purely conjectural particle, designed to be almost undetectable. The actual neutrino was detected only on the 1950s.)

Pauli was very uncertain about his neutron-neutrino idea, and while he told people about it privately, he did not want the idea to be published. One of the first times it was mentioned in print was in a report given by S.A. Goudsmit at an international conference in Rome in October 1931. [29] However, at the same meeting, Bohr discussed "Atomic stability and conservation laws," saying about β-decay:

> If energy were conserved in these processes, it would imply that the individual atoms of a given radioactive product were essentially different, and it would be difficult to understand their common rate of decay. If, on the other hand, there is no energy balance, it is possible to explain the law of decay by assuming that all nuclei of the same element are essentially identical. This conclusion would also be in accord with the general evidence on the nuclear statistics of non-radioactive elements, which has revealed the essential identity of any two nuclei containing equal numbers of protons and electrons. [30]

A proposal rather close to our present idea of the neutrino was first presented by Pauli at the Seventh Solvay Conference in October 1933, a year and a half after the neutron had been discovered. A few months later, Fermi made his β-decay theory, conserving all important physical quantities and fitting the β- decay lifetimes very well. Nevertheless, in October 1934 at an international conference in London-Cambridge, the preferred theory presented was not Fermi's, but a non-conserving theory proposed by Guido Beck and Kurt Sitte and openly advocated by Bohr. [31]

5   The Legacy of Uncertainty: The Neutron and the Fermi-Field

The *annus mirabilis* of elementary particle physics was the year 1932. Here is how the discoveries went: January, deuterium (Urey et al.); February, the neutron (Chadwick); April, the first accelerator induced nuclear reactions (Cockroft and Walton); August, the positron (Anderson); September, the cyclotron (Lawrence). In the same year, 1932, Heisenberg wrote a three-part paper which introduced a neutron-proton model of the nucleus. [32]

Heisenberg's model is widely praised in nuclear physics textbooks, and some of the physicists who were active in nuclear theory during the 1930s (e.g., Bethe) have said that it allowed them to use quantum mechanics, because it effectively took electrons out of the nucleus. In Heisenberg's model, nuclei are built of protons and neutrons interacting through charge-exchange forces. In the Hamiltonian describing the nucleus, only neutron and proton space and spin coordinates appear, and the isospin operators are introduced to change the nucleon type. Thus, if one ignores the frequent mention and use of nuclear electrons in the Heisenberg paper, treating it as pure

phenomenology of nuclear systematics, it is possible to argue that Heisenberg's model makes quantum methods available to nuclear physics (although the usefulness of such a partial approach had already been demonstrated by Gamow in his α-particle model of the nucleus).

However, there are still electrons, and they play an important role, in Heisenberg's "neutron-proton model" of the nucleus. For example, the neutron is an electron-proton compound; the charge that is exchanged to provide the attractive binding force is an electron; in β-decay radioactivity, the electron is emitted without a neutrino (and it is thus an energy, momentum, and angular momentum non-conserving theory); in addition to the electrons bound in neutrons and particles, there are other "free" nuclear electrons to account for the frequent occurrence of interactions involving high energy radiation, e.g., Bremsstrahlung.

It is difficult for us to see how so radical a departure from physical norms could have been tolerated. It is, in fact, so difficult that most textbook authors are embarrassed to reveal that Heisenberg's fundamental theory violated almost all conservation laws (charge is an exception to this rule), or that half of the Heisenberg work consisted of wrestling with this devil! In the Hamiltonian, one sees that the neutron is treated as an electron-proton composite of spin 1/2, obeying Fermi statistics, while the proton is an elementary fermion. The p-p interaction is pure Coulomb; the n-n interaction is a double exchange, as in the hydrogen molecule, or more generally, as in covalent bonding; the n-p interaction is one-electron exchange, as in the ion H_2^+. It was only after the success of the Fermi β- decay theory that Heisenberg accepted the idea of the neutrino and the "elementary" neutron, and he was one of the first to do so! [33]

Fermi's theory of β-decay contributed much to the solution of the difficulties of nuclear structure theory, aside from being a good account of this special form of radioactivity. Embracing Pauli's neutrino (so christened by Fermi after Chadwick's discovery of the proton's neutral partner), the theory treated the emission of an electron-neutrino pair, coupled in a "four-vector" state, much like the emission of a photon from an excited atom. The photon was not "in the atom" to begin with, but it was created in the transition. Thus electrons and neutrinos need not be inside nuclei. Advances in radiation theory using QED also showed that the large observed radiative interactions were made by virtual electron-positron pairs in the nuclear Coulomb field– these were the "low-mass" radiating charges of the nucleus. The radiative processes consisted of, besides Bremsstrahlung, pair production and pair annihilation. [34]

