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Life of μ: The Observation of the Spontaneous Decay of Mesotrons and its Consequences, 1938–1947

Daniela Monaldi

IHPST, University of Toronto, 91 Charles St. W., Toronto, ON, M5S 1K7 Canada.
E-mail: daniela.monaldi@utoronto.ca

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Summary

The mesotrons, or mesons, were the first elementary particles observed to be inherently unstable. This essay offers a reconstruction of the stream of researches related to mesotron decay, and examines how these researches shaped some of the basic concepts and practices of the emerging field of particle physics. Mass measurements could not settle the question of whether the mesons were a homogeneous kind of particles or an assortment of particles with different masses. The assumption of a single mass prevailed not on experimental grounds but because the mesons were identified tentatively with the carriers of the nuclear force according to a theory formulated by Hideki Yukawa. The identification gained currency because it entailed the prediction of meson decay, and thereby upheld the promise of a unified explanation of nuclear and cosmic-ray phenomena. In turn, the observation of decay and the measurement of the mean lifetime created the conditions for investigating the nuclear interactions of mesons at rest. Interest in these interactions was heightened, immediately after WWII, by the prospect of building and using accelerators to acquire knowledge about fundamental nuclear processes. Using decay to study nuclear capture, however, led to the realization that there exist not only different kinds of mesons but also two nuclear forces.

Contents

1. Introduction

The spontaneous decay of elementary particles was first observed by cosmic-ray researchers around 1940. The particles observed to be unstable were the recently discovered *mesotrons*, or *mesons*, and their disintegrations were thought to be the most elementary form of β-radioactivity. As the theoretical understanding of mesotrons changed over the years, so has their name. They are known today as *muons*, and their decay is considered to be an independent phenomenon governed by the same

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fundamental force as nuclear β-decay. Muons, in fact, are not the only unstable particles. Modern physics is characterized by the transience of its fundamental entities.

How can a particle decay? As long as we think of decay in the atomistic sense, as the decomposition of an aggregate into its elementary constituents, either a particle cannot decay or it is not elementary. But the advances of microphysics in the early twentieth century were based on a fundamental similarity between material particles and electromagnetic waves. Waves are not permanent: they are emitted and absorbed. It is an underlying axiom of modern physics that material particles also can surge and disappear like ocean surf. This post-atomistic conception emerged from late-nineteenth-century unitary theories of matter and light, the relativistic mass-energy equivalence, and the early conceptions of wave–particle duality. It took shape at the end of the 1920s in the notion of 'matter fields' and found full formal expression in the structure of quantum field theory.[1] Nevertheless, it became concrete and operational in physical research only gradually, through a complex interplay of mathematical representation, conceptual understanding, experimental interpretation, and instrumental techniques. This transition from the abstractly general to the concretely general passed through the specific cases of a few particles. The creation and annihilation of electron-positron pairs is the best known of them.[2] But while the recombination of matter with antimatter had old conceptual roots, a real novelty of the new physics was the possibility for an elementary entity to disappear without external cause, bequeathing its energy, charge, and other conserved quantities to a newly created set of different entities. The first particle predicted to 'decay' in this way was the neutron. The prediction, however, was not linked to a suitable experimental setting and remained hypothetical until 1948.[3] The particle that translated the abstract possibility of decay into an actual phenomenon was the mesotron. Therefore, examining the history of the observation of mesotron decay can offer a useful perspective on events that bridged the formulation of quantum field theory and the shaping of laboratory practices in high-energy physics.[4]

The story of the mesotron is a popular chapter of particle physics history.[5] A typical account runs as follows. Upon its discovery in 1937, the mesotron was

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[1] S. S. Schweber, *QED and the Men Who Made It: Dyson, Feynman, Schwinger, and Tomonaga* (Princeton, NJ, 1994).

[2] M. De Maria and A. Russo, 'The Discovery of the Positron', *Rivista di Storia della Scienza*, 2 (1985), 237–86; Xavier Roqué, 'The Manufacture of the Positron', *Studies in History and Philosophy of Modern Physics*, 28 (1997), 73–129, and references therein. See also Schweber, *QED and the Men Who Made It*, xxii and 5 (note 1). For the conceptual lineage of particle-antiparticle annihilation, see Joan Bromberg, 'The Concept of Particle Creation before and after Quantum Mechanics', *Historical Studies in the Physical Sciences*, 7 (1976), 161–91.

[3] Roger H. Stuewer, 'Mass-Energy and the Neutron in the Early Thirties', *Science in Context*, 6 (1993), 223–27.

[4] The history of mesotron decay has been examined, as a part of the history of meson theory, in Laurie M. Brown and Helmut Rechenberg, *The Origin of the Concept of Nuclear Forces* (Bristol, 1996), 177–97. Brown and Rechenberg characterize it as a 'typical theory–experiment confrontation' (p. 178). My reconstruction focuses on how observations of mesotron decay were the core of a theory–experiment alliance that created new directions of scientific enquiry.

[5] Experiments on mesotrons and their relation to theory are the subject of many participant recollections in L. M. Brown and L. Hoddeson, eds, *The Birth of Particle Physics; Based on a Fermilab Symposium* (Cambridge, 1983); David Cline and Gail Riedasch, eds, *50 Years of Weak Interactions; Wingspread Conference (1984)* (Madison, WI, 1984); B. Foster and P. H. Fowler, eds, *40 Years of Particle Physics; Proceedings of the International Conference to Celebrate the 40th Anniversary of the Discovery of the p- and V-particles, held at University of Bristol, 22–24 July 1987* (Bristol, UK, 1988). An account that is partly a recollection and partly an historical analysis is in Abraham Pais, *Inward Bound: of Matter and Forces in the Physical World* (Oxford and New York, 1986), 452–56.

identified, because of its mass, with the particle responsible for the force that keeps together protons and neutrons in the atomic nucleus according to a theory proposed by Yukawa. Despite some confusing difficulties, the identification persisted until 1947, when an experiment carried out by three young physicists in Rome demonstrated that mesotrons interacted with nuclei 10^{12} times more weakly than expected according to Yukawa's theory. As a solution to this paradox, it was suggested that two kinds of mesons existed. One kind, the Yukawa meson, would interact as strongly as the theory predicted. The other kind would be the weakly interacting particle hitherto detected in cosmic rays, and it would be the decay product of the first. The two-meson hypothesis was immediately confirmed by the discovery of the π-meson, which took up the role of the nuclear force carrier. The old mesotron, now called the μ-meson, was instead a particle that no theory had predicted or could explain. The sorting out of mesons was eventually formalized in terms of two distinct nuclear forces, the strong and the weak. The π-meson, or *pion*, and the μ-meson, or *muon*, were then re-classified as belonging to two different families of particles, distinguished by their different sensitivity to fundamental forces. In retrospect, the identification of the muon with the Yukawa particle appeared as a prolonged delusion, which Oppenheimer famously called a 'ten-year joke'.[6]

The salience of this famous mistake derives from its relevance to the present understanding of particles and forces and from the moral that it conveys: it exemplifies science's capability to rectify itself on the strength of experimental discoveries. Historians have also highlighted the pion-muon imbroglio as a development central to the disciplinary partition and to the periodization of modern physics. It was, in fact, a major occurrence in the confluence of nuclear physics, cosmic-ray studies, and quantum field theory that gave rise to the sub-field of particle physics. Furthermore, since it led to the recognized existence of more than one meson, it has been regarded as the opening act of the 'particle population explosion' that characterized the growth of the new field. Finally, it occupied a strategic position, at the threshold between the crafts-like and the industrial-like phases of twentieth-century physical research.[7] Nevertheless, as long as the episode is regarded only as a formidable error, its relations to these historical nodes appear as a notable but inconsequent series of coincidences. My discussion aims at outlining connections between various aspects of mesotron history, thus contributing to an appreciation of the constructive role that mesotron research played in forging basic conceptual and practical tools of high-energy physics.

[6] J. R. Oppenheimer, 'Thirty Years of Mesons', *Physics Today*, 19 (1966), 58.

[7] An overview of the early history of particle physics—characterized as the 'turbulent confluence' of nuclear physics, cosmic-ray studies, and quantum field theory (p. 4)—is Laurie M. Brown and Lillian Hoddeson, 'The birth of elementary particle physics: 1930–1950' in *The Birth of Particle Physics* (note 5), 3–36. Laurie M. Brown, Max Dresden, and Lillian Hoddeson, 'Pions to quarks: particle physics in the 1950s', in *Pions to Quarks; Particle Physics in the 1950s; Based on a Fermilab Symposium*, edited by L. Brown, M. Dresden, and L. Hoddeson (Cambridge, 1989), 3–39 is an introduction to the following period of particle physics history. Early meson physics, especially from the point of view of theory, is discussed in Brown and Rechenberg, *The Origin of the Concept of Nuclear Forces* (note 4). The mesotron–muon episode is touched upon, with different emphases and for different purposes, in several discussions concerning aspects of the early history of particle physics, for example, in Peter L. Galison, *How Experiments End* (Chicago, 1987), 124–6; Peter L. Galison, *Image and Logic: a Material Culture of Microphysics* (Chicago, 1997), 202–10; Allan Franklin, *Are There Really Neutrinos? An Evidential History* (Cambridge, MA, 2001), 100–4. A fairly detailed summary in a larger historical perspective is provided in Helge Kragh, *Quantum Generations; A History of Physics in the Twentieth Century* (Princeton, NJ, 1999), 194–205.

In the received story, prediction and observation of mesotron decay make a remarkable but accidental appearance, which can be summarized in the words of one of the principal historical actors:

> To account for the β decay, Yukawa had postulated that these particles were unstable, with a mean life of the order of microseconds, each decay process giving rise to an electron and a neutrino.
>
> It was natural to identify the Yukawa particle with the cosmic-ray mesotron. This identification, of course, was wrong, and so was the lifetime assigned by Yukawa to his particle. But two wrong assumptions led to a conclusion that, as you know, turned out to be correct, namely, that cosmic-ray mesotrons were unstable with a mean life on the order of microseconds.[8]

But the discovery of mesotron instability was not due to the uncanny cancellation of two unrelated mistakes. Given the persistent inability to decide by measurement whether mesotrons came in various masses or in a single one, it was the meshing of cosmic-ray data with Yukawa's β-decay hypothesis that substantiated the identification. In turn, the stability of the decay hypothesis against data propagated the inaccurate conviction that Yukawa had predicted a 'mean life of the order of microseconds'. What sustained the identification—and thus produced new lines of enquiry, new experimental methods to answer them, and new answers from the experimental methods—was not quantitative agreement between theory and observations but the promise of a coherent, unified explanation of cosmic-ray and nuclear phenomena. Mesotron experimenters profitably adopted and maintained the identification as a working hypothesis despite its quantitative inadequacy. The beginning of the accelerator programme in 1946 was accompanied by a shift in the perception of functions and priorities of experimental and theoretical work. This change entailed a re-evaluation of the relation between meson observations and meson theory, and led to the rejection of the existing formal structure of meson theory. The π-meson, on the other hand, was identified as a new particle thanks to its distinctive 'μ-decay', that is, its decay into another meson rather than into a β-particle (electron). Therefore, its 'recognition' as the Yukawa meson could not be simply the replacement of a wrong particle with the right one within a fixed theory, but was the end result of a process that included a radical refashioning of the theory itself.

