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NEW ENTITIES, OLD PARADIGMS: ELEMENTARY PARTICLES IN THE 1930S[1]

JAUME NAVARRO
Cambridge University

RESUMEN

En este artículo pretendo analizar los procesos por los cuales se descubrieron y aceptaron nuevas partículas elementales en los años anteriores a la segunda guerra mundial. Muchos libros de divulgación de física de partículas elementales suelen narrar una historia lineal sobre la búsqueda de las últimas partículas constitutivas de la materia. Ésta empezaría en 1897, con el descubrimiento del electrón, y avanzaría linealmente añadiendo nuevas partículas: fotones, protones, neutrones, positrones, neutrinos, etc. Sin embargo, el problema de los elementos constitutivos de la materia no era un tema central de la física en la década de 1930. En sentido estricto, se puede asegurar que antes de la segunda guerra mundial no existía una nueva disciplina de partículas elementales en física. De todos modos la aparición de nuevas partículas trajo consigo cambios radicales en el concepto de partícula elemental, perdiendo éstas algunas de las propiedades que hasta entonces se habían considerado fundamentales en una partícula «elemental».

ABSTRACT

The aim of this paper is to analyse the processes by which a number of new elementary particles were discovered and accepted by the scientific community in the years before World War II. Many popular accounts of particle physics depict a linear history of the search for the ultimate constituents of matter, which would start in 1897, with the discovery of the electron, and move on to an increasing number of new particles: photon, proton, neutron, positron, neutrino... Nevertheless, the question about the building-blocks of the material world seems to have been a secondary concern for physicists who were involved in the discovery of the new elementary particles in the 1930s. In the strong sense, one can argue that there was no such thing as a new discipline of particle physics before the Second World War. However, the introduction of new particles in the table of elementary particles brought radical changes in the concept of elementary particles, for these lost some of the fundamental properties that an 'elementary' particle was expected to have.

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1. Introduction

In May 1980, a large number of scientists and historians of science gathered at Fermilab for an international conference on the history of particle physics. The proceedings of this conference were published in a book entitled *The Birth of Particle Physics*, an expression that perfectly reflects the aim of L. Brown and L. Hoddeson to prove «that elementary particle physics evolved out of cosmic-ray and nuclear physics in the period 1930-50» [BROWN and HODDESON, 1983, p. xi]. However, the studies on the discovery of each of the new particles that have been written by professional historians of science pay little attention to the impact that the new discoveries had on the birth of a new discipline of particle physics. Certainly, there are some passing comments on the issue, which can be classified in two types: those who think that, before the outbreak of the war, elementary particle physics was already a field independent of other physical areas, such as nuclear physics, radioactivity, electrodynamics or cosmic rays; and those who maintain that, in 1939, there was no such thing as a new discipline of particle physics. The case of L. Brown is very significant. Still defending the general thesis stated above, his later works show that he is getting more and more inclined to defend that elementary particle physics had been born before the war, especially out of nuclear interaction theories and meson formalism that had begun with Yukawa's work[2]. Another date that is sometimes defended in attempting to locate the birth of elementary particle physics is the year 1932. A. Miller, for instance, said that «modern nuclear physics and elementary-particle physics began with... Heisenberg's (1932a)» [MILLER, 1984, p. 158].

H. Kragh also thinks that particle physics was born before the war, but he uses arguments from the experimental physics. «By 1930», he says, «physicists began experimental research on the atomic nucleus with the result that the harmonious two-particle consensus was soon destroyed. The 1930s saw a number of new particles and the beginning of a new speciality in physics, elementary particle physics. Together with research on the nucleus, cosmic radiation was the prime source of information about the new particles. Although cosmic radiation had been studied since the First World War, it became a major research area of physics in the 1930s» [OLBY, 1990, p. 668].

Other historians prefer to situate the origin of the new discipline after the war, with the discovery of the pion. A. Pais makes the interesting remark that «there is no sharp answer to the question: How old is particle physics? Still, if I had to single out one event to mark its beginning, I would choose the discovery of the μ meson in 1947 because of its superfluousness in the scheme of things at that time» [PAIS, 1961, p. 24]. In a similar way, S.S. Schweber writes that «modern particle physics can be said to have begun with the end of World War II» [KRIGE and PESTRE, 1997, p. 607], and H.L. Anderson writes that «it is reasonable to suggest as Alvarez has, that modern particle physics had its start in 1946, during the last days of World War II, when a group of young Italians, Conversi, Pancini and Piccioni, while hiding from the Germans, carried out a remarkable experiment» [COLLOQUE, 1982, p. 106], which led to the distinguishing of the μ meson and the π meson.

Certainly, the question of the configuration of a new field or discipline depends partly on the way that these concepts are defined. In the strong sense, it is clear that there was no such thing as a new discipline of particle physics before the Second World War. According to standard historiography, a scientific discipline has most of the following elements: (i) strong and stable communications between the practitioners, (ii) a number of agreed central problems and paradigms, (iii) professional organisations and research institutes devoted to these questions, (iv) journals and conferences dedicated to this area, (v) specific courses in the field as part of university education[3]. None of these elements were fully developed in the case of elementary particle physics in the 1930s. However, I would like to argue that there is still room for discussion on a related issue, i.e. the question of whether the new discoveries were creating a sense of crisis in the understanding of the constitution of matter.

The aim of this paper is to analyse the processes by which the new elementary particles were discovered and accepted by the scientific community in the years before World War II. Many popular accounts of particle physics depict a linear history of the search for the ultimate constituents of matter, which would start in 1897, with the discovery of the electron, and move on to an increasing number of new particles: photon, proton, neutron, positron, neutrino[4]... Nevertheless, the question about the building-blocks of the material world seems to have been a secondary concern for physicists who were involved in the discovery of the new elementary particles in the 1930s. As a matter of fact around 1930 there was a sense that many atomic phenomena had been successfully explained with the new Quantum Mechanics and there was a shift towards nuclear phenomena. This was possible due to the new experimental techniques available, such as electronic circuits and counters, photographic plates, etc. However, the almost philosophical question about the ultimate constituents of matter was not directly addressed. For the sake of my argument, I will first analyse the common characteristics in the processes of discovery and acceptance of the new particles[5]. Then I will present the first explicit tables of elementary particles that I have found in the scientific literature and the changes in the concept of an «elementary» particle in physics. I will finally discuss how the new particles helped to make a case for the need for highly expensive new scientific tools, especially the race for ever higher energy accelerators.

2. Common features in the discoveries of new particles in the period 1932-39

2.1. *Reluctance to accept new particles.*

In the 1920s, a simple system of three elementary particles had come to be accepted: two massive particles, the electron and the proton, with different masses and opposite charges, were the only 'bricks' out of which matter was built up. The photon, with neither mass nor electrical charge, was also regarded as an elementary particle. There were some serious problems with this schema, especially with the nuclear electrons, but its simplicity was sufficient to ensure its popularity. After the proliferation of elements in the periodical table in the 19th century, the existence of a three element table to explain all matter was very much welcomed. That is the reason why each new discovery of an elementary particle caused so much trouble among scientists.