The upshot was that it became *unnecessary* to postulate the existence of electrons in any nucleus, even those that β-decay. Heisenberg enthusiastically accepted the idea of the Fermi-field, not only for β-decay, but also as the nuclear analog of the electromagnetic field. Thus, much as atoms were held together by the exchange of electromagnetic quanta, the photons, nuclear forces were to be carried by the quanta of the nuclear field, i.e., electron-neutrino pairs. The small value of the Fermi coupling constant, fitted at low energies to the observed rates of β-decay, would be compensated in the case of nuclear binding, where higher virtual energies were dominant, by large matrix elements of the interaction. Indeed, these matrix elements were more than large–they were infinite! Thus, if the integrations in calculating the matrix elements were cut off at a suitably chosen high energy (again implying a characteristic length), it was possible to fit the required strength of nuclear binding forces. [35]

Unfortunately for the many physicists who had been attracted by the high degree of unification presented by the Fermi-field theory of nuclear forces, it was not possible to fit both the strength

and the *range* of nuclear forces simultaneously by the choice of cutoff. That such a procedure would fail by many orders of magnitude became clear to Heisenberg when he worked out the details; independently, this result was found and published by two Russians, Igor Tamm and (once again) Dmitri Iwanenko. [36]

Meanwhile, in far-off Japan, a young physicist of the next scientific generation, Hideki Yukawa, advanced boldly to the next step. Challenging the new orthodoxy of quantum mechanics and quantum fields, just as the previous generation had done in postulating and developing those new dynamical systems, Yukawa decided that a *new* field should have a *new* quantum, not the electron, not the electron-neutrino pair, but a quantum all of its own. He called this the "heavy quantum," or the "U-quantum," of the nuclear force field, which he called the U-field.

This scientific revolution that has been called, by Yoichiro Nambu, the paradigm of modern elementary particle theory, namely, the identification of forces and their representation by quantum fields, having their characteristic quanta, came about this way, as Yukawa relates it:

> The crucial point came to me one night in October [1934]. The nuclear force is effective at extremely small distances, on the order of 2×10^{-15} cm. That much I knew already. My new insight was the realization that this distance and the mass of the new particle that I was seeking are inversely related to each other. Why had I not noticed that before? The next morning, I tackled the problem of the mass of the new particle and found it to be about two hundred times that of the electron. It also had to have the charge of plus or minus that of the electron. Such a particle had not, of course, been found, so I asked myself, "Why not?" The answer was simple: an energy of 100 million electron volts would be needed to create such a particle, and there was no accelerator, at that time, with that much energy available. [37]

After presenting this paper at a physics meeting, and after submitting the article with his theory to a journal in November 1934, Yukawa felt that his struggle with the problem of nuclear forces had been, for the time being at least, resolved. He concluded his account of his scientific life up to that time as follows:

> I felt like a traveler who rests himself at a small tea shop at the top of a mountain slope. At that time I was not thinking about whether there were any more mountains ahead. [38]

I too feel that it is time now to rest, without proceeding further with this description of the legacy of uncertainty.

References

[1] See, e.g., L.M. Brown and L. Hoddeson, eds., *The Birth of Elementary Particle Physics* (Cambridge University Press, 1983).

[2] David Cassidy, *Uncertainty* (New York: W.H. Freeman, 1992), p.226.

[3] J. Mehra and H. Rechenberg, *The Historical Development of Quantum Mechanics, Vol. 5* (New York: Springer-Verlag, 1987), esp. pp. 822 ff.

[4] The 1926 visit of Schroedinger is described vividly by Heisenberg in his book *Physics and Beyond,* translated from the German by A.J. Pomerans (New York: Harper and Row, 1971), pp. 73- 76.

[5] Note 3, pp. 76-77.

[6] Note 3, p. 78.

[7] Note 3, p. 70.

[8] A. Einstein, B. Podolsky, and N. Rosen, "Can quantum-mechanical description of physical reality be considered complete?", Phys. Rev. **47** (1935), pp. 777-780. N. Bohr, [same title], Phys. Rev. **48** (1935), 696-702. For an account of the Bohr-Heisenberg interaction in 1937, see A. Pais, Niels Bohr's Times (Oxford: Clarendon Press, 1991), pp. 295-316.

[9] W. Heisenberg, *The Physical Principles of the Quantum Theory*, trans. by C. Eckart and F.C. Hoyt (University of Chicago Press, 1930), p. 47.

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[11] In Wheeler and Zurek, note 10, p. 485.