2. The discovery of heavy electrons, and their disappearance

After the discoveries of positron and neutron in 1932, physicists began classifying particles into two groups according to their masses. The class of 'light particles' included photons, electrons, positrons, and the unobserved but commonly accepted neutrinos. Protons and neutrons, whose masses were very close to one another and approximately 2000 times the electron mass, constituted the class of 'heavy particles'. In 1937, Carl D. Anderson and Seth Neddermeyer of Caltech, and Jabez C. Street and Edward C. Stevenson of Harvard produced conclusive evidence that the most

[8] Bruno B. Rossi, 'The decay of 'mesotrons' (1939–1943): experimental particle physics in the age of innocence', in *The Birth of Particle Physics* (note 5), 183–205 (p. 185).

penetrating component of cosmic radiation consisted of a new class of particles.[9] Their findings settled a vexing question about the range of validity of the new quantum-relativistic theory of interactions between charged particles and the electromagnetic field, quantum electrodynamics (QED).[10] The cosmic-ray experimenters showed that the measured energy losses of cosmic rays in matter agreed thoroughly with values calculated from QED if, everything else remaining unchanged, the masses of the penetrating particles were assumed to be higher than the mass of electrons and lower than that of protons. Anderson and Neddermeyer carefully argued that, if charge and mass were to be regarded as the only parameters characterizing the electron in quantum theory, then the 'better working hypothesis' was:

> 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[.]

They called the penetrating particles 'heavy electrons', and added:

> The experimental fact that penetrating particles occur both with positive and negative charges suggests that they might be created in pairs by photons, and that they might be represented as higher mass states of ordinary electrons.[11]

There was no reason to think that the heavy electrons should have a unique mass value. In fact, measuring precisely the masses of cosmic-ray particles was not an easy task. Street and Stevenson did provide a mass estimate of 'approximately 130 times the rest mass of the electron [with] a probable error of some 25 percent'. A Japanese team also observed the intermediate particles and produced a mass estimation, which corresponded to approximately 200–270 times the electron mass.[12] Shortly after the discovery, Neddermeyer published a letter in which he pointed out that

> [s]ome of the range–curvature relations indicate that the mass [of the new particles] is of the order of a hundred times the mass of the electron, but there are also reasons for believing that the mass might not be unique, and that many masses, ranging from a few times the electron mass up to very large values, may exist.[13]

According to Neddermeyer, if the rest energy of an electron was associated with its 'fundamental' frequency through the de Broglie relation, $\nu = m_e c^2/h$, the electron could possess other 'modes', each of a different frequency and thus associated with a different mass. Neddermeyer's hypothesis did not leave a lasting impression.

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[9] Seth H. Neddermeyer and Carl D. Anderson, 'Note on the Nature of Cosmic Ray Particles', *Physical Review*, 51 (1937), 884–6; J. C. Street and E. C. Stevenson, 'New Evidence for the Existence of a Particle of Mass Intermediate Between the Proton and the Electron', *Physical Review*, 52 (1937), 1003–4.

[10] The history of the intermediate particles discovery is analysed in Peter Galison, 'The Discovery of the Muon and the Failed Revolution against Quantum Electrodynamics', *Centaurus*, 26 (1983), 262–316. Also, in Galison, *How Experiments End*, 75–133 (note 7).

[11] Neddermeyer and Anderson, 'Note on the Nature of Cosmic Ray Particles', 886 (note 9).

[12] Y. Nishina, M. Takeuchi, and T. Ichimiya, 'On the Nature of Cosmic Ray Particles', *Physical Review*, 52 (1937), 1198–9.

[13] Seth H. Neddermeyer, 'The Penetrating Cosmic-Ray Particles', *Physical Review*, 53 (1938), 102.

Nevertheless, since the uncertainty on mass measurements was to persist for more than a decade, the possibility that the new particles had an assortment of masses remained, accompanied by a subliminal ambiguity about whether particles with different masses were necessarily distinct particles or different states of the same particle.

Admitting the existence of new particles demanded a justification of why the particles had not been observed before. As Neddermeyer and Anderson put it,

> If the penetrating particles are to be distinguished from free electrons by a greater mass, and since no evidence for their existence in ordinary matter obtains, it seems likely that there must exist some very effective process for removing them.[14]

A possible removal mechanism was soon suggested by Homi J. Bhabha, an Indian theorist who was then working in England. In a study of the consequences of Anderson and Neddermeyer's finding within the framework of QED, Bhabha asserted:

> Indeed, in the experimental evidence itself there are definite hints that one new particle alone may not suffice to explain all the facts. We must therefore be prepared for the eventuality that a later and more complete theory may allow particles to exist whose rest masses may take on one of an infinite number of possible values of which only a few may turn out to be stable. With this idea is connected the possibility of a particle changing its rest mass, the difference in the energy being radiated or communicated to some other particle in the immediate neighbourhood. This change in the rest mass may be spontaneous or caused by an external agency.[15]

According to Bhabha's multiple-mass hypothesis, the heavy electrons would transform into ordinary electrons by making transitions from higher to lower mass states. The idea stemmed from the same analogy with radiation theory employed by Fermi in his theory of β-decay: a particle switching to a lower mass was akin to an atom passing from an excited state to a state of lower energy.[16] The simile relied on the possibility of representing the mass change as a state transition without assuming an internal structure for the particle. It also implied that the transition could be either spontaneous or induced by external conditions, but provided no direction as to the probability for a spontaneous transition to occur. Bhabha reckoned that the

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[14] Neddermeyer and Anderson, 'Note on the Nature of Cosmic Ray Particles', 886 (note 9).

[15] H. J. Bhabha, 'On the Penetrating Component of Cosmic Radiation', *Proc. Roy. Soc.*, A164 (1938), 290. Bhabha was concerned, in particular, with the energy losses of cosmic ray particles in lead and aluminium measured by P. M. S. Blackett and J. G. Wilson at Birkbeck's College in London. These measurements were in disagreement with the QED formulae for electrons above a critical energy, which seemed to decrease with increasing atomic number of the medium (over 10^{10} MeV in air, around 2×10^3 MeV in aluminium, and between 2 and 4×10^2 MeV in lead). At this stage, Bhabha's analysis reflected Blackett's re-interpretation of his own data in the light of the heavy electrons hypothesis. Blackett had moved from regarding the measurements as a demonstration that QED broke down at high energies to interpreting them as evidence that heavy electrons, if they existed, had to turn into ordinary electrons when losing energy. On this point, see Martha Cecilia Bustamante, 'Blackett's Experimental Researches on the Energy of Cosmic Rays', *Archives Internationales d'Histoire des Sciences*, 47 (1997), 132–7.

[16] Enrico Fermi, 'Tentativo di una teoria dei raggi beta', *Nuovo Cimento*, 2 (1934), 1–19. Reprinted in *Enrico Fermi: Collected Papers (Note e Memorie)*, edited by E. Amaldi et al., 2 vols (Chicago and Rome, 1962), I, 559–4.

probability would most likely be either too high or too low to affect the behaviour of cosmic particles during their fast passage through the atmosphere. Moreover, he was seeking an explanation for the apparent behaviour of the penetrating rays in different materials. Thus, he discounted spontaneous transitions as irrelevant to understanding cosmic rays, and suggested instead that a heavy electron might undergo a change in its rest mass when accelerated by the electric field of a nucleus, radiating the residual energy as a photon.

3. Yukawa's theory

In two months, Bhabha had moved to a different understanding of the new particles, thereby changing his mind about spontaneous transitions. He and other theorists began to look at the particles as a promising link between cosmic rays and nuclear physics after a note by Oppenheimer and R. Serber directed them to the work of an unknown Japanese physicist, Hideki Yukawa. Yukawa had published his theory two years earlier in a Japanese journal and had sent copies of his article, which was written in English, to several prominent Western physicists. His ideas, however, remained unnoticed until Oppenheimer and Serber made a censorious mention of them in one of the first theoretical analyses of the heavy electrons just discovered in cosmic radiation.[17]

Yukawa set forth to find a general theory of the interaction of elementary particles starting from the three partial theories then available: QED, Heisenberg's *Platzwechsel* theory of the nucleus, and Fermi's theory of β-radioactivity.[18] In Heisenberg's theory, the nucleus was a quantum mechanical system composed of protons and neutrons, and the attractive force that kept a proton–neutron system together was modelled upon the chemical bond of the H_2^+ molecular ion. According to Heisenberg's analogy, a 'change of place' (*Platzwechsel*) of a negative charge occurred between two heavy particles, one of which could then be imagined as transforming from the neutron state into the proton state, and the other from the proton state into the neutron state, with a given frequency. In Fermi's theory, a heavy particle changed from the neutron state into the proton state by emitting an electron and a neutrino. Although Fermi was concerned only with finding a mechanism for β-decay, his four-particle interaction was adopted by Heisenberg and others as a possible mechanism for proton–neutron attraction and adapted to the *Platzwechsel* idea: two heavy particles would interact by emitting and absorbing electron–neutrino pairs.[19] This theoretical plan, which was called

[17] J. R. Oppenheimer and R. Serber, 'Note on the Nature of Cosmic Ray Particles', *Physical Review*, 51 (1937), 1113.

[18] For historical analyses of Heisenberg's, Fermi's, and Yukawa's theories, see Joan Bromberg, 'The Impact of the Neutron: Bohr and Heisenberg', *Historical Studies in the Physical Sciences*, 3 (1971), 307–41; Olivier Darrigol, 'The quantum electrodynamical analogy in early nuclear theory or the roots of Yukawa's theory', *Revue d'Histoire des Sciences*, 41 (1988), 225–97; Helmut Rechenberg and Laurie M. Brown, 'Yukawa's Heavy Quantum and the Mesotron (1935–1937)', *Centaurus*, 33 (1990), 214–52; Cathryn Carson, 'The Peculiar Notion of Exchange Forces—II: From Nuclear Forces to QED, 1929–1950', *Studies in History and Philosophy of Modern Physics*, 27 (1996), 99–131; Brown and Rechenberg, *The Origin of the Concept of Nuclear Forces* (note 4).

[19] For a detailed examination of how forces came to be thought of as mediated by the 'exchange' of field quanta, and the roles of Heisenberg's, Fermi's, and Yukawa's theories in the evolution of this concept, see Carson, 'Exchange Forces, 1929–1950', 99–131 (note 18).

'Fermi-field theory', ascribed the two phenomena of proton–neutron binding and β-decay to the same field of force, whose quanta were the light particles. The plan encountered a serious difficulty in reproducing the essential quantitative features of the phenomena that it was designed to represent. In Fermi's theory, the probability of the interaction between heavy particles and light particles was expressed by a coupling constant, the Fermi constant '*g*', the value of which was to be fixed empirically, by the observed rates of β-decay.[20] The same coupling had to govern proton–neutron attraction in the Fermi-field theory. But to obtain empirically adequate binding energies using the Fermi constant, the range of the force would have to be two orders of magnitude lower than the empirical range of the nuclear force.