J. Chadwick announced the existence of the neutron in a letter to *Nature* in February 1932. He was the first to break the spell that had discouraged the Berlin and Paris teams from forming the hypothesis of a new particle to account for the so-called Po-Be radiation[6]. This radiation had been widely studied for almost two years by W. Bothe and H. Becker in Berlín, by F. Joliot and I. Curie in Paris[7], and also by Chadwick himself in Cambridge; but none of these groups had thought to identify the radiation with a new neutral particle.

The 'neutron' that Chadwick was proposing was not, at first, a new elementary particle. In a special issue of the *Proceedings of the Royal Society*, in May 1932, physicists from the Cavendish Laboratory would stress that «the simplest hypothesis one can make about the nature of the particle is to suppose that it consists of a proton and an electron in close combination» [CHADWICK, 1932, p. 702]. The proceedings of the Solvay Conference, held in Brussels in October 1933, show that the nature of the neutron was a puzzle. It was not until the first half of 1934, with the new determination of the mass of the neutron by Chadwick and a young assistant of his, M. Goldhaber, that the neutron was widely accepted as an elementary particle[8], completing a two years 'period of discovery' [NAVARRO, 2001].

In the Solvay Conference, the interest in the neutron was shared with another just-identified particle: the positive electron or positron. As in the case of the neutron, the positron could have been identified earlier in the decade. R. Millikan, at Caltech, appointed C.D. Anderson to study the scattering produced by cosmic rays, having in mind the nature of cosmic radiation. By the autumn of 1931, Anderson had the first photographs of cosmic-ray scattering, which were «dramatic and completely unexpected» [ITHACA, 1962, p. 141], as he recalled many years latter. The number of positive particles was higher than expected, and there was no easy explanation of the fact that their behaviour was energetically similar to that of electrons, at a time when protons where the only known positive particles. Since the possibility of a new particle was not borne in mind, Anderson's explanation of some of his photographs was that «in rare cases, the tracks of curvature that indicate positives might be in reality electrons scattered backwards by the material underneath the chamber and are traversing it from bottom to top» [ANDERSON, 1932, p. 405].

The standard history of the discovery of the positive electron, which can be traced back to the 'official' history written by Millikan in 1935 to stress the importance of 'his' Norman Bridge Laboratory [MILLIKAN, 1935b, pp. 320-321], says that Anderson, in order to decide whether these particles were backwards coming electrons or positive particles, arranged an experiment that gave a photograph of a track that could only be interpreted as a positive electron[9]. However, the well-known photograph that was taken by Anderson did not provide conclusive evidence of a new particle. Anderson was aware that a single photograph, obtained almost by chance, in the less prestigious field of cosmic ray observations, could not be the definitive proof required to accept a new particle, and many times, in the following months, he regretted having published a note on the issue in *Science* [DE MARIA, IANNELLO and RUSSO, 1991]. It was not until the end of March 1933 that he made up his mind and sent a long article to *Physical Review* in which there was more evidence of the existence of positive electrons, although he still phrased it in moderate and speculative language [ANDERSON, 1933a].

Anderson's article appeared just at the time that P.M.S. Blackett and G. Occhialini, in Cambridge, were prepared to argue that positive electrons were present in cosmic rays in very high quantities, and not only as odd events, as Anderson had cautiously claimed. The experimental set that both scientists had managed to arrange was a very important step in controlling the stochastic field of cosmic-ray experiments[10], and in easing the way to the acceptance of the new particle. Even so, it was not until positrons were easily controlled in nuclear physics laboratories that the positron could claim to be a completely accepted new particle[11].

The third new particle to come on the stage, in the early thirties, was the neutrino. Its history is somewhat different from those of the neutron and the positron, since it was accepted long before it could be experimentally detected (which did not occur until 1956). In fact, the first proposal by W. Pauli in December 1930 was an «almost unbelievable suggestion» to account for the «desperate situation» [PAULI in VON MEYENN, 1985, pp. 39-40] that some fundamental principles were in. The reason for his proposal was both the statistical problem with some atomic nuclei and the conservation of energy in β-radiation[12]. As a matter of fact, the 'neutron' that Pauli proposed was actually a mixture of the neutron and the neutrino, as we now know them to be [BROWN, 1978]. I think it is important to point out that Pauli's proposal to include a new elementary particle was not taken seriously, not even by Pauli himself. In fact, it was not until the positive electron was discovered that Pauli «came back to his old idea of the existence of a 'neutrino'» [PAULI in VON MEYENN, 1985, p. 158]. However, his new 'neutrino' was now a compound of the positive and negative electron. It was Fermi's theory of β-decay, developed just after the Solvay Conference, which helped the neutrino to come to be accepted as a new elementary particle.

The neutrino was, during the thirties, a particle that was worth experimentally searching for and theoretically talking about. Some experiments were designed to try to detect neutrinos[13], but no direct evidence for them could be found. The conclusion was always that «if a neutral radiation is emitted (...) to compensate the energy distribution of the β rays, it must consist of particles of small mass and zero magnetic moment. Such particles would be exceedingly difficult to detect» [CHADWICK and LEA, 1934, p. 60]. Some theoretical physicists soon treated the neutrino as a fundamental particle, and tried to speculate about it. Fermi's theory of radioactivity was, of course, the main area in which neutrinos and their possible properties were studied[14], but de Broglie's proposal of a 'neutrino theory of light' and the scientific literature that it generated helps us to understand how seriously the neutrino was taken in the thirties. The main point of the neutrino theory of light was to consider that photons were not fundamental particles but a combination of both a neutrino and an antineutrino, and so to relate the electromagnetic processes (for which photons were responsible) to the nuclear forces (in which neutrinos were involved)[15].

By 1935, the well-established table of three particles had been modified to include new particles: the neutron, the proton and the neutrino. One might think that the atmosphere was ready for new particles to be easily accepted when experimental evidence was found. But the history of the so-called mesotron[16] proves this assumption to be wrong. In the 1934 Conference in London, Anderson and S.H. Neddermeyer expressed their surprise by the fact that «the penetrating power of the cosmic-ray [primary] electrons in lead as shown by cloud chamber experiments is greater than that permitted by theory in its present form» [INTERNATIONAL, 1935, p. 181]. Anderson and Neddermeyer were directly criticising the theory that H. Bethe and W. Heitler had recently developed on the interaction of electrons with matter, thus starting what P. Galison [1983 and 1987] has called a 'revolution against QED'. Other physicists, such as B. Rossi, P. Auger or L. Leprince-Ringuet, were arguing that cosmic radiation was formed out of two clearly distinct components. If both were thought to consist of electrons, it was clear that their behaviour depended on their energy[17], and that QED was wrong for high-energy electrons.