[12] For Bohr's feelings at this time, see A. Pais, Note 8, especially Chapter 16.

[13] The Landau-Peierls criticism is discussed in L.M. Brown and H. Rechenberg, "Landau's work on quantum field theory and high-energy physics," in *Frontiers of Physics*, E.A. Gotsman, Y. Ne'eman, and A. Voronel, editors (Oxford: Pergamon Press, 1990), pp. 53-81. See also: O. Darrigol, "Coherence et completude de la mechanique quantique: l'example de 'Bohr-Rosenfeld'," Rev. Hist. Sci. **XLIV/2** (1991), pp. 137-179; A. Miller, "Measurement problems in quantum field theory in the 1930's," in *Sixty-two Years of Uncertainty*, A. Miller, editor, (New York: Plenum, 1990).

[14] L. Rosenfeld, "On quantum electrodynamics," in *Niels Bohr and the Development of Physics*, W. Pauli, editor (Oxford: Pergamon Press, 1955), pp. 70-95, especially p. 70.

[15] In Note 14, Rosenfeld says the paper was submitted to the Danish Academy on 2 December 1932, and "The reading of the fourteen or so successive proofs only took about one more year."

[16] Note 14, p. 72. And on p. 79, Rosenfeld says: "At the end of our laborious inquiry, we had thus completely vindicated the consistency of quantum electrodynamics, at least in its simplest form."

[17] Pais, note 8, p. 362.

[18] P.A.M. Dirac, "Quantum mechanics of many-electron systems," Proc. Roy. Soc. (London) **A114** (1929), pp. 243-265.

[19] Note 1, especially the editors' introduction.

[20] A. Pais, "The early history of the theory of the electron: 1897-1947," in *Aspects of Quantum Theory*, A. Salam and E.P. Wigner, editors (Cambridge University Press, 1972), pp. 79-93.

[21] P.A. M. Dirac, "The quantum theory of the electron," Proc. Roy. Soc. (London) **A117** (1928), pp. 610-624.

[22] W. Heisenberg and W. Pauli, "Zur Quantendynamik der Wellenfelder," Zeit. f. Phys. **56** (1929), pp. 1-61 and ibid. **59** (1930), pp. 168-190.

[23] L.M. Brown and H. Rechenberg, "Nuclear structure and beta decay (1932-1933)," Am. J. Phys. **56** (1988), pp. 982-988.

[24] N. Bohr, "Faraday Lecture", J. Chem. Society London (1932), pp. 349-384; "Atomic Stability and conservation laws," *Convegno di Fisica Nucleare* (Rome, R. Accademia d'Italia, Atti, 1932), pp. 119- 130.

[25] Bohr to Dirac, 29 August 1930; quoted in note 2, p. 287.

[26] Joan Bromberg, "The impact of the neutron:Bohr and Heisenberg," Historical Studies in the Physical Sciences **3** (1971), pp. 307-341.

[27] See note 24. In these lectures, Bohr proposed the nonconservation of energy in β-decay.

[28] Quoted from L.M. Brown, "The idea of the neutrino," Physics Today, September 1978, pp. 23-28.

[29] S.A. Goudsmit in *Convegno di Fisica Nucleare*, Note 24, p. 41. This remark is discussed in note 28.

[30] Note 29, p.129

[31] International Conference on Physics, London 1934, Vol.1, Nuclear Physics (Cambridge University Press, 1935).

[32] W. Heisenberg, "ber den Bau der Atomkerne," Zeit. f. Phys. **77** (1932), pp. 1-11; ibid. **78** (1932), pp. 156-164; ibid. **80** (1933), pp. 587-596.

[33] The first suggestion that the neutron was elementary was put forward by D. Iwanenko, "Sur la constitution des noyeaux atomique," Comptes rendus (Paris) **195** (1932), pp. 439-441.

[34] L.M. Brown and D.F. Moyer, "Lady or Tiger?–the Meitner- Hupfeld effect and Heisenberg's neutron theory," Amer. J. Phys. **52** (1984), pp. 130-136.

[35] L.M. Brown and H. Rechenberg, "The Fermi-field theory of nuclear forces," Max Planck Institute for Physics and Astrophysics, preprint, Dec. 1987, MPI-PAE/PTh 92/87.

[36] Ig. Tamm, "Exchange forces between neutrons and protons and Fermi's theory," Nature **133** (1934). p. 981; D. Iwanenko, "Interaction of neutrons and protons," ibid., pp. 981-982.

[37] H. Yukawa, *Tabibito* (The Traveler), trans. by L.M. Brown and R. Yoshida (Singapore: World Scientific, 1982), p. 202.

[38] Note 37, p. 203.