Puzzling over the coupling-range problem of the Fermi field led Yukawa to a breakthrough. He realized that a correct range and intensities could be obtained if the force was produced by a new fundamental field. The new field could mediate both proton–neutron binding and β-decay, but it could have different couplings with the heavy particles and with the light particles, thus allowing independent intensities for the two interactions. Articulating the analogy with electrodynamics, Yukawa replaced the Coulomb potential with a potential that decayed exponentially with the distance. The exponential parameter represented the range of the nuclear force. This form of the potential implied that the quanta of Yukawa's field, unlike the electromagnetic ones, had mass, and the mass was inversely proportional to the range. Accordingly, Yukawa's quantum would be none of the particles hitherto known to physics, for its mass would have to be about 200 times the mass of the electron.[21]

The electromagnetic–nuclear analogy and the mass–range relation are the best known features of Yukawa's theory. Yukawa, however, aimed at a general theory of the interactions of elementary particles at a time when no inventory of fundamental forces had been made. As Yukawa moved from a modification of the Fermi field, his model naturally encompassed proton–neutron binding as well as β-decay.[22] Yukawa's explanation of β-decay integrated the nuclear quantum with Fermi's process and Dirac's particle-antiparticle creation. It represented β-decay as a second-order process, in which a heavy quantum was emitted in the neutron–proton transition and then reabsorbed in the creation of an electron–antineutrino pair. The coupling of the field with the light particles was a free parameter that could be fixed to reproduce the experimental β-decay rates, independently of the energies involved in proton–neutron attraction. Yukawa

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[20] In a field theory, the 'coupling constant' is a parameter that quantifies the strength of the interaction because it determines the probabilities for all the processes allowed by the interaction term. In Fermi's formulation, *g* had the dimensions of volume × energy, and Fermi calculated that for β-decay the coupling constant had to assume typical values of the order of $g = 4 \times 10^{-50}$ cm^3 erg. (Fermi, 'Tentativo', 18 (note 16).)

[21] Hideki Yukawa, 'On the Interaction of Elementary Particles. I', *Proceedings of the Physico-Mathematical Society of Japan*, 17 (1935), 48–57. Reprinted in Hideki Yukawa, *Tabibito. (The Traveler)*, translated by L. Brown and R. Yoshida (Singapore, 1982), 209–18.

[22] Yukawa's general aim is revealed not only by the paper's title but also by its introduction, which closes with the following remark: 'Besides such an exchange force and the oridinary [*sic*] electric and magnetic forces there may be other forces between the elementary particles, but we disregard the latter for the moment'. (Yukawa, 'On the Interaction of Elementary Particles. I', 210 (note 21).)

reckoned that the light particles' coupling was as small as 10^{-8} times that of the heavy particles.[23]

It was natural for the theorists of the time to expand the Fermi model to include all aspects of nuclear physics. The difficulties met by this project highlighted a large disparity in the observed intensities of the processes involved, but dealing with the disparity within a single theoretical model remained a desirable goal. Yukawa's theory was expressly conceived for this purpose, and it succeeded by allowing for separate couplings of one field with different kinds of particles. Theoreticians readily recognized the disjoined coupling as an original strength of Yukawa's field, which permitted a consistent description of the exchange forces inside a nucleus and of the β-emissions from a nucleus. Bhabha, for example, was attracted to Yukawa's theory precisely by this feature:

> The theory of Yukawa therefore has in our opinion the following advantages over other theories. It separates the theory of nuclear forces from that of the β-decay in such a way that the former only depends on the interaction of the proton and neutron with the heavy electron [i.e. Yukawa's quantum], while the latter also depends on its interaction with the electron and neutrino, so that by making the former interaction strong and the latter weak it is possible to explain the large magnitude of nuclear forces and the weakness of the β-decay.[24]

In this sense, Yukawa initiated the process of dis-unification of strong and weak forces within a unified theory.

4. The nuclear connection and the prediction of decay

Physicists felt 'naturally tempted'[25] to identify the particles of intermediate mass found in cosmic radiation with the nuclear quanta postulated by Yukawa. The tempting identification, to which I shall refer as the nuclear connection, gained Yukawa's ideas the attention of the international physics community.[26] At the same time, it reconfigured the theoretical space in which the heavy electrons had begun to be framed.

Anderson asserted that the observation of heavy electrons 'was based on experimental measurements and procedures, with no guide from any theoretical

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[23] According to the current Standard Model, nuclear binding is due to the *strong nuclear force* and β-decay to the *weak nuclear force*, the two forces being irreducible to one another in the domain of presently attainable energies. For this reason, Fermi's theory is sometimes characterized as the first theory of weak interactions, and Yukawa's theory as the first theory of strong interactions. (See, for example, Pais, *Inward Bound*, 580 (note 5).) Of course, neither Fermi nor Yukawa meant such a distinction.

[24] H. J. Bhabha, 'On the theory of heavy electrons and nuclear forces', *Proceedings of the Royal Society of London*, A166 (1938), 501–27 (p. 503).

[25] Gian Carlo Wick, 'Range of Nuclear Forces in Yukawa's Theory', *Nature*, 142 (1938), 993–4 (p. 993).

[26] The significance of the discovery of heavy electrons for the acceptance of Yukawa's theory has been examined by Stephen G. Brush, as one of three case studies on the role of theoretical predictions in theory evaluation. Brush's analysis supports the philosophical claim that empirical confirmation of a prediction provides corroboration for the theory, not in the sense of increasing the probability that the theory is true, but in the sense of making it 'more reasonable to pursue that hypothesis than one that has not been corroborated'. (Stephen G. Brush, 'Prediction and Theory Evaluation: Subatomic Particles', *Rivista di Storia della Scienza, Serie II*, 1 (1993), 47–152 (p. 116).)

predictions'.[27] Available historical evidence backs Anderson's assertion, if it is understood to refer solely to predictions *about* the existence of new particles. The experimenters did not know of Yukawa's work until after the experimental evidence for new particles was established, and the experimental evidence relied on a symbiosis between instrumental observations and another set of theoretical predictions, namely, those of QED energy losses of fast charged particles in matter. Theory had no part in directing the experimenters to the final result, but it had an essential role in constituting the 'experimental measurements and procedures' out of which the result took shape.

Anderson also expressed the view that Yukawa's suggestion, had it been known, would have accelerated the discovery by lowering the experimenters' caution:

> I believe that a theoretical idea like Yukawa's would have appealed to the people carrying out the experiments and would have provided them with the belief that maybe, after all, there was some need for a particle as strange as a meson, especially if it could help explain something as interesting as the enigmatic nuclear forces.[28]

This passage illustrates the subtle power of retrospection. An examination of the sources confirms that nuclear theorists welcomed a particle helpful in deciphering the nuclear forces, and in turn were able to attract experimenters to a research programme directed by the link between cosmic radiation and nuclear theory. It does not confirm, however, that prior to the connection the experimenters were restrained by the thought that heavy electrons were strange and not needed. Not only did Anderson and Neddermeyer stress the possibility of multiple masses when presenting their conclusive evidence for heavy electrons, but they were unsure whether the intermediate particles indicated by their previous experiments should be regarded as the same entities or yet others.[29] Purposeless particles, it seems, became strange only after a purpose for them had been invented.

The effect of the nuclear connection was to open a second phase in the study of cosmic-ray intermediate particles. The core of Yukawa's theory was the relation between the range of nuclear forces and the mass of a field quantum. This relation between a fundamental mass and a fundamental length was the kind of idea that theorists of the 1930s found seductive. Oppenheimer and Serber, for example, judged the theory highly problematic[30] but embraced the mass–range relationship. Quantum-field theorists in Europe were even more receptive and were ready to take a more constructive stance than Oppenheimer's. They immediately developed and improved Yukawa's sketchy model, as did Yukawa and his collaborators in Japan. They focused on finding a fully relativistic interaction form and on investigating which field types were capable of reproducing all observable features of nuclear phenomena. In the course of this search, various combinations of charge and spin of the heavy quanta were tried. For example, quanta of spin one were proposed to account for the sign and directionality of proton–neutron attraction, and neutral quanta were postulated in order to reproduce the observed charge

[27] C. D. Anderson and H. L. Anderson, 'Unraveling the particle content of cosmic rays', in *The Birth of Particle Physics*, edited by Brown and Hoddeson, 131–54 (p. 149) (note 5).

[28] Anderson and Anderson, 'Unraveling the particle content of cosmic rays', 149 (note 27).

[29] Neddermeyer and Anderson, 'Note on the Nature of Cosmic Ray Particles', 886 (note 9).

[30] Yukawa's theory was affected by the same problem of divergent quantities as QED; the problem, however, was more serious because of the larger coupling between heavy particles and field.

independence of nuclear forces. In 1938, the Italian theoretician Gian Carlo Wick showed that the relation $\lambda = h/mc$ between the range λ and the mass m of a field quantum did not depend on any specific form of the interaction, but could be derived from two general principles: the conservation of energy and Heisenberg's uncertainty relation $\Delta E \times \Delta t \sim h$.[31] This trans-theoretical justification of Yukawa's mass–range relation became a recurrent argument in particle physics.

In an influential article published in the summer of 1938, Hans Euler and Werner Heisenberg articulated the implications of the nuclear connection for the study of cosmic radiation. The two authors noted that, although experiments were still unable to answer the question of whether all intermediate particles had the same mass or not, the data could be 'reconciled' with a single mass value of about 160 times the electron mass.

> If this assumption is not made, at present there is no viewpoint that could lead to statements about the behaviour of these particles. On the other hand, if the existence of one determined kind of particles with a single rest mass of about 160 electron masses is assumed, it is possible to associate these particles with a theory of nuclear forces that was proposed by Yukawa in the year 1935 and was elaborated by him and several others.[32]

The hypothesis of a single mass came to be regarded as the only *Ansatz* that could structure the study of the new particles precisely because it allowed a connection with the promising new theory of nuclear forces. The discoverers of heavy electrons resisted this theory-driven redefinition of their particles. In the second half of 1938, names for the new particles were proliferating, and one of them was 'yukon', clearly a signal that the nuclear connection was taking hold. Anderson and Neddermeyer, prodded by Robert A. Millikan, protested that it was premature to assign theoretically loaded names to particles about which too little was known:

> Although from the experiments so far performed, it is not possible to say definitely whether the new particles exist with a unique mass only, or whether they occur with a range of masses, it does appear quite certain that the mass, whether unique or not, is greater than that of an electron and less than that of a proton. One must consider then three types of particles all carrying electric charges of equal magnitude: electrons, the new particles and protons. We should like to suggest therefore the word 'mesotron' (intermediate particle) as a name for the new particles. It appears quite likely that the appropriateness of

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[31] Wick, 'Range of Nuclear Forces', 993–4 (note 25). Wick reasoned that exchanges of field quanta cannot be 'actual' emission and absorption processes because they violate the conservation of energy with the creation of an amount of energy at least equal to the rest energy of the quanta. They are what physicists call 'virtual' transitions, that is, processes that 'exist' in formal representation but are unobservable as a matter of principle. An energy imbalance of the order of ΔE can 'exist' only as long as it is not observable. That is possible, by virtue of the uncertainty principle, if the emitted quantum of rest energy $\Delta E = mc^2$ lasts no longer than the time $\Delta t \sim h/\Delta E$. This means that a virtual quantum of mass $m = \Delta E/c^2$ can travel no farther than the distance $\lambda = h/mc$ before being absorbed.