The other phenomenon involved in the discovery of the mesotron and the development of QED was the 'showers' of particles that were detected in cloud chambers. It was the close work of two teams in California (the experimentalist one formed by Anderson and Neddermeyer and the theorists J.F. Carlson and J.R. Oppenheimer) that opened the way to the suggestion that «the actual penetration of these rays has to be ascribed to the presence of a component other than electrons and photons; (...) if these are not electrons, they are particles not previously known to physics» [CARLSON and OPPENHEIMER, 1937, p. 220]. The way that led to the identification of the new particle was not to distinguish between cosmic-ray particles in terms of high and low energies, but to separate them into two groups, «one consisting largely of shower particles and exhibiting a high absorbability, the other consisting of particles entering singly which in general lose a relatively small fraction of their initial energy» [NEDDERMEYER and ANDERSON, 1937, p. 884]. With this distinction, Anderson and Neddermeyer were ready to state that «there exist particles of unit charge, but with a mass (which may not have a unique value) larger than that of a normal free electron and much smaller than that of a proton» [NEDDERMEYER and ANDERSON, 1937, p. 885]. This statement (together with the experiments that led to it) is usually considered as the starting point of the existence of mesotrons. However, the early acceptance of mesotrons as elementary particles is not clear, since they tried to think of them in terms of a «higher mass state of ordinary electrons» [NEDDERMEYER and ANDERSON, 1937, p. 885].

Actually, there were many experimental groups who were working on the new suggested particles, and different attempts to measure the mass of mesotrons were undertaken[18]. Although the results covered a wide range of possible values for the mass of the new particles[19], these attempts prove that, by 1938, there was almost no doubt that a new kind of particle had to be included in the table of elementary particles: the positive and negative mesotron.

2.2. *The role of 'theoretical predictions'*

A second aspect in the thirties was that every new particle had to be presented as a previously predicted entity in some not yet tested theory. Except for the case of the neutrino, in which the whole 'discovery' was nothing but a theoretical prediction, the identification of the new particles with some previous prediction was part of the unconscious precondition of accepting them, even though these identifications were not straightforward.

When the neutron was identified by Chadwick, a campaign started in the Cavendish Laboratory to stress the fact that the new particle was «a proton and an electron in close combination, the 'neutron' discussed by Rutherford in his Bakerian Lecture of 1920» [CHADWICK, 1932, p. 697]. It had been on that occasion that the then new director of the Cavendish suggested «the possible existence of an atom of mass 1 which has zero nuclear charge. Such an atomic structure seems by no means impossible. On present views, the neutral hydrogen atom is regarded as a nucleus of unit charge with an electron attached at a distance (...). Under some conditions, however, it may be possible for an electron to combine much more closely with the H nucleus, forming a kind of neutral doublet» [RUTHERFORD, 1920, p. 396]. The Cavendish Laboratory had played little role in the developments in theoretical physics in the 1920s, due to Rutherford's empiricist approach to science. However, the discovery of the neutron was seen as a possibility of gaining some prestige in theoretical physics, since it could be related to a theoretical prediction that was also made at the Cavendish. This was especially important at a time when the Cavendish needed to raise funds for new facilities and experimental equipment [HUGHES, 2000].

All the articles written in Cambridge and published in May 1932 in the *Proceedings of the Royal Society* stressed the fact that the neutron was the doublet that Rutherford had talked about in his 1920 Bakerian Lecture. It is quite clear that Rutherford's was only a suggestion, not a prediction. Ironically, this association between the particle that had been suggested in 1920 and the one that was found in 1932 was somehow responsible for the delay in the recognition of the neutron as an elementary particle, since the first announcements talked of the neutron as «a proton and an electron form[ing] a small dipole, or (...) the more attractive picture of a proton embedded in an electron» [CHADWICK, 1932, p. 702], it being «the first step in the combination of the elementary particles towards the formation of a nucleus» [CHADWICK, 1932, p. 706]. This view helped, at first, to make it possible to think of neutrons as nuclear constituents, just like a sub-structure such as alpha-particles. But the inconsistencies of this idea were evident very soon and, together with the determination of the mass of the particle, forced the acceptance of an 'elementary' neutron.

It was also in Cambridge that P.A.M. Dirac had speculated about the possible existence of a new particle, in his attempts to formulate a complete quantum-relativistic theory of the electron. Dirac at first thought of the negative energy states of electrons as a representation of protons, thus accomplishing the 'dream of the philosophers' to explain all matter with only one equation. However, Dirac soon realized that his 'holes' had to be interpreted as «a new kind of particle, unknown to experimental physics, having the same mass and opposite charge to an electron. We may call such a particle an anti-electron» [DIRAC, 1931, p. 61].

It is widely agreed that Dirac's suggestion had nothing to do with Anderson's interpretation of the positive electron. N.R. Hanson argued that

Blackett and Occhialini were the ones to undertake the 'meta-physical' discovery of the positron, in their 1933 paper [HANSON, 1963, p. 159]. It was in that article that the first link between the experimental discovery and the theoretical suggestion was established. However, Roqué [1997] has recently argued that Blackett and Occhialini did not take much advantage of Dirac's theory, since this theory was not very well regarded in 1933. In fact, this link did not even help the popularity of Dirac's theory [BRUSH, 1993]; but the need for some theoretical explanation of the new particle somehow forced the introduction of this relationship with Dirac's theory of 'holes', which was the only one that was available at the time.

More troublesome was the link that was established between the mesotron and a theoretically proposed particle by a then unknown Japanese physicist, H. Yukawa. When Anderson and Neddermeyer wrote their 1937 article about the 'heavy electron', the question arose of what its role was, since these particles were not observed in ordinary matter. The first written reaction to Anderson and Neddermeyer's article was by Oppenheimer and R. Serber, who said that it had «been suggested by Yukawa that the possibility of exchanging such particles of intermediate mass would offer a more natural explanation of the range and magnitude of the exchange forces between proton and neutron than the Fermi theory of the electron-neutrino field[20]». Yukawa's suggestion had been made in Japan more than two years before the experimental discovery of the heavy electrons and had gone almost completely unnoticed in the West.

From then on, many efforts were made to match the experimental data about the mesotron, especially regarding its mass and half-life, with the theoretical conditions of meson theories. In 1938 and 1939, there was a huge production of papers on what was called a 'meson theory of nuclear interaction'. The suggestion was also made that not only positive and negative mesotrons existed, but also neutral ones, to account for the charge independence of nuclear forces[21]. The identification of the heavy electron, mesotron or meson, with Yukawa's heavy quantum was broadly held before 1939, in spite of some serious incoherencies between theoretical and experimental results[22]. It should be recalled that all the experimental data were obtained in the cosmic-ray field, whereas the theory talked about nuclear phenomena. Step by step cosmic-ray investigations turned out to be one of the experimental grounds on which to develop nuclear physics research, in the same way that radioactivity had been until then.