[32] H. Euler and W. Heisenberg, 'Theoretische Gesichtspunkte zur Deutung der kosmischen Strahlung', *Ergebnisse der exakten Naturwissenschaften*, XVII (1938), 1–69 (p. 24). ('*Wenn man diese Annahme nicht macht, so gibt es einstweilen noch keine theoretischen Gesichtspunkte, die zu Aussagen über das Verhalten dieser Teilchen führen könnten. Wenn man jedoch die Existenz einer bestimmten Teilchensorte von einer Ruhemasse von etwa 160 Elektronenmassen annimmt, so liegt es nahe, diese Teilchen in Verbindung zu bringen mit einer Theorie der Kernkräfte, die im Jahre 1935 von Yukawa vorgeschlagen und von ihm und verschiedenen anderen Forschern ausgearbeitet worden ist*'.)

this name will not be lost, whatever new facts concerning these particles may be learned in the future.[33]

The name 'mesotron' was intended as a reminder that the evidence available only revealed a possibly heterogeneous class of entities, the 'intermediates'. The other names were soon forgotten, but mesotron came to be used interchangeably with its variant, 'meson'. Despite the name's intention, the nuclear connection did become the prevalent working hypothesis for cosmic-ray experimentalists, and the body of theoretical developments that originated from Yukawa's ideas came to be known as 'meson theory'. Some physicists tried to pursue the original interpretation of intermediate particles as heavy electrons having a spectrum of masses[34], but voices as authoritative as Hans A. Bethe's heaped discredit on this view:

> Considerable discussion has been devoted to the question of naming the new particle. [...] It seems to us that the only name definitely to be avoided is 'heavy electron'. If there is any truth to the current theories then the new particle differs from an electron as much as any particle can: It has a different mass, a different spin, and different statistics [...] The use of the name 'heavy electron' is even dangerous because it leads easily to misconceptions.[35]

Thus, the first effect of the hypothetical identification with the heavy quantum was that of reducing the intermediates to a single one. Contrary to what has been occasionally maintained, Yukawa's particle was not received as a daring transgression of some tacit economy rule on the existence of fundamental entities.[36] With respect to the interpretation of cosmic radiation, it functioned as a means of particle non-proliferation. Yukawa's theory, which had played no role in the discovery of the heavy electrons (plural), was in this sense responsible for the discovery of the mesotron (singular).

The nuclear connection integrated nuclear structure, β-radioactivity, and cosmic-ray phenomena in a coherent explanatory scheme. Cosmic radiation was at that time the only source of high-energy particles on which any candidate theory of fundamental interactions could be tested, and from which empirical information

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[33] Carl D. Anderson and Seth H. Neddermeyer, 'Mesotron (Intermediate Particle) as a Name for the New Particles of Intermediate Mass', *Nature*, 142 (1938), 878.

[34] G. E. M. Jauncey, 'Possible Origin of the X Particle', *Physical Review*, 52 (1937), 1256. See also Paul Weisz, 'Zenith Angle Distribution of the Hard Component of Cosmic Rays and the Mass of the Mesotron', *Physical Review*, 55 (1939), 1266–7.

[35] H. A. Bethe, 'The Meson Theory of Nuclear Forces. I. General Theory', *Physical Review*, 57 (1940), 260–72 (p. 262). The perfunctory 'if' in Bethe's pronouncement reads as a premonition because after 1947, mesotrons did return to be seen as heavy electrons, although by then the particle world was becoming more complicated. In present terms, the mesotrons were muons, and muons are 'second generation leptons'. This means that they are almost like higher mass states of ordinary electrons. The presently accepted structure of the lepton 'family' recalls the old view of multi-mass electrons to a remarkable extent but not exactly. A muon does have the same spin as an electron; however, it does not simply decay into its lower mass state, the electron, shedding the excess energy in the form of a photon or a neutrino. It decays into an electron, a neutrino (muon neutrino), and an antineutrino (electron antineutrino). This three-body disintegration is believed to be necessary because muons (and muon neutrinos) carry—besides rest energy, electrical charge, and spin—a 'lepton number' different from the lepton number of electrons (and electron neutrinos), and in each elementary process the different lepton numbers must be separately conserved. (The electron antineutrino carries the lepton number of the electron but with an opposite sign.)

[36] See, for example, Yukawa, quoted in Brown and Rechenberg, *The Origin of the Concept of Nuclear Forces*, 111 (note 4). See also Takehiko Takabayasi, 'Some characteristic aspects of early elementary particle theory in Japan', 294–303 (p. 295), and Takabayasi's remarks in 'Second round-table discussion', 278, in Brown and Hoddeson, eds., *The Birth of Particle Physics*, 278–92 (note 5).

for the construction of fundamental theories could be extracted. Given the large uncertainties on the values of masses and the scarcity of data on interactions between mesotrons and nuclei, the most fruitful approach to probing the promised explanatory coherence was the link between β-decay and the behaviour of cosmic radiation.

An almost offhanded remark by the Swiss theorist Ernst C. G. Stückelberg led Bhabha to reconsider the spontaneous decay of the heavy electrons. Stückelberg was developing a very general field theory of particle interactions, which predicted the existence of several new field quanta. Among these was an intermediate quantum, a 'Bose electron', similar to Yukawa's. Upon learning about heavy electrons, Stückelberg published a letter associating the cosmic-ray intermediate particles with his own and Yukawa's prediction—the only mention of Yukawa's theory in the international physics literature independent of Oppenheimer and Serber's note—and pointed out that the particles would be unstable, their mass being greater than the sum of electron and neutrino masses.[37] In early 1938, Bhabha submitted a paper to the Royal Society of London in which he put Yukawa's theory in relativistically invariant form, and generalized it to proton–proton interactions by including a neutral heavy quantum in addition to the charged ones. Following Stückelberg's remark, Bhabha returned to the idea of spontaneous instability, this time within the terms of the nuclear connection and therefore under the hypothesis of a single mass for the heavy electrons.[38] Yukawa's description of β-decay implied that a free nuclear quantum could transform into an electron and a neutrino without violating the conservation of energy. Bhabha recalled that the energy losses measured by Blackett and Wilson indicated that the penetrating particles behaved like heavy electrons at high energies and like ordinary electrons at low energies. He interpreted these data as showing that heavy electrons had a large probability of changing into ordinary electrons when their energy decreased below about 200 MeV. But, while he had previously envisioned mass-state transitions with photon emission in a nuclear field, he now turned to nuclear reactions.

He reasoned that, for charge conservation, there were

> essentially only two ways in which a single heavy electron may disappear. If, for example, it has a negative charge, it may collide with a proton and communicate its charge to it, the proton changing into a neutron, or it may turn into an ordinary electron by changing its rest mass.[39]

The first of these mechanisms was a process of nuclear capture. For the second mechanism, Bhabha now upheld the possibility that the change of mass might be a spontaneous transition due to the coupling between heavy quanta and light particles, in other terms, a β-decay. He thus extended the terminology and conceptual apparatus of radioactivity to elementary particles in cosmic radiation. As for radioactive disintegrations, the statistical decay rate in a sample of identical particles would be independent of the particles' circumstances and would only depend upon a time parameter distinctive of the particles' species. This quantity, the 'mean lifetime', was then an experimentally observable characteristic of the intermediate particle.

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[37] E. C. G. Stückelberg, 'On the Existence of Heavy Electrons', *Physical Review*, 52 (1937), 41–2.

[38] Bhabha, 'Heavy electrons and nuclear forces', 502 (note 24).

[39] Bhabha, 'Heavy electrons and nuclear forces', 501–2 (note 24). See note 15 on the parallel between Bhabha's and Blackett's views.

The identification with β-radioactivity offered an indication about the value of the mean lifetime. Furthermore, transferring the lifetime concept from radioactive samples to cosmic rays led Bhabha to a consideration that was crucial to the application of the decay hypothesis to cosmic-ray data:

> A positive U-particle [i.e. Yukawa's nuclear quantum] at rest may disintegrate spontaneously into a positive electron and a neutrino. This disintegration being spontaneous, the U-particle may be described as a 'clock', and hence it follows merely from considerations of relativity that the time of disintegration is longer when the particle is in motion. We believe that this may have to do with the fact observed by Blackett and others that below 2×10^8 e.v. most cosmic ray particles are electrons, above this energy heavy electrons. In a previous paper we have shown that the experimental evidence requires that heavy electrons can apparently turn into ordinary electrons. Our U-particles are then to be identified with the heavy electrons, and it follows that most of the heavy electrons have been created either in the Earth's atmosphere or not very far from it.[40]

Bhabha's suggestion prompted two developments. In Japan, Yukawa and his collaborators calculated the first estimate of the mean time of decay of a free nuclear quantum at rest and found $\tau = 0.5 \times 10^{-6}$ s.[41] In Germany, Euler and Heisenberg, developing their single-mass *Ansatz*, applied Bhabha's decay hypothesis to the analysis of cosmic-ray evidence. They were able to show that the assumption of 'a natural β-radioactivity'[42] could account quantitatively for an anomaly that had been detected in the absorption of heavy electrons. According to a longstanding rule called the 'mass absorption law', a fast ionizing particle in matter would lose energy in proportion to the amount of mass that it traversed, independently of the density of the medium. Heavy electrons, however, seemed to be absorbed faster while traversing a large layer of atmosphere than crossing the same quantity of mass in a slab of solid material. Various forms of this effect, which came to be known as 'anomalous absorption of air' or 'mass absorption anomaly', had been recorded since 1934, but had hitherto remained scattered deviances. Spontaneous decay could explain the anomaly, because the particles would disappear in larger numbers during their flight through the atmosphere than in the short time they took to cross a thin layer of dense matter.[43] The principle of relativistic time dilation allowed Euler and Heisenberg to quantify this explanation. Assuming a unique mass value of $160m_e$, from anomalous

[40] H. J. Bhabha, 'Nuclear Forces, Heavy Electrons and the b-decay', *Nature*, 141 (1938), 118.

[41] At least, Euler and Heisenberg, attributed this value to Yukawa and, in a letter to Heisenberg Yukawa, referred to it as 'our calculation'. (Yukawa to Heisenberg, 15 July 1938, quoted in Brown and Rechenberg, *The Origin of the Concept of Nuclear Forces*, 182 (note 4).) The first published value of Yukawa and his collaborators was $\tau = 1.3 \times 10^{-7}$ s. (Hideki Yukawa and Shoichi Sakata, 'Mass and Mean Life-Time of the Meson', *Nature*, 143 (1939), 761 and references therein.)

[42] Euler and Heisenberg, 'Theoretische Gesichtspunkte', 26 (note 32).

[43] The decay explanation for anomalous absorption measurements was first put forward, in a qualitative manner, by H. Kulenkampff. (H. Kulenkampff, 'Bemerkungen über die durchdringende Komponente der Ultrastrahlung. (Zum Teil nach Messungen von H. Kappler und H. Martin.)', *Verhandlungen der Deutschen Physikalischen Gesellschaft*, 2 (1938), 92. Reviews of existing absorption observations, together with their re-interpretation in terms of the decay hypothesis, are given in P. M. S. Blackett, 'High Altitude Cosmic Radiation', *Nature*, 142 (1938), 692–3; P. M. S. Blackett, 'Further Evidence for the Radioactive Decay of the Mesotrons', *Nature*, 142 (1938), 992; Bruno Rossi, 'Further Evidence for the Radioactive Decay of Mesotrons', *Nature*, 142 (1938), 993; Bruno Rossi, 'The Disintegration of Mesotrons', *Reviews of Modern Physics*, 11 (1939), 296–303.

absorption data they derived a decay mean life $\tau = 2.7 \times 10^{-6}$ s. Their assessment of the degree of theory-observation concordance displayed by this value was to have a widespread and lasting impact among cosmic-ray experimentalists, as follows:

> This value is about 5 times larger than that calculated form the Yukawa theory. In consideration of the uncertainty of many details in the Yukawa theory, especially of the mass of the heavy electrons, this agreement is quite satisfactory.[44]

Uncertainties are the lubricant of scientific life. Scientists would be constantly stuck if they insisted on exact agreement between theoretical numbers and measurements. Luckily, they take a more pragmatic approach and settle for what Kuhn called 'reasonable agreement' within current uncertainties.[45] There is no predefined agreement gauge. The evaluation of reasonable agreement is entirely contingent upon circumstances that are experienced as relevant by the researchers. For the decay hypothesis, which was received as a much needed resource to clarify tangled phenomena, the agreement appeared not only reasonable but quite satisfactory. The following note by Blackett to Heisenberg is representative of cosmic-ray specialists' response ('barytrons' was one of the names invented for the heavy electrons):

> I am entirely delighted with your beautiful explanation of the mass absorption anomaly as due to the decay of the barytrons (what are they to be called?). It is so simple and obvious, that I can't understand why we didn't all see it as soon as the decay theory was put forward! I gave special emphasis to your explanation (in a very simplified form) in my address to the British Association.[46]

In his address to the British Association, Blackett provided his own preliminary evaluation of mean lifetime from existing data, $\tau = 2 \times 10^{-6}$ s, and commented:

> Though this value is about four times that predicted by Yukawa, the agreement must be considered as most satisfactory in view of the early stages of the theory and of the crudeness of the deductions from experiments.
> There seems, therefore, to exist definite experimental evidence for the spontaneous decay of the new particle. The accurate determination of this time of decay and of the mass of the particle is now one of the outstanding problems of cosmic ray research.[47]

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[44] Euler and Heisenberg, 'Theoretische Gesichtspunkte', 42 (note 32). ('*Dieser Wert ist etwa 5mal so groß wie der aus der Yukawaschen Theorie berechnete. In Anbetracht der Unsicherheit mancher Einzelheiten in der Yukawaschen Theorie, insbesondere der Masse des schweren Elektrons, ist diese Übereinstimmung durchaus befriedigend*'.)