2.3. *Simplifying attempts*

Before World War II, every new elementary particle was regarded as an undesired element increasing the number of fundamental 'building-blocks' out of which matter was made. The simple schema of only three particles, widely accepted before 1932, was too simple and clear for it to be abandoned without resistance. At first, as I have tried to show above, other possible explanations were attempted before accepting the existence of new elementary particles. But once that was no longer possible, other kinds of guesses were made, many of which were without much basis. The aim was to try to explain the ancient particles in terms of the new ones, and so to keep the number of elementary particles low.

The mass of the neutron proved to be a clue to establish whether the neutron could still be regarded as a compound of a proton and an electron. However, in discussing the way to determine this value, Curie and Joliot proposed a different reaction to that used by Chadwick[23]. According to their data, the mass of the neutron was higher than that of the sum of a proton and an electron. This meant that the neutron could no longer be thought to consist of 'a proton and an electron in close combination'. On the other hand, their value for the mass of the neutron proved useful for the hypothesis that Anderson had first made in early 1933, that «if the neutron should prove to be a fundamental particle of a new kind rather than a proton and a negatron in close combination, (...) the proton will then in all probability be represented as a complex particle consisting of a neutron and a positron» [ANDERSON, 1933b]. Pauli, in a letter to Heisenberg[24], and I. Tamm in correspondence with Dirac[25] were also quite keen on this idea.

Curie and Joliot defended this idea during 1933. At the Solvay Conference, the difficulties of knowing which was the 'really' elementary particle, the proton or the neutron, became all too apparent. As J. Perrin pointed out, «there is, no doubt, a complete symmetry, from the point of view of complexity, between the neutron and the proton; both particles shall be either two independent elementary particles, or both complex ones» [CONSEIL DE PHYSIQUE 7, 1934, p. 332]. By the beginning of 1934, there was almost no doubt that the neutron and the proton were two elementary particles.

Another simplifying attempt was that of the so-called 'neutrino theory of light', proposed by L. de Broglie in early 1934, which I have mentioned above.

The main point of this theory was that since neutrinos and antineutrinos seemed to be necessary particles to account for β-decay and nuclear interactions, photons could be thought of as aggregates of neutrinos. This was not only a simplifying guess, as far as the number of elementary particles is concerned, but also an attempt to explain both nuclear and electromagnetic forces in terms of one and the same theory[26]. As Brown and Rechenberg pointed out, the latter may have been the main reason for the echo that this proposal found among such theoretical physicists as P. Jordan or L. Kronig [BROWN and RECHENBERG, 1996]. By 1938, the neutrino was almost completely accepted, and attempts to attribute to it other roles than that which was given it by Fermi were completely abandoned, since the meson theories had changed the views on nuclear interactions.

However, Jordan kept speculating on ways to reduce the number of elementary particles. In his paper on the «Theory of Elementary Particles», written in December 1938, he tried to reduce all matter to neutrinos (both neutrino and antineutrino) and electrons (both positive and negative) [JORDAN, 1939]. With such a simplification the rest of the particles would appear as follows: both positive and negative mesotrons would be a compound of positive electron plus antineutrino and negative electron plus neutrino; as far as heavy particles were concerned, a proton would appear as consisting of a positive electron plus a neutrino and antineutrino, while a neutron could be regarded as a compound of two neutrinos and one antineutrino.

The last of these 'simplifying attempts' that is worth considering was the suggestion, made by Blackett, that the 'heavy electron' could be a 'heavy' electron; that is, an electron in a different state of mass. According to Blackett, «there are two main types of possible explanation. The first is that the particles are heavy when energetic, but change their mass suddenly, during collisions with nuclear fields (...). The second is that in which the penetrating rays are supposed to have the electronic rest mass, but are distinguishable from normal electrons by some unknown property» [BLACKETT, 1938, p. 106]. As the mass of the new particle was not clear, some thought it was possible to think of the mesotron as a normal electron which developed a greater mass after some Compton scattering [JAUNCEY, 1937]. As this mass seemed to be variable, there was also room for speculation on the need for neutrinos. «Allowing the expelled β-particle to have a variable rest mass thus removes the need for postulating the neutrino» [JAUNCEY, 1838, p. 106].

3. Tables of elementary particles from 1930 to 1939

If scientists had had a primary interest in discovering the constitution of matter in the thirties, there would have probably been a great number of tables of elementary particles in both esoteric and popular writings, as was common from the fifties onwards. However, there are few such tables, and they appear only implicitly in different contexts, especially in writings on nuclear physics. Most of the tables are condensed into little paragraphs, at times very speculative ones, in theoretical or experimental articles. Only on one occasion, in the proceedings of a Conference in Warsaw in 1938, have I found a picture of a table of elementary particles.

We should start with the standard table of particles that was peacefully accepted before 1932, with three elements: a proton (a positive particle responsible for the mass in matter), an electron (a negative particle responsible for electric phenomena) and a photon (a particle with no charge and no rest mass, responsible for the energy transitions in atoms). The simplicity of this table, which made it worth maintaining, has been stressed throughout this paper.

	Positive	**0-Charge**	**Negative**
High mass	PROTON		
Little mass			ELECTRON
No mass		PHOTON	

In February 1934, an article with the title 'New Particles' appeared, with the recent discoveries of new particles, such as the neutron and the positron, but also the deuteron. This article, written by the science journalist J.G. Crowther, asked that since «all matter is made of multiples of some primordial units; what are these primordial units?» The answer to that question is quite significant, because it reveals the strength of Joliot and Curie's proposal, as well as showing the atmosphere of deep change that was present in the scientific community and in the general public: «Until a year ago they were believed to be the electron and the proton. The electron is the unit of negative electricity and the proton was believed to be the unit of positive electricity. All the matter in the universe is built of protons and electrons. But within the last few months the belief that the proton itself is complex has been strengthened. It is now believed that the proton consists of a neutron and that new particle, the positive electron (...) As the neutron is 2000 times heavier than either the negative or positive electron, the mass of the material of the universe may be due almost entirely to neutrons. (...) In 1934 there is evidence that 999 out of every 1000 parts of the mass of matter in the universe is a drama in the medium of neutrons, in which electrons, positrons and photons provide the action and the lighting» [CROWTHER, 1934, p. 209].

The table corresponding to this paragraph, in which the neutrino is still missing, would be as follows:

	Positive	**0-Charge**	**Negative**
Mass		NEUTRON	
Action	POSITRON	PHOTON	ELECTRON

Indirectly, this schema opened the door to the possibility of a negative proton, a particle about which there was some speculation after the discovery of the positive electron. However, with this table in mind, the possible negative proton need not be a new elementary particle, in the same way that the positive proton was not.

Although the question of whether or not the proton and the neutron were elementary particles was almost settled by 1934, we can still find some later doubts, such as those expressed by Millikan in 1935, when he wrote in *Science* that «we need now at least 3 fundamental elements, namely, either (1) positive and negative electrons and neutrons or (2) positive and negative electrons and protons» [MILLIKAN, 1935a, p. 215]. Even in 1936, Allan Ferguson, who was the president of the physics and mathematical section in the British Association for the Advancement of Science, expressed his doubts by saying that «whether the neutron is an elementary particle and the proton may be written as neutron + positron, or whether we have more justification for considering the neutron as proton + electron are matters which cannot be discussed in detail here» [FERGUSON, 1936, p. 404].