[45] Thomas S. Kuhn, 'The Function of Measurement in Modern Physical Science', in *The Essential Tension* (Chicago, 1977), 179–224 (p. 184).

[46] Blackett to Heisenberg, 10 September 1938, quoted in Brown and Rechenberg, *The Origin of the Concept of Nuclear Forces*, 183 (note 4). Blackett's enthusiasm might have been compounded by the fact that Euler and Heisenberg were able to explain his observations on the different energy spectra of heavy electrons and ordinary electrons (see note 15). They clarified that the disappearance of heavy electrons at low energies was a combined effect of decays and of increased energy losses by ionization. (Euler and Heisenberg, 'Theoretische Gesichtspunkte', 40–1 (note 32).)

[47] Blackett, 'High Altitude Cosmic Radiation', 693 (note 43).

Supported by an array of pre-observations, the decay hypothesis made its debut as an empirically adequate prediction, and was thereby readily translated into an effective plan of research. This prediction—the 'wrong' side of Yukawa's theory—transformed the nuclear connection from wishful thinking into a usable working model.

5. The indirect observation of decay and the question of the mass

The experimental programme outlined by Blackett got under way. Several groups of cosmic-ray researchers in England, France, Italy, and the United States took up the challenge of observing mesotron decays, for this undertaking was potentially rewarding and suited to their instruments and methods. They were builders and operators of 'cosmic-ray telescopes', devices designed to electronically count particles passing through suitable arrangements of Geiger–Müller counters and material absorbers, to determine the statistical rates of particles of different penetration power. Setups of this kind could be easily adapted to measure mean lifetimes. Even though achieving consistency among measurements proved problematic, the results from the newly designed experiments were all consistent with the decay explanation of anomalous absorption of air given by Euler and Heisenberg. Experiments of this kind were classified as 'indirect' tests of mesotron decay.[48] By the middle of 1940, the experimental value of the lifetime, as estimated by different groups from anomalous absorption observations with cosmic-ray telescopes, pointed unambiguously to the range of about 1 to 5×10^{-6} s.

Unfortunately, improved calculations of decay probability from meson theory reduced the theoretical uncertainties but did not improve the theory–observation agreement. Immediately after the initial appraisal of satisfactory agreement, Yukawa had to reply to a congratulatory note by Heisenberg by pointing out that there had been a factor two mistake in the first calculation of the mean lifetime.[49] The correct result, $\tau = 0.25 \times 10^{-6}$ s, was ten times lower than Euler and Heisenberg's experimental value. Moreover, Yukawa and his collaborators had taken the mesotron mass to be $100m_e$, whereas Heisenberg and Euler used $160m_e$, and Blackett $200m_e$. Both theoretical and experimental estimates depended on the mass, the uncertainty of which remained very large. Since the theoretical values decreased with increasing masses more rapidly than the experimental values, the agreement would have been even worse if calculators and measurers had used consistent masses.

Lothar W. Nordheim, a German theorist at Duke University, criticized Yukawa's calculation because the value of the Fermi constant used in it derived from the β-decay of heavy elements, whereas a value from light elements was more appropriate. With this correction and a mass of $200m_e$, Nordheim obtained $\tau = 1.6 \times 10^{-9}$ s, 'a value about 10^{-3} times too small'.[50] Christian Møller, Léon Rosenfeld, and S. Rozental of the Copenhagen Institute of Theoretical Physics suggested that the difficulty shown by Nordheim could be solved if it was admitted that two independent meson fields were at play, one with spin one (vector field) and the

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[48] The terminology of 'indirect' and 'direct' observations was widely used. See, for example, Rossi, 'The Disintegration of Mesotrons', 296 (note 43). For an historical analysis of experiments on the decay of mesotrons and their degrees of directness, see D. Monaldi, forthcoming.

[49] Yukawa to Heisenberg, 15 July 1938, quoted in Brown and Rechenberg, *The Origin of the Concept of Nuclear Forces*, 182 (note 4).

[50] L. W. Nordheim, 'Lifetime of the Yukawa Particle', *Physical Review*, 55 (1939), 506.

other with spin zero and negative parity (pseudoscalar field).[51] Møller and Rosenfeld had already proposed their two-field mixture in order to avoid the assumption that the only nuclear quantum was a still-unobserved neutral meson. The hypothesis of a purely neutral field would have solved other computational difficulties of meson theory, yet Møller and Rosenfeld disliked it, not because it postulated an unobserved particle but because it amounted to giving up the 'remarkable connexions' between nuclear force, β-decay, and cosmic rays.[52] Bethe, working at Cornell University, undertook a comprehensive revision of meson theory, and enlisted the collaboration of Nordheim for the part concerned with meson decay. Bethe and Nordheim also expressed the view that the unified explanation offered by Yukawa's theory was very appealing, but they had to recognize that it was impossible to obtain a quantitative agreement between mesotron decay and β-decay. They reasoned that, since it appeared necessary to revert to the direct Fermi interaction for β-decay, the unity was broken, and thus one might as well adopt the purely neutral version of the meson field. Their unconvincing conclusion, however, was to leave the question open as to whether it was still premature to expect quantitative predictions from Yukawa's theory.[53]

Indeed, the lifetime discrepancy was only one of the difficulties faced by meson theory, and models involving meson pairs acquired a certain popularity among theorists because they seemed to afford—if the right combination of spins and masses was found—a simultaneous solution to more than one problem. Of particular interest for the history of mesotron decay was a modification of Møller and Rosenfeld's mixture developed by Julian Schwinger in 1942, in which the two mesons were assumed to have different masses. The heavier meson would thus be 'highly unstable' against decay into the lighter meson and a γ-ray, and would not have been observed.[54]

Besides the measured lifetime, the penetration power that was characteristic of the observed intermediate particles presented another problem for the nuclear connection. The little propensity manifested by mesotrons to be absorbed and scattered by nuclear matter was incompatible with the high probability of nuclear interactions predicted by meson theory. This difficulty, however, was perceived to be caused by the method used to calculate interaction probabilities, the perturbation method, which had been developed for QED. Alternative methods, more suitable to a field theory governed by strong coupling, were explored for meson interactions, and appeared promising in accounting for the low scattering probabilities.[55]

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[51] C. Møller, L. Rosenfeld, and S. Rozental, 'Connexion between the Life-time of the Meson and the Beta-Decay of Light Elements', *Nature*, 144 (1939), 629.

[52] C. Møller and L. Rosenfeld, 'The Electric Quadrupole Moment of the Deuteron and the Field Theory of Nuclear Forces', *Nature*, 144 (1939), 476. One of the computational troubles of meson theory was that the nuclear potential was divergent at small distances. Møller and Rosenfeld's field mixture achieved the cancellation of this infinite term, but only at the lowest order of the perturbation expansion.

[53] H. A. Bethe and L. W. Nordheim, 'On the Theory of Meson Decay', *Physical Review*, 57 (1940), 998–1006 (p. 1004).

[54] Julian Schwinger, 'On a Field Theory of Nuclear Forces', *Physical Review*, 61 (1942), 387.

[55] The perturbation method presupposed that the interaction probability was dominated by the most elementary interaction mechanisms (the lowest-order terms of the perturbation expansion series). This was evidently inappropriate in the case of strong coupling, in which more complex processes (higher-order terms) became increasingly significant. For an extended and detailed examination of meson theory difficulties and the rich variety of approaches to solve them, including strong and intermediate coupling theories, and meson pair theories, see Brown and Rechenberg, *The Origin of the Concept of Nuclear Forces*, 253–91 (note 4).

Field theories involving meson pairs were also formulated in Japan. From 1941 to 1945, a group of Japanese physicists created an informal 'Meson Club' and examined systematically the difficulties of Yukawa's theory. The idea that the observed mesotrons and Yukawa's quanta were different particles was considered as a possible solution. In particular, Shoichi Sakata and Takesi Inoue developed a model in which the Yukawa particle decayed into the cosmic-ray mesotron and a neutrino, with a lifetime of the order of 10^{-8} s. The cosmic-ray mesotron was assumed to be a fermion with spin 1/2 and to decay into an electron, a neutrino, and a neutral particle 'equivalent to the neutrino', with a lifetime corresponding to that measured by experimenters. The intensity of its nuclear interactions was thus determined by the rates of its free decay and of nuclear β-decay, and could therefore also account for the low interaction rates observed in cosmic rays. But Sakata and Inoue's two-meson hypothesis remained internationally unknown because of the war.[56]

The promising agreement had turned into an alarming gap. Interestingly, the alarm rang only for theoreticians. Not one of the various experimental reports of mesotron lifetime measurements of this period shows that the experimenters took note of the theory–observation discrepancy. The only theoretical lifetimes mentioned in experimental papers were either the incorrect $\tau = 0.5 \times 10^{-6}$ s or indications such as 'of the order of the microsecond', which were as fictional as they were vague. After the programmatic phase, experimenters quickly lost interest in comparing their measured lifetimes with actual theoretical calculations, and turned instead to criteria of internal consistency of individual experiments and on coordination of different experiments to validate observational results. An elementary particle's decay was a novel phenomenon, and measuring a time parameter of the order of a millionth of a second was an exciting experimental challenge that required new techniques and apparatus. By means of innovative applications of the method of coincidence counting, cosmic-ray experimenters were able to establish the existence of mesotron decays and to measure the mean life in ways that they perceived to be purely experimental. The results from different experiments were at the same time consistent enough to breed confidence that mesotron decay was an experimental fact, and inconsistent enough to justify a continuing interest in improving measurements.