In a very different context, Heisenberg tried to present a unifying table for elementary particles and their roles in the structure of matter. This was in an article, written in 1935, to defend the Fermi field theory. That is the reason why its importance as a table of elementary particles is only indirect, for it was mainly designed to compare the electromagnetic field with the Fermi field [HEISENBERG, 1935, p. 110].

	Atomic shell	**Atomic Nucleus**	
Elementary components	NUCLEI and ELECTRONS	PROTONS and NEUTRONS	
Particles emitted in transitions	LIGHT QUANTA	ELECTRONS POSITRONS NEUTRINOS	LIGHT QUANTA
Corresponding field	MAXWELL FIELD	FERMI FIELD	MAXWELL FIELD
Interaction	COULOMB FORCES	EXCHANGE FORCES	COULOMB FORCES

The development of the Fermi field theories had consolidated the following as elementary particles: the proton, the neutron, both electrons (positive and negative), both neutrinos (neutrino and antineutrino) and the photon. Thus there were seven elementary particles, each one with a specific role in atomic and nuclear phenomena. But, as the mesotron was discovered, the number increased by two (or three, if we also consider the neutral meson), which forced new unifying attempts. As A. Proca and S. Goudsmit wrote in 1939, «the number of particles that one can consider as elemental in physics has increased in the past years in such a way that the return to unity is now necessary». To do so, they held that «the opinion that the rest masses of the elementary particles are nothing but the eigenvalues of one operator and that the different particles can be considered as different states of the same fundamental system should be developed» [PROCA and GOUDSMIT, 1939]. In fact, what they were proposing, following Neddermeyer's ideas for the mesotron, was to consider a quantification of the mass. Proca and Goudsmit also echo Jordan's proposal that all particles be made out of positive and negative electrons plus neutrinos.

Less optimistic, as far as reduction of the number of particles was concerned, was the Indian theoretical physicist, working in Britain, J.H. Bhabha, who was seriously committed to the development of meson theories. In order to justify the existence of a neutral mesotron, which he thought necessary to account for the charge independence of nuclear interaction[27], he thought of a symmetric table of elementary particles in which these were arranged according to both their charge and their mass, each one with three possible values. «The U-particles [the mesotrons] being charged, they cannot explain the close range proton-proton interaction. To formulate this —he said— we would have to introduce a neutral particle N of about the same mass M_U (...). The introduction of such a particle may not seem very arbitrary when we consider that it would give us a symmetrical state of affairs, with all the particles falling into three groups with masses of the order $M_P \approx 1840m_e$, $M_U \approx 200m_e$, and m_e, there being positive, negative and neutral particles of each group» [BHABHA, 1938, p. 117]. The table of such a proposal, in which the possible existence of a negative proton is allowed, would be as follows:

	Heavy	**Medium Mass**	**Light**
Positive	PROTON	POSITIVE MESOTRON	POSITRON
Neutral	NEUTRON	NEUTRAL MESOTRON	NEUTRINOS and PHOTON
Negative	NEGATIVE PROTON?	NEGATIVE MESOTRON	ELECTRON

To finish this section, I shall present the only explicit picture of a table of elementary particles that I have found in the scientific literature [LES NOUVELLES, 1939, p. 162]:

	Masse au repos	**Charge**	**Spin**
Electron	m_0	$-e$	1/2
Positron	m_0	$+e$	1/2
Electron lourd	100 à 200 M_0	$\pm e$	1
Neutron	M_n	0	1/2
Proton	M_p	$+e$	1/2
Photon	0	0	1 (ou 0 ?)
Neutrino	0	0	1/2

This table appears in the communication that L. Brillouin presented in the Conference organised by the International Institute for Intellectual Cooperation in Warsaw in 1938. The name of the Conference was «The New Theories in Physics», and Brillouin's communication dealt with the problem of elementary particle statistics[28]. This table, with a doubt expressed in a footnote as to whether or not the 'neutral heavy electron' is needed, can be considered as the standard table of elementary particles that most scientists had in mind just before World War II. However, some thought that more changes were going to happen, and that more particles might appear. As Brillouin wrote, «the elementary particles in physics have been multiplied and their list has not been completely finished for the time being» [LES NOUVELLES, 1939, p. 161].

This last sentence agrees with S.S. Schweber's suggestion that the discovery of the mesotron started a period of chaos which lasted until 1952[29]. However I think that in the specific period between 1937 and 1939 this does not't completely apply. Before World War II, in spite of the increase in the number of particles, the possibility of attributing a nuclear role to each one of the new particles made the situation somehow different from the one after the discovery of the muon and the so-called 'strange' particles. Then it was much more difficult to understand their role in the structure of matter. As Pais once asked, «what was the muon useful for?» [PAIS, 1986, p. 454]. This question could be postponed until after the War, when the discovery of the π-meson and the conversion of the mesotron in its decay product made the 'uselessness' of the particle apparent.

4. Evolution of the concept of elementary particle

In the introduction to the article cited above, Brillouin explains quite well what an elementary particle was before 1933. «I think it is necessary», he said, «to look some years back, if one wants to understand what role the concept of elementary particle plays in physics nowadays. Ten years ago we were used, due to chemistry and physics, to consider all matter constituted of atoms, which were at the same time formed out of a nucleus and electrons; radioactivity proved the complex constitution of nuclei, so that it seemed that the only fundamental elements were the proton and the electron, both with a given mass, electric charge and spin. Means to transform, create or annihilate neither the electron nor the proton were known, so that one could define them as 'fundamental permanent particles'» [LES NOUVELLES, 1939, p. 125].

In the twenties, elementary particles were those 'fundamental bricks', out of which all matter happened to be 'built', as mechanistic philosophers in the seventeenth century, or even Dalton, thought. Particles were thought to be permanent and completely simple. In this context, photons were an exception, since they were created and annihilated in energy transitions; there was not a fixed number of them. However, this seemed to be a minor problem, since photons were not 'constituents of matter' in the strong sense of the expression. The discovery of new particles and of new phenomena related to them helped in some way to break this image of what an elementary particle was.

Perhaps the first revolutionary change took place when the positive electron became the 'anti-electron' and, with it, the idea that particles were created and annihilated was consolidated. Blackett and Occialini's photographs showed that the positron usually appeared connected to a negative electron. «There are three possible hypotheses that can be made about the origin of these particles», they wrote. «They may have existed previously in the struck nucleus, or they may have existed in the incident particle, or they may have been created in the process of collision. Failing any independent evidence that they existed in separate particles previously, it is reasonable to adopt the last hypothesis. Further, in view of the well-known difficulties in treating electrons in a nucleus as independent mechanical entities, the last hypothesis becomes perhaps more convenient» [BLACKETT and OCCHIALINI, 1933, p. 712]. The appearance of electron-positron couples was the first time that the creation of particles was observed. Dirac had already interpreted the transition of an electron from a negative energy state to a positive one in his equation in terms of a creation-annihilation process; but as Oppenheimer explained in 1963, Dirac's 'hole theory' did not lead to the acceptance of creation-annihilation processes, nor did it help the starting of an experimental program to find out the validity of such a speculation[30].

It should also be stressed that creation and annihilation were two processes that were experimentally discovered one after the other. Blackett and Occhialini, as well as Joliot and Curie, provided evidence, in early 1933, of the simultaneous appearance of an electron-positron couple. The opposite process, i.e. the annihilation of two particles giving rise to photons, was not observed until late 1933 by J. Thibaud. However, this was an expected process which caused no trouble among scientists. «These results», Thibaud wrote, «constitute the first experimental test in favour of a process, long expected, of annihilation of matter» [THIBAUD, 1933, p. 1631].

The discovery and acceptance of the positron had many implications, as far as the change in the concept of an elementary particle is concerned. It was the first discovery to break the schema of three particles (proton, electron and photon). But it also introduced the more radical problem that 'elementary' particles, at least the light ones, could be transformed into radiation or created from it. This considerably helped the understanding of the β-radiation process; and, indirectly, it also helped the idea of the neutron as a completely elementary particle. Since electrons could be created and annihilated, there was no need for them to exist in the nucleus before the emission, either alone or forming 'compound' particles such as neutrons.

Fermi's explanation of the β-process made use of this idea, together with Pauli's modified proposal of a neutrino, but at the same time it introduced a new conception of the elementary particles: their instability and consequent decay. The first time that this possibility was expressed was in a letter to Physical Review, dated the 11th July, 1934, and written by H.G. Wolfe and G.E. Uhlenbeck, then in Michigan. They applied Fermi's theory to the possible β-decay, not of a nucleus as a whole, but of a proton or neutron alone. «If the mass difference of proton and neutron is larger than the sum of the masses of electron and neutrino, then the Fermi theory predicts the spontaneous occurrence for the free particle of either the transition (1) or the transition (2), depending on the sign of the mass difference» [WOLFE and UHLENBECK, 1934, p. 237], (1) and (2) being the following transitions:

$$(1) \qquad N \rightarrow P + e^- + n$$

$$(2) \qquad P \rightarrow N + e^+ + n$$

It should be remembered that, by the time this letter was written, the polemics about the mass of the neutron had not totally come to an end. When this was settled, in August that year, it was clear that the neutron could spontaneously decay. As M. Goldhaber recalled many years later, «I remember being quite shocked when it dawned on me that the neutron, an 'elementary particle', as I had by that time already learned to speak of it, might decay by β-emission with a half-life that I could roughly estimate from the existing systematics of the β-decay of complex nuclei to be about half an hour or shorter, since the neutron was a simpler system[31]» [STUEWER, 1979, p. 88].

However, it was not the neutron, but the mesotron, that provided experimental evidence for such decay. Since Yukawa's proposal of a U-quantum and the later assignment of the 'heavy electron' to play such role, it was clear that the mesotron could be regarded as a 'relativistic clock', since it could decay spontaneously, and the measured time of decay would depend on its speed. A photograph obtained in late 1939 by E.J. Williams and G.E. Roberts was the first time that a mesotron was seen to turn into an electron (and presumably neutrinos). This new property of 'elementary' particles removed one of their most important properties: their immutability, giving rise to what Schweber calls «new 'impermanent' particles» [KRIGE and PESTRE, 1997, p. 608].

A third change in the concept of elementary particle was that particles could be assigned a new mission in matter: that of being vehicles for nuclear interactions, similarly to photons. The starting point in this change was Heisenberg's theory of nuclear interaction, prior to Fermi's and Yukawa's proposals, and in which nucleons were bound together thanks to the interchange of an electron. «Heisenberg's theory introduced nuclear forces of the exchange type. That is, the force between neutron n and proton p involved their 'sharing' an electron (...) At the cost of blurring the distinction between 'composite' and 'elementary', Heisenberg's theory was able to deal with nuclear states and their transitions» [BROWN and RECHENBERG, 1996, p. 31].

Elementary particles could be created or annihilated, could decay into other particles and in this way could explain nuclear interactions. The classical concept of 'elementary' particles was being little by little replaced by that of 'fundamental' particles, their role in the structure of matter being no longer of a straightforward mechanistic kind.

5. New experiments, new theories

One of the main characteristics of the discipline of elementary particle physics from the 1950s was the creation of new accelerators, which were able to work with particles at energies higher than those provided by cosmic rays. The smaller the particle to be studied, the bigger the machine that was needed. The race for bigger accelerators started in the early thirties, when E.O. Lawrence built the first cyclotron in California. The question that I want to address here is the relationship between the first accelerators and the initial history of particle physics in the thirties.

In 1932, the first important result achieved by an accelerator was presented. It was the first nuclear disintegration, performed by J.D Cockroft and E.T.S. Walton, in June 1932. G. Gamow's theory of nuclear structure predicted that accelerated protons would be able to disintegrate light nuclei, and Rutherford patronised the construction of a large linear accelerator in order to undertake such experiments. Cockroft and Walton's accelerator managed to reach energies of up to 0.7 MeV. The experiment was confirmed by

Lawrence's circular accelerator in California, which could provide energies of up to 1 MeV.

Lawrence managed to make a considerable profit from his accelerator by obtaining easier radioactive isotopes for medical use, which helped him to raise money and build better circular accelerators[32]. Meanwhile, other laboratories in Europe and in the United States, kept working with linear accelerators. These accelerators could not provide energies higher than those provided by cosmic rays, which were the source for mesotrons in the thirties and the forties. Nevertheless, Lawrence was aware that more powerful accelerators were needed in order to study mesotrons in the laboratory, and to stop depending on the unpredictable photographs of cosmic-ray traces. In this sense Lawrence was advocating, in 1939, a cyclotron of at least 120 inches, which would thus have energies of 100 MeV to produce mesotrons. At that time, a comparison was made between such an accelerator and the great telescope that had been planned on Mount Palomar. The latter would reveal the secrets of stars and galaxies, while the former would help us to understand the intimate structure of fundamental particles [HEILBRON and SEIDEL, 1989]. This accelerator had to be delayed because of the outbreak of the War; when it was finally built, it was 184 inches long. But even then, the new particles were discovered in the less predictable field of cosmic rays. It is interesting to point out the fact that, when Lawrence talked about this new accelerator before the war, he was thinking of a means to control and to better study the already known mesotrons, but not to create new particles, as happened in the mature discipline of elementary particles[33].