Persistent difficulties in reconciling the results of different experiments led investigators to scrutinize the assumptions needed for data interpretation. Researchers became aware that, while other assumptions could be disposed of by means of experimental re-design, theoretical re-analysis, or auxiliary experimental checks, assuming a mass value was indispensable to obtaining a mean lifetime value from the decay interpretation of anomalous absorption. They became convinced that the quantity that could be 'directly' measured from anomalous absorption observations was not the lifetime but the ratio between lifetime and rest energy, τ/mc^2. When they realized that even consistent mass values failed to reconcile results, some questioned whether it was legitimate to try and squeeze one lifetime from the data, given that it was still uncertain whether there was one mass. Mass measurement could do very

[56] Brown and Rechenberg, *The Origin of the Concept of Nuclear Forces*, 277–80 (note 4); R. E. Marshak, 'Particle physics in rapid transition', in *The Birth of Particle Physics*, 376–401 (p. 378) (note 5).

little to reassure that mesotrons were one homogenous kind of particle. In a 1939 review of the evidence on the new particles, Anderson and Neddermeyer warned their readers that,

> it has become increasingly likely that a complete interpretation of the experimental data is not to be found in the single assumption of unstable particles with unit charge and unique mass of the order of 200 electron masses.[57]

In 1941, John A. Wheeler and Rudolf Ladenburg compiled all the experimental mass estimates that had been published until then. They found a spread between 100 and $250m_e$, with sporadic lower and higher entries, and an average at $180m_e$. They concluded that available evidence did not 'allow a decision of the very important question whether the mass of the meson is unique'.[58] Initially, the application of the decay hypothesis to existing absorption data had dealt a hard blow to the multiple-mass hypothesis, for it appeared to confirm the nuclear connection. Now, the proliferation of τ/mc^2 values had the effect of reviving the idea of many masses. M. A. Pomerantz of the Franklin Institute in Swarthmore, Pennsylvania, concluding a detailed experimental study of mesotron decay, noted that the 'uniqueness of the rest mass of the meson has not yet been established, and the existence of a variable mass would obviously play an important role in these considerations'.[59] Paul Weisz, a German theorist also working at the Franklin Institute, performed a comparative analysis of anomalous absorption data and found that the measured lifetimes could be reconciled with one another if a spectrum of masses was assumed. The relation of such mass spectrum with Yukawa's theory was left unclear. Weisz clinched his argument with the puzzling remark that, according to his analysis, the experimental lifetime might actually be close to '$\tau_0 = 5 \times 10^{-7}$ s, which is Yukawa's original estimate of the mesotron lifetime'.[60]

Neither the lifetime discrepancy nor the resurfacing of the multiple-mass hypothesis led to a reconsideration of the nuclear connection. Mesotron experimenters remained faithful to Yukawa's theory in a way that is difficult to explain from our present point of view. Today, to receive any degree of empirical support, a theory would need to predict not only the exact lifetime of the particle in question but also its decay modes and the corresponding branching ratios down to the finest details. Yukawa's theory failed at this quantitative kind of explanation. For us, the spontaneous instability of a particle does not call for an explanation; but then, we live in a post-mesotron time. The late 1930s–early 1940s was a transitional period, when the prediction that a particle would spontaneously disintegrate was no longer surprising but not yet commonplace. On the one hand, the evidence of mesotron decays was being consolidated through experimental practices that did not need a quantitative theoretical description of mesotron decay for the interpretation of instrumental outputs. On the other hand, it is admissible that experimenters felt less comfortable with self-destroying particles than quantum-field

[57] Seth H. Neddermeyer and Carl D. Anderson, 'Nature of Cosmic-Ray Particles', *Reviews of Modern Physics*, 11 (1939), 191–207 (p. 207).

[58] John A. Wheeler and Rudolf Ladenburg, 'Mass of the Meson by the Method of Momentum Loss', *Physical Review*, 60 (1941), 754–61 (p. 760).

[59] M. A. Pomerantz, 'The Instability of the Meson', *Physical Review*, 57 (1940), 3–12 (p. 11).

[60] Paul Weisz, 'The Rest Mass of the Mesotron', *Physical Review*, 59 (1941), 845–9 (p. 849).

theorists did and were, therefore, inclined to cling to a theoretical framework that made the self-destruction appear justified, however imperfect and unnecessary the framework might appear to us. Such a disposition, helped by the general impression that the theory was still at an immature stage, might have induced experimenters to regard the lifetime discrepancy as a puzzle for theoreticians rather than as a symptom of theory–observation incompatibility. Furthermore, as experiments moved on to unexplored territory, the theoretical framework proved repeatedly capable of anticipating new findings. The qualitative explanatory power of Yukawa's theory overrode, for a time, its quantitative shortcomings.

6. The direct observation of mesotron decay and the question of nuclear capture

Even as theoretical and experimental lifetime estimates were diverging, the nuclear connection scored a series of successes from experiments aimed at detecting the electrons emitted by decaying mesotrons. Experiments of this kind were classified as 'direct' observations of decay. The first direct observation of decay with a counter setup was attempted by C. G. Montgomery, W. E. Ramsey, D. B. Cowie, and D. D. Montgomery of the Franklin Institute. They reported 'no measurable effect'[61] and offered three possible explanations. First, mesons might not decay; in this case, anomalous absorption would need a different explanation. Second, the mesotron lifetime might be smaller than expected, and anomalous absorption could be accounted for by admitting that the mesotron mass did not exceed $50m_e$. Since this value was lower than masses measured in cloud chambers and too low to give the correct range of nuclear forces, the implication was that counter sets and cloud chambers were revealing different kinds of mesons, and that the particles seen in cloud chambers were 'nuclear mesons', locally produced in nuclear interactions, whereas counter telescopes focused on the 'cosmic-ray mesons'. But the explanation that Montgomery et al. considered most likely was the third one: cosmic-ray mesons and nuclear mesons were indeed the same, and they were the mediators of the nuclear force; as a consequence, when slowed down inside a medium, they were 'strongly absorbed by atomic nuclei' and did not undergo β-decay.[62]

At the beginning of 1940, E. J. Williams and G. E. Roberts at the University College of Wales, working with a cloud chamber, obtained proof that slowed-down mesotrons did decay into electrons (plus something invisible).[63] The probability that a mesotron came to the end of its path inside the useful volume of a cloud chamber was low, and previous instances of end-range pictures had shown no evidence of newborn electrons. Williams, in collaboration with G. R. Evans, was able to publish a second meson-electron photograph a few months after the first.[64] Williams and Roberts emphasized that, with the evidence of a mesotron track at the end of its range transforming into a fast electron track,

[61] C. G. Montgomery et al., 'Slow Mesons in Cosmic Radiation', *Physical Review*, 56 (1939), 635–9 (p. 638).
[62] Montgomery et al., 'Slow Mesons in Cosmic Radiation', 639 (note 61).
[63] E. J. Williams and G. E. Roberts, 'Evidence for Transformation of Mesotrons into Electrons', *Nature*, 145 (1940), 102–3.
[64] E. J. Williams and G. R. Evans, 'Transformation of Mesotrons into Electrons', *Nature*, 145 (1940), 818–9.

> the remarkable parallel between the mesotron and the Yukawa particle is taken one stage further. In terms of Yukawa's theory, the phenomenon observed may be described as a disintegration of the mesotron with the emission of an electron, thus constituting the most elementary form of β-disintegration.[65]

Thus, both the observation and the non-observation of electrons from stopping mesotrons reinforced the nuclear connection. Presumably, conflict between the two conclusions was prevented by the supposition that nuclear capture would depend on the density of the medium in such a way as to be predominant in a solid absorber but negligible in the gas of a cloud chamber.

A work by Yukawa and his student Daiske Okayama, although not quoted in experimental papers, would have confirmed these conjectures. Yukawa and Okayama had studied the interactions of heavy quanta with nuclei, and found the theory's predictions to be in accord with experiments: the probability of capture was negligible for fast moving quanta and for slow quanta in a gaseous medium, while it was always more probable than decay in a dense medium. But Yukawa and Okayama had assumed that only the nuclear forces acted on the particles, whereas nuclei and quanta carried electric charge. Extending their work, Sin-itiro Tomonaga and Gentaro Araki calculated the effect of the electrostatic potential of the nucleus, and found that it caused a large difference between the capture probabilities for particles of different electrical charge. Electrical repulsion was sufficient to keep positive quanta away from the nuclear zone, while electrical attraction increased the probability of nuclear capture for the negative quanta. Hence, decays should be detected, but only for positive mesons. Tomonaga and Araki pointed out that the only cloud-chamber pictures showing evidence of decay were pictures of positive tracks. They also accounted for the null result of Montgomery et al. by supposing that the 'slow mesons they observed are not identical with the ordinary cosmic-ray mesons and have a much smaller lifetime'.[66] Montgomery and associates had entertained the same possibility, although they had called *their* particles 'cosmic-ray mesons'. Despite the general predilection for a unified picture with a single kind of meson, conceding the existence of different kinds was the most common strategy for dealing with perplexing experimental evidence. These alternative scenarios usually implied that only one kind would be associated with Yukawa's theory, thus leaving the other mesons and their decays to exist as purely empirical realities, outside the reach of current explanation.

An experiment suitable to test Tomonaga and Araki's correction was set up at Laval University by Franco Rasetti, who had been one of the leaders of Fermi's group at the University of Rome. Rasetti judged that the negative result of Montgomery et al. was caused by instrumental problems. Devising a way to overcome the problems, he was able to provide a measurement of the mean lifetime—which, although not very accurate, was the first 'direct' measurement, independent of mass assumptions—as well as an estimation of the fraction of stopped mesotrons that were seen to decay. Rasetti was happy to report that his

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[65] Williams and Roberts, 'Evidence', 102 (note 63). Cloud-chamber images of stopping mesotrons were of little use for measuring the lifetime. Williams and Roberts estimated a mesotron mass of $\mu = (250 \pm 70) m_e$. For the lifetime, they could only provide a very loose upper limit of approximately 2×10^{-4} s.

[66] S. Tomonaga and G. Araki, 'Effect of the Nuclear Coulomb Field on the Capture of Slow Mesons', *Physical Review*, 58 (1940), 90–91.

decay fraction 0.42 was in accord with Tomonaga and Araki's calculations.[67] He commented on the satisfactory agreement between his mean life value and those of the indirect experiments but, following the current trend, made no mention of the two-orders-of-magnitude discrepancy with the theoretical values. The trail blazed by Rasetti was advanced by Bruno Rossi, another Italian émigré who, having been the leading figure of cosmic-ray physics in Italy, had rebuilt his career in America with a series of experimental investigations of mesotron decay.[68] Working at Cornell University in collaboration with Norris Nereson, Rossi designed an electronic timing circuit capable of measuring the time intervals between the stopping of a mesotron in a solid absorber and the ejection of an electron from the absorber. By this means, Rossi and Nereson were able to display a precise exponential decay curve and to measure the mean lifetime, $\tau = (2.3 \pm 0.2) \times 10^{-6}$ s.[69] They did not, however, measure the decay fraction. Rossi, one of the early supporters of the decay hypothesis, had at this point quietly dropped any reference to theoretical predictions. Perhaps he, too, would have become interested in nuclear capture, had he and his colleagues in America not interrupted research on fundamental physics to work on atomic weapon development at Los Alamos.

Two European teams followed Rasetti's steps during the war years: Pierre Auger, Roland Maze, and Robert Chaminade of the University of Paris, and Marcello Conversi and Oreste Piccioni of the University of Rome. Conversi and Piccioni were two junior members of the group of 'survivors' who strived to remain active in Rome after the departures of Fermi, Rasetti, Rossi, and other prominent Italian physicists.[70] Working on neutron physics under the leadership of Edoardo Amaldi and on mesotrons under the leadership of Gilberto Bernardini, these scientists endeavoured to maintain the traditions of nuclear physics and cosmic-ray studies that had been established by Fermi and Rossi. While participating in indirect experiments with Bernardini's group, Piccioni and Conversi were inspired by Rasetti's achievement to attempt a direct observation of mesotron decay. They ran their experiment under the difficult circumstances of wartime, even when their colleagues were forced to suspend their activities. What Piccioni and Conversi aimed at, and achieved, was to improve on Rasetti's work by using advanced fast electronics. With their first series of measurements, completed in the spring of 1944, they were able to display a curve

[67] Franco Rasetti, 'Disintegration of Slow Mesotrons', *Physical Review*, 60 (1941), 198–204. It was known that mesotrons were positively and negatively charged, with a 20% excess of positives.