In a completely different context, i.e. that of theoretical physics, the first attempts at a theory of elementary particles appeared when Pauli and V. Weisskopf tried to explain the process of creation-annihilation without using Dirac's unpopular 'holes'. The 'anti-Dirac' theory (as Pauli used to call it) was only consistent for spin 0 particles. This was prior to Yukawa's formalism of his heavy quantum; and, as S. Sakata recalled many years later, «this research was of a purely formal interest at that time» [BROWN and RECHENBERG, 1996, p. 144]; the topic remained speculative until the π meson was discovered.

The first general attempt to describe not only the already known particles, but also *any* possible particle, was worked out by Dirac in 1936. As a matter of fact, he tried to develop a relativistic equation for particles of spin 0 and spin 1. However, the way in which he presented it suggests that his efforts were nothing but mathematical developments with little physical basis, and even less experimental basis. In 1936, he wrote that «the elementary particles known to present-day physics, the electron, positron, neutron and proton, each have a spin of a half, and thus the work of the present paper will have no immediate physical application. All the same, it is desirable to have the equations ready for a possible future discovery of an elementary particle with a spin greater than a half, or for approximate application to composite particles. Further, the underlying theory is of considerable mathematical interest» [DIRAC, 1936, p. 448].

This article is referred to many times in 1938 and 1939, and also in some post-war articles, as the starting point for theories of particles of any spin. These pre-war attempts were very hesitant; and few people were working on them. Together with M. Fierz, Pauli wrote an article about 'particles of arbitrary spin' [FIERZ and PAULI, 1939], and, with F.J. Belinfante, another one about 'known and unknown particles' [PAULI and BELINFANTE, 1940]. Also Pauli's former pupil, N. Kemmer, would take part in the development of the so-called meson theories.

In a letter to Dirac, in 1938, Pauli wrote that, at first, he had thought, together with Fierz, that «no elementary particles (...) with a spin greater than 1 can exist» [VON MEYENN, 1985, p. 608], but, by the time their article was going to be published, the conclusion was very different, «because it turned out that we had overlooked an important possibility and a consistent theory of particles with arbitrary spin in an electromagnetic field is possible» [VON MEYENN, 1985, P. 617]. However, this does not immediately imply that Pauli and Fierz were seriously thinking that higher spin particles really existed, for «the theory for such particles is considerably more complicated than for smaller spin values» [FIERZ and PAULI, 1939, 231]. During 1939, Pauli's opinion on higher spin particles changed quite a few times. In April, spins higher than 1 seemed to be out of the question, thanks to new mathematical developments by Kemmer, which made Pauli feel at ease[34]; but, only one month later, he finished a postcard to Kemmer with the expression «up to a higher spin[35]!», which reflected his new belief in possible high spin particles.

To conclude the discussion of this subject, it should be remembered that Pauli and Heisenberg had started to work on a communication for the Solvay Conference that was going to be held in late 1939, but which could not take place because of the war. Heisenberg was expected to talk about the general properties of elementary particles. However, the topic was so ambiguous that he could not help asking what exactly it meant [VON MEYENN, 1985, P. 629]. Their work together for this conference led to the theorem of the relation between spin and statistics, which was published during the war in *Physical Review* and in the *Review of Modern Physics*.

All these attempts before the war show that theoretical physicists were also trying to explain, in a unitary way, all the elementary particles that were known at the moment. It is true that many of these developments played a central role in the birth of a particle physics discipline, and that, in some cases, speculation about possible new particles was discussed; but before the war there was no general interest in starting to talk about new and 'superfluous' particles, something which didn't occur until the mesotron, which turned into a μ-meson, proved not to play a central role in the constitution of matter.

6. Conclusion

The discovery of new particles in the period 1932-1939 forced big changes in the table of elementary particles, but this fact didn't imply the birth of a new discipline in physics. The discoveries of positrons and neutrons can be better understood when they are considered together, not only because they happened at the same time, but because this coincidence helped physicists to accept that the number of elementary particles had to increase. However, their acceptance brought radical changes in the concept of elementary particles, for these lost some of the fundamental properties that an 'elementary' particle was expected to have. The creation-annihilation process, which turned the positive electron into an anti-electron, helped to bring about this change of mind, which came together with the first theories of nuclear interactions and Fermi's explanation of β-decay. 'Inner' electrons, as Marie Curie used to call them, ceased to pre-exist in the nucleus, and were created, together with a new particle, i.e. the neutrino, when emitted. At the same time this meant that not only the nucleus but also 'elementary' particles such as neutrons decayed and had relatively short half-lives. The heavy electrons, or mesotrons, were also unexpected guests, but in a similar way to what had previously happened, they were also accepted once they had a fundamental nuclear role attributed to them.

The new entities were discovered in the well established fields of nuclear and cosmic-ray physics, and to these fields they belonged. The increase in the number of elementary particles didn't mean the immediate creation of a new discipline, nor even a major revival of the cosmological questions about the constitution of matter. Popular physics in the thirties was far from being dominated by «particle hunters». From a retrospective point of view, many conceptual and social changes still had to take place before a discipline of elementary particles could be consolidated.

NOTES

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2. This is what he claims, for instance, in his collaboration in the work *Twentieth Century Physics* [BROWN, PAIS and PIPPARD, 1995, p. 401], or in the book *The Origin of the Concept of Nuclear Forces*, written together with H. Rechenberg [BROWN and RECHENBERG, 1996, p. 112].
3. HENTSCHEL [2002, p. 420] uses a similar characterization to study the discipline of spectroscopy.
4. See, e.g., NE'EMAN and KIRSCH [1986]; TREFIL [1980]; GELL-MANN [1994]
5. My aim is not to write a history of the discovery of each of the particles, since many historians have largely done this, but to analyse the common features in them.
6. For a classical history of the discovery of the neutron see, e.g., SIX [1987] and ITHACA [1962]
7. SIX [1988] makes a very interesting study of why Joliot and Curie did not discover the neutron, in spite of the fact that they had almost the same experimental evidence as Chadwick in Cambridge.
8. CHADWICK and GOLDHABER [1934]. For a complete history of the dispute on the mass of the neutron see STUEWER [1993]. Certainly, Heisenberg was the first to claim that the neutron was an elementary particle; but this was not widely accepted until mid-1934.
9. Some histories of the discovery of the positron can be found in ANDERSON [1961]; HANSON [1963]; DE MARIA and RUSSO [1985]; GALISON [1987]
10. BLACKETT and OCCHIALINI [1932] is the first article in which the coincidence method is explained: two Geiger counters in the edges of the cloud chamber allow an automatic photograph only when a charged particle has been detected by both counters, and so it is almost certain that the particle has crossed through the

chamber. BUSTAMANTE [1997] has studied the importance of Blackett's improvements in cosmic-ray research during the thirties.