[68] Rossi's own recollections are in Bruno B. Rossi, 'The decay of "mesotrons" (1939–1943): experimental particle physics in the age of innocence', in *The Birth of Particle Physics*, 183–205 (note 5); Bruno Benedetto Rossi, *Moments in the Life of a Scientist* (Cambridge, 1990).

[69] Bruno Rossi and Norris Nereson, 'Experimental Determination of the Disintegration Curve of Mesotrons', *Physical Review*, 62 (1942), 417–22.

[70] E. Amaldi, 'Gli anni della Ricostruzione. Parte I', *Scientia*, 114 (1979), 29–50; Edoardo Amaldi, Giovanni Battimelli, and Michelangelo De Maria, *Da via Panisperna all'America: I fisici italiani e la seconda guerra mondiale* (Rome, 1997); M. Conversi, 'The period that led to the 1946 discovery of the leptonic nature of the "mesotron"', in *The Birth of Particle Physics*, 242–50 (note 5); M. Conversi, 'Early Study of Muons and Muon Decay', in *50 Years of Weak Interactions*, 154–67 (note 5); Marcello Conversi, 'From the discovery of the mesotron to that of its leptonic nature', in *40 Years of Particle Physics*, 349–68 (note 5); Oreste Piccioni, 'The observation of the leptonic nature of the 'mesotron' by Conversi, Pancini, and Piccioni', in *The Birth of Particle Physics*, 222–41 (note 5); Oreste Piccioni, 'The history of the discovery of the extended leptonic nature and a comment on an article in *Scientia*', in *50 Years of Weak Interactions*, 486–508 (note 5); Oreste Piccioni, 'The Discovery of the Muon', in *History of Original Ideas and Basic Discoveries in Particle Physics*, 143–62.

of exponential decay, and to measure the lifetime as $\tau = 2.33 \times 10^{-6}$ s ± 6.5 per cent.[71] Since communications between Italy and the United States were closed at the end of 1941, Piccioni and Conversi did not know of Rossi and Nereson's work until the following summer, when communications across the Atlantic were re-opened. Their virtual interlocutors were Auger, Maze, and Chaminade, who had also recorded a decay fraction in Rasetti's style, but had found it to be close to one. Piccioni and Conversi measured the decay fraction to be 0.49, in clear disagreement with the French and in reasonable agreement with Rasetti; therefore, they thought that they had confirmed the Tomonaga–Araki effect.[72]

The fact that only a fraction of the stopping mesotrons were seen to decay could be accounted for not only by charge-selective nuclear capture but also by the existence of a stable component in the mesotron population. Extending the resources of their experimental tradition, Piccioni and Conversi found a way to provide a 'presumably definitive answer' to this question.[73] They placed above the apparatus two magnetized iron blocks, with opposite magnetization, which acted as 'magnetic lenses', focusing towards the counters particles of one charge and deflecting away particles of opposite sign. Piccioni and Conversi prepared the experiment, and another researcher, Ettore Pancini, joined them upon his return to Rome after his participation in the Partisan movement in northern Italy. Thus, the first of this series of measurements to appear in the international literature was a letter by Conversi, Pancini, and Piccioni of October 1945, in which they reported having confirmed once again the Tomonaga–Araki effect with the use of magnetic lenses.[74] Reporting about the activities of the Rome group at the first post-war gatherings of Italian and European physicists, Amaldi and Bernardini were able to quote Piccioni, Conversi, and Pancini's result as the final stroke in a picture of impressive coherence, according to which Yukawa's theory constituted the unifying theoretical framework for research on nuclear and cosmic-ray physics.[75]

7. Mesons in 1946

Conversi, Pancini, and Piccioni's demonstration of charge-dependent nuclear capture was received internationally as important evidence about the nuclear interactions of mesotrons. In mid-November 1945, The American Philosophical Society hosted a symposium on 'Atomic Energy and its Implications'. It was one of the many gatherings in which physicists, while celebrating demobilization, began

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[71] M. Conversi and O. Piccioni, 'Misura diretta della vita media dei mesoni frenati', *Il Nuovo Cimento*, 2 (1944), 40–70; M. Conversi and O. Piccioni, 'On the Mean Life of Slow Mesons', *Physical Review*, 70 (1946), 859–73.

[72] M. Conversi and O. Piccioni, 'Sulla disintegrazione dei mesoni lenti', *Il Nuovo Cimento*, 2 (1944), 71–87; M. Conversi and O. Piccioni, 'On the Disintegration of Slow Mesons', *Physical Review*, 70 (1946), 874–81.

[73] Conversi and Piccioni, 'Sulla disintegrazione dei mesoni lenti', 87 (note 72).

[74] M. Conversi, E. Pancini, and O. Piccioni, 'On the Decay Process of Positive and Negative Mesons', *Physical Review*, 68 (1945), 232.

[75] Edoardo Amaldi, 'Sulle ricerche di fisica nucleare eseguite a Roma nel quadriennio di guerra. Relazione presentata al Convegno dei fisici ed elettrotecnici, Como, novembre 1945', *La Ricerca Scientifica*, 16 (1946), 61–5; Gilberto Bernardini, in *Report of an International Conference on Fundamental Particles and Low Temperatures, held at the Cavendish Laboratory, Cambridge, on 22–27 July 1946*, vol. I (London, 1947).

reckoning with recent contributions of nuclear physics to weapons research, and pondered the discipline's future. One of the main themes emerging from these reflections was the scientists' urge to free themselves from demands of immediate practical results, and to reapply their talents to the problems of pure research. Mindful to reclaim their disciplinary independence without forgoing the political clout that they had earned, physicists insisted more strongly than ever that basic and applied science are separate yet genetically related endeavours: pure research generates knowledge, and knowledge generates applications. The science that had produced the bomb had become the poster-perfect illustration of the argument with which Vannevar Bush was promoting the National Science Foundation, a publicly funded agency at the same time devoted to the common good and governed by 'the free play of free intellects'.[76] Nuclear physicists could persuasively advocate the view that their recent advances were only the first steps into an unknown field; that reactors and bombs were samples of the technological harvest to be expected from the new field; and that physical research would best fulfil its utilitarian mandate if it was allowed to grow without utilitarian constraints.

In his lecture at the symposium, John A. Wheeler applied Bush's argument to nuclear physics by means of the exploration metaphor, linking it to the coded image of the 'Italian navigator' reaching the new world of controlled nuclear fission.[77] He called processes like fission, in which the total number of nucleons (i.e. protons and neutrons) was conserved, 'nucleonic transformations' and likened them to the island touched by Columbus. Beyond the 'island of nucleonics', the vast 'continent of ultranucleonics' awaited discovery. Unlike Columbus, Wheeler and colleagues did have some anticipation of what lay ahead, because they could use as a sextant Einstein's mass–energy relation, which revealed that the energy liberated in nucleonic reactions was only a small fraction of that locked within matter. 'Ultranucleonic transformations', which implied the destruction of neutrons and protons, would make the rest energy of these particles available. Indication that such transformations were possible was found in cosmic radiation, in which protons were destroyed in nuclear collisions in the upper atmosphere, creating bursts of mesons.

> Discovery [*sic*] how to release the untapped energy on a reasonable scale might completely alter our economy and the basis of our military security. For this reason, we owe special attention to the branches of ultranucleonics—cosmic-ray phenomena, the mechanism of energy production in special stars, field theory, and particle transformation physics—where a single development may

[76] *Science, the endless frontier: a report to the President by Vannevar Bush, director of the Office of Scientific Research and Development, July 1945* (Washington, DC, 1945). Whether or not atomic weaponry fitted the definition of common good was, of course, another important issue in physicists' reflections. Separating pure science from its applications was also a way to deal with this problem. The themes that I outlined here are also considered, in a larger historical perspective, in S. S. Schweber, 'Some Reflections on Big Science and High Energy Physics in the United States', *Rivista di Storia della Scienza*, 2 (1994), 127–89.

[77] 'The Italian Navigator has reached the New World' was the message with which A. H. Compton (head of the Metallurgical Laboratory in Chicago) telephonically informed J. B. Conant (chairman of the National Defense Research Committee) that Fermi had successfully activated the first atomic pile on 2 December 1942. The anecdote is reported, with small variations, by many sources, for example, Laura Fermi, *Atoms in the Family; My Life With Enrico Fermi Architect of the Atomic Age* (Chicago, 1954), 198; E. Segrè, *Enrico Fermi Physicist*, paperback ed. (Chicago, 1972), 129; Daniel J. Kevles, *The Physicists. The History of a Scientific Community in Modern America* (New York, 1979), 326 and references therein.

> produce such far-reaching changes. Other nations have not neglected this work during the war. We must prepare to resume it vigorously.[78]

It was now time, Wheeler argued, to survey past and present, and to plan the future. The first thing to do was to separate the experimental concerns—that is, draw an inventory of known facts about particles, and set out to increase the store of experimental knowledge—from the goal of formulating a theory to reduce the empirical knowledge to order. Wheeler's experimental strategy was well defined: energetic particles should be made and studied in the laboratory by means of accelerators. A new machine was already operating at the General Electric Company in Schenectady, New York. This device was the most advanced of a new type of particle accelerator, called 'induction accelerators' or 'betatrons'. It was capable of accelerating electrons to 100 MeV, and was regarded mainly as a means to generate X-rays. The X-rays radiated by the accelerated electrons had enough energy to generate mesons, which could thus be produced in large numbers in a controlled environment. According to Wheeler, the betatron researchers had already obtained, although not yet announced, the creation of mesons. Mass production of mesons in betatrons was therefore an open path to ultranucleonics. But the drive to more powerful accelerators should not stop there. Since there were reasons to believe that high-energy cosmic protons were destroyed in explosive micro-events upon entering the atmosphere, with the resulting production of multiple mesons, a goal of ultranucleonics was the construction of new machines capable of accelerating protons up to 1–5 billion electron-volts in order to take control of these processes. Universities could not afford such expensive apparatus, but if there were any scientific projects which deserved government support, this was one of them. Another path to ultranucleonics was the study of cosmic radiation at high altitudes, in which particles of extremely high energy (up to 10^{18} eV) were found. Among the cosmic-ray phenomena to be investigated, the most relevant were the creation and destruction of mesons. High-altitude experiments were also costly. Transporting heavy scientific equipment to the upper atmosphere by means of military airplanes was 'the kind of program under which one or the other of the armed services [could] make a substantial contribution to fundamental science'.[79]

The advocacy for large-budget projects hinged upon the claim that much experimental work still needed to be done on the way to ultranucleonic energy, because meson theory was still 'in a state of free experimentation with ideas and great uncertainty as to principle', and was unable to give a 'reasonable quantitative account' of the radioactive decay of mesons and nucleons.[80] Here was a landscape quite different from that described by Amaldi at more or less the same time. Amaldi envisioned Yukawa's theory as the unifying framework of nuclear and cosmic-ray studies and considered it a conceptual structure essentially confirmed by experiments, with only formal details to be adjusted. Wheeler regarded Yukawa's theory and the class of models derived from it as speculative and inadequate, and highlighted the existence of a body of experimental evidence that was independent

[78] John A. Wheeler, 'Problems and Prospects in Elementary Particle Research', *Proceedings of the American Philosophical Society*, 90 (1945), 36–47 (pp. 36–7).

[79] Wheeler, 'Problems and Prospects', 39 (note 78).