11. ROQUE [1997], stresses the fact that during 1933 the positive electron was widely accepted, thanks to the fact that it could be studied by those scientists who were devoted to radioactivity, which was a much more controllable and prestigious field than cosmic-ray studies. This study was very closely related to the most astonishing phenomenon of the electron-positron pair creation and absorption.
12. The history of the principle of conservation of energy and its problems regarding the spectra of b-emission have been widely explained. See Pais, A. in DONCEL [1987]. See also JENSEN [2000].
13. Some of the most remarkable attempts were done by Chadwick and Lea in 1934, Bethe and Peierls in 1934, Nahmias in 1935, Leipunski in 1936, Crane and Halpern in 1938, Alichanian and Nikitin in 1938 and by Crane in 1939.
14. Among these properties, that of the mass of the neutrino and its ionisation power were the most important, for they were related to the coherence of Fermi's theory as well as to the possibility of detecting them.
15. By 1938, the neutrino theory of light, in which P. Jordan was very much involved, was abandoned after M.H.L. Pryce proved its inconsistency (cf. PAIS [1986, p. 419], and after it had been made clear that there were two very different nuclear forces (what we now call the weak and strong forces). This last element was important since it led to the abandonment of the search for a unified field theory.
16. The mesotron was the name given to the particle discovered by Anderson and Neddermeyer, which was later called the muon or m-meson after World War II. However, other names such as 'heavy electron', meson or mesoton may appear in this article, since there was no consensus on the name to use for this particle just after its discovery.
17. Anderson [1961, p. 828]: «This then was the situation in 1934 in which the sea-level penetrating particles had this paradoxical behaviour. They seemed to be neither electrons nor protons. We tended, however, to lean toward their interpretation as electrons and 'resolved' the paradox in our informal discussions by speaking of green electrons and red electrons —the green electrons being the penetrating type, and the red the absorbable type which lost large amounts of energy through the production of radiation».
18. The first experiments were carried out by Street and Stevenson and by Nishina, Takeuchi and Ichimiya.
19. The following are some of the values obtained by the different teams, compared to that of the electron:
    $m = 130\ m_e$, Street and Stevenson (in 1937), Harvard;
    $m = 350\ m_e$, Corson and Brode (in 1938), California;
    $m = 200\ m_e$, Williams and Pickup (in 1938), Liverpool;

$m = 240\ m_e$, Neddermeyer and Anderson (in 1938), Caltech;
$m = 180\ m_e$, Nishina, Takeuchi and Ichimiya (in 1937), Tokio;
$m = 240\ m_e$, Leprince-Ringuet, Gorodetzky, Nageotte and Richard-Foy (in 1940), Paris.

20. OPPENHEIMER and SERBER [1937, p. 1113]. Similarly, STUECKELBERG [1937, p. 41], says that «it seems highly probable that Street and Stevenson, and Neddermeyer and Anderson have actually discovered a new elementary particle, which has been predicted by theory».
21. The first suggestions came from KEMMER [1937] and especially BHABHA [1938]
22. Among these problems, the difference between the mass and the half-life of the theoretical and the experimental particle was one, but as neither of them was clearly established, the contradiction could be maintained. More difficult to understand is the fact of the spin: while the experimental particle was regarded as a heavy electron, with a corresponding 1/2 spin, the early meson theories needed a spin 0 particle.
23. Chadwick used the reaction ${}^{11}B_5 + {}^{4}He_2 \rightarrow {}^{14}N_7 + {}^{1}n_0$, while Curie and Joliot thought it was worth using the possible reaction ${}^{10}B_5 + {}^{4}He_2 \rightarrow {}^{13}C_6 + {}^{1}n_0 + e^+$.
24. Pauli to Heisenberg, 14 July 1933, in VON MEYENN [1985, pp. 185]: «About the possibility of discussing further the exchange forces from a point of view of conservation of angular momentum, we should still consider the following: (a) Certainly a neutron may never disintegrate into an electron and a proton, but perhaps in a more sophisticated way into a proton, an electron and a neutrino; or (b) a proton may disintegrate into a neutron and a positive electron (Anderson). [My translation]
25. Tamm to Dirac, 5 June 1933, in KOJEVNIKOV [1993, p. 64]: «Many times this year I was about to write you, especially after Blackett's and Occhialini's paper appeared. I got used to say that your prediction about the existence of the antielectron has no parallel in history of science (...). Your theoretical prediction about the existence of the antielectron, being unstable in the 'ordinary space' outside the nucleus, seemed so extravagant and *totally new*, that you yourself dared not to cling to it and preferred rather to abandon the theory. And now the experiment unexpectedly proved you to be right and even presented you with the neutron, to make the 'hole' stable with and to form a proton!»
26. It may be useful here to recall that at that time, both strong and weak nuclear interactions (in present day terminology) were supposed to be explained in terms of Fermi's theory.
27. The first experimental evidence for charge independence came from Tuve's experiments in Washington in 1936.
28. Brillouin says, at the beginning of his communication, that Dirac was the one who was appointed to talk about that subject. The correspondence of Dirac, which I have been able to read, gives no trace of any such thing.

29. Schweber in KRIGE and PESTRE [1997, p. 600]: «The search for the ultimate constituents of matter has had a cyclic history since its inception at the beginning of the nineteenth century. Each stage was initially characterised by incoherence. But the confusion gave way to a measure of clarity through classification, and with the help of the latter the empirical data reduced to some measure of order. Once that order was ascertained new level of substance and structure was discovered, and became charted with the help of new instruments and technologies. Again incoherence and confusion reigned until regularities operating at the level were discerned, classified and modelled.»
30. Interview with J.R. Oppenheimer, 20 November 1963, p. 25. (Archive for the History of Quantum Physics): «I must have seen Dirac's note on electrons and protons shortly after it came out. I think that year (1929-1930) I went first to Berkeley and came at Christmas time to Pasadena. My recollection is that I saw this in Pasadena. I guess the following note, or actually paper, on radiative transitions had something about the annihilation. You could then ask 'what did I think?' (...) I don't think that I thought about mechanisms which would produce pairs until the Anderson thing. I think that I had no opinion as to whether this conclusion of the theory would be born out. (...) I talked to Anderson about it. (...) Before the positron? Sure, and he talked to me, but I didn't encourage him to think that this was a good experiment and he didn't look for positrons because there might be a place for them in a theory of whose general rightness no one was at all sure».
31. The lifetime of the neutron (around 11 minutes) was first measured in 1950.
32. HEILBRON and SEIDEL [1989]. They stress the fact that all the cyclotrons built in the thirties in the States were used mostly in medical research, leaving fundamental physical research aside.
33. Lawrence wrote on 21st February 1940: «The discovery of mesotrons in cosmic rays will be of little value in the course of time unless there is developed a way of producing them, and learning of their manifold properties —ultimately to be put in the service of mankind». HEILBRON an SEIDEL [1989, p. 473]
34. Pauli to Kemmer, 9 April 1939, in VON MEYENN [1985, p. 623]. He writes: «Es befriedigt mich zu sehen, dass wieder bei positiv definiter Energiedichte keine höheren Spins als 1 vorkommen können.»
35. Pauli to Kemmer, 10 May 1939, in VON MEYENN [1985, p. 652]. The expression in German is: «Auf zum höheren Spin!»

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