[80] The magnetic moment of proton and neutron was another phenomenon of which meson theory failed to give a quantitative account. Wheeler, 'Problems and Prospects', 45 (note 78).

of it. The Conversi–Pancini–Piccioni demonstration of nuclear capture was the capstone of the nuclear connection in one view, and the first step of an open-ended empirical exploration in another. As a pre-theoretical fact, it was essential to the agenda of ultranucleonics because it showed that mesotrons did enter in nuclear reactions. Many questions, however, remained to be answered:

> (1) with what masses may mesons exist; (2) is the radioactive life of a meson uniquely fixed by its mass; (3) does the life time vary in a substantial way between mesons of different masses; (4) is it possible to establish the existence of those mesons which are sometimes postulated to exist with a characteristic life several orders of magnitude shorter than the value, about two microseconds, found for the great majority of the mesons which reach sea level; does the radioactive decay of a meson always take place by division into an electron or positron and a neutrino [. . .]?
> [. . .] can confirmation be obtained for preliminary indications that roughly half of the mesons whose energies are moderated in solid bodies undergo capture [. . .] are negative mesons captured by atomic nuclei; and, if so, is the capture cross section a selective or a smoothly varying function of atomic number [. . .]?[81]

These questions were likely to occur to any mesotron researcher who caught the wind change and decided to switch to theory-skeptical mode.

The American Physical Society (APS) gathered for its 1945 annual meeting—a meeting of 'unforeseen and unprecedented size'[82]—in January 1946, in New York. The main event of the conference was a symposium on nuclear energy, but a real sensation was stirred by the session 'Nuclear Transformations Produced by High-Energy X-Rays', which featured reports from the 100 MeV betatron. The first paper, 'Artificial Production of Mesons by X-Rays from a 100-MeV Induction Accelerator', was presented by Marcel Schein of the University of Chicago, one of the physicists who had proposed the production of multiple mesons by primary protons in the high atmosphere. Schein was followed by G. S. Kleiber, A. J. Hartzler, and D. C. Baldwin of General Electric, who talked about nuclear processes induced by high-energy X-rays and the artificial production of particles of intermediate mass. Unfortunately, only the titles of these papers were published.

The impact of the betatron news on the physics community can be reckoned, for example, by the fact that the first project of the newly established Institute for Nuclear Physics at the University of Chicago, where Fermi and Edward Teller went to work once they left Los Alamos, was a 100 MeV betatron.[83] And the long-range effects of the announced manufacture of mesons can be glimpsed from a letter that Gilberto Bernardini wrote his friend, the theoretician Enrico Persico, after hearing reports from his colleagues across the Atlantic:

> In Rome, with great difficulty we have started working again. Actually, at first, when the Phys. Rev. arrived until all of 1944, we were under the illusion that we

[81] Wheeler, 'Problems and Prospects', 44 (note 78).
[82] Karl K. Darrow, '1945 Annual Meeting at New York, January 24–26, 1946'. *Proceedings of the American Physical Society,* in *Physical Review*, 69 (1946), 246.
[83] Fermi to Amaldi and Wick, 24 January 1946, in Amaldi, Battimelli, and De Maria, *Da via Panisperna all'America*, 166 (note 70). In 1947, the project was changed into a 450 MeV synchrocyclotron. (Segrè, *Fermi*, 173 (note 77).)

> had kept up. Unfortunately, it was only an illusion and now, with the Plutonium piles, the 100 MeV betatrons (Bruno Pontecorvo wrote me that they made homemade mesons, and that it seems that there are many meson masses, from 20 electron masses up; if it is true, it is a thing of exceptional importance for all elementary particle physics), we are losing ground at kilometers per second, and perhaps with no hope of catching up again.[84]

Despite the commotion, betatron-made mesons have disappeared from the annals of particles physics, according to which the first 'artificial' mesons were produced in 1948 at Berkeley's 184-inch synchrocyclotron.[85] What happened to the 1946 vintage of homemade mesons? General Electric hired Bethe as a consultant. Schein and associates had directed their beam of X-rays to a cloud chamber immersed in a magnetic field and had examined the tracks produced in the chamber by charged particles, which were presumably generated in nuclear reactions between the high-energy photons and the nuclei in the chamber walls. In order to identify the particles, the betatron physicists applied a common method: they measured the curvature and range of the tracks that ended in the chamber, and calculated the mass from these quantities. They recorded a variety of masses intermediate between the electron and proton masses. Bethe found reason to question these findings. He warned that the scattering of the particles on the gas molecules that filled the chamber could alter the curvature considerably, and that strong magnetic fields and gases of low atomic number should be used to minimize the disruption. Since these conditions were not met at the General Electric laboratory, Bethe concluded, 'Curvature measurements on tracks ending in the chamber are therefore in general not significant'.[86]

Schein, Hartzler, and Kleiber published a written version of their results only in September. The brevity of their communication suggests that Bethe's intervention inflicted considerable damage upon their earlier claims. They reported having found tracks not immediately identifiable as electrons or protons but concluded that it was necessary to carry out further investigations before their nature could be identified.[87] Also, in September, Wheeler organized a meeting in New York, which was attended by many visitors from overseas and included only papers on cosmic rays, the design and operation of accelerators, and elementary particle theory. In the main session, 'Cosmic-ray and Subnucleonic Physics', Kleiber, Baldwin, and another collaborator, E. A. Luebke, disclosed the outcome of their investigations. They had collected a sample of 98 end-range tracks, using gasses of low atomic number and increasing the magnetic field as Bethe had advised, and also recorded a control sample without magnetic field to evaluate the effects of scattering. 'There is no evidence from this

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[84] '*A Roma, con grande difficoltà abbiamo ripreso a lavorare. Anzi in un primo tempo, quando arrivarono le Phys. Rev. fino a tutto il 1944, avemmo l'illusione di aver mantenuto il passo. Purtroppo era solo un'illusione e ora, con le pile al Plutonio, i betatroni da 100 MeV (mi ha scritto Bruno Pontecorvo che hanno fatto i mesoni in casa e che sembra che vi siano tante masse mesoniche, da 20 masse elettroniche in su; se è vero, è cosa di eccezionale importanza per tutta la fisica delle particelle elementari), perdiamo terreno a chilometri al secondo e forse senza speranza di recupero*'. Bernardini to Persico, 4 February 1946, in Amaldi, Battimelli, and De Maria, *Da via Panisperna all'America*, 168–9 (note 70).

[85] See, for example, Robert N. Cahn and Gerson Goldhaber, *The Experimental Foundations of Particle Physics* (Cambridge, 1989), 20; Pais, *Inward Bound*, 479 (note 5).

[86] H. A. Bethe, 'Abstract H5. Influence of Multiple Scattering on Curvature Measurements', *Proceedings of the American Physical Society,* in *Physical Review*, 69 (1946), 689.

[87] Marcel Schein, A. J. Hartzler, and G. S. Klaiber, 'Production of Heavily Ionizing Particles by X-Rays Generated by a 100-Mev Betatron', *Physical Review*, 70 (1946), 436.

series of photographs for particles of intermediate mass'.[88] All the apparent deviations from proton masses could be accounted for by multiple scattering.

The excitement about artificial mesons projected the multi-meson scenario in sharper relief than cosmic rays had ever done, and roused an interest in nuclear processes involving meson creation that the betatron disappointment did nothing to abate. At the 'Cosmic rays and Subnucleonics' meeting, Anderson and Rossi summarized the status of experimental evidence and submitted lists of outstanding experimental questions very similar in substance to Wheeler's.[89] The production of mesons in the upper layers of the atmosphere was the main focus of many speakers. Schein described a series of balloon experiments that supported the hypothesis that cosmic-ray mesons originated from the interaction of primary protons with air nuclei. Bethe and Richard P. Feynman developed theoretically Schein's hypothesis. The question of the mass was also lively debated. Donald J. Hughes reviewed the methods of mass measurement and concluded that, despite the large uncertainties, it was extremely likely that the mesotron did not possess a unique mass. Louis Leprince-Ringuet, summarizing the results of a long series of observations by his team at the École Polytechnique, argued that in cosmic radiation there exist mesons of about $200m_e$ as well as mesons four to five times heavier. In contrast, W. B. Fretter and R. B. Brode reported that their observations with a doublet of cloud-chambers at the University of California consistently indicated a mass between 160 and $200m_e$. The claims about observations of mesons of different masses, in cosmic rays as well as at the General Electric betatron, underwent scrutiny by Bethe towards the end of the year. Having examined each case in detail, Bethe summed them up in one statement: 'An analysis [of the effects of multiple scattering] shows that *all* published meson tracks are compatible with a unique mass of about 200 electron masses'.[90] He concluded by solemnly voicing his aversion for multiple-mass particles:

> All known fundamental particles have a definite rest mass. To discover a particle for which this is not the case would be a tremendous deviation from previous experience. For this reason, the most unambiguous and accurate experimental evidence would have to be obtained before a non-unique rest mass could be accepted. It is obviously very easy to explain each new experiment by a new assumption, and this was done in the last few years in the case of the meson mass: It is much more difficult to explain all experiments by a minimum of assumptions, which was the way in which physics has progressed in the past. The burden of proof lies always with the discoverer of a new phenomenon, and must in this case lie with the advocates of the variable rest mass of the meson.[91]

This was an unfortunate case of overkill. Time has proven Bethe right for almost all the tracks he examined, but among the putative mesons that he disqualified was also a $990m_e$ particle that Leprince-Ringuet and Michel L'héritier had found in 1944 at

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[88] G. S. Klaiber, E. A. Luebke, and G. C. Baldwin, 'Abstract C11. Range–Momentum Measurements of Particles Emitted in Nuclear Disintegrations Induced by 100-MeV X-Rays', *Proceedings of the American Physical Society,* in *Physical Review*, 70 (1946), 789–90.

[89] Bruno Rossi, 'Abstract C7. Some Problems in the Study of Cosmic-Ray Mesons', *Proceedings of the American Physical Society,* in *Physical Review*, 70 (1946), 788.

[90] Emphasis in the original. H. A. Bethe, 'Multiple Scattering and the Mass of the Meson', *Physical Review*, 70 (1946), 821–9 (p. 821).

[91] Bethe, 'Multiple Scattering and the Mass of the Meson', 829 (note 90).

1000 m of altitude in the Alps, and that is now considered to be the first observed K^+ meson.[92]

The outlook that characterized the beginnings of 'high-energy nuclear physics'[93] was a back-to-basics approach according to which, on the one hand, existence and decay of mesotrons were purely empirical phenomena and, on the other hand, it was understood that mesotrons played an essential part in nuclear processes. In other terms, the dominant attitude combined skepticism toward the formal structure of meson theory, as derived from Yukawa's work, with reliance on the causal content of the theory itself, which had come to be regarded as a-theoretical. This ambivalence surfaces, for example, in the review 'Physics in 1946' by Philip Morrison. In the section titled 'The Push Toward High Energy', Morrison wrote:

> These objects, known now as mesotrons [...], are believed on quite general grounds to be associated with those extraordinary attractive forces which make nuclear matter sticky, which bind together the neutrons and protons in the nucleus. [...][94]

The belief rested on the description of forces as the exchange of field quanta and on the relation between the range of the nuclear field to the mass of the quanta. As for the theory that had articulated the existence of mesotrons, their decays, and the mass–range relation out of the general principles, Morrison declared,

> It must be said that [...] the best theorists have worn thin their patience on this theory. No consistent description has yet been given of the properties of nuclear forces. All the more has this challenged experimenters to make mesons in the laboratory and there to study them in the number and the detail which is impossible while their source is still only the cosmic ray [*sic*].[95]

8. The Conversi–Pancini–Piccioni effect and meson-to-meson decays

The attention of mesotrons researchers in 1946 was fixed on nuclear capture. Several factors converged upon this phenomenon. Nuclear physicists emerged from war-related research with a magnified sense of possibilities for experimental investigations, the perception that a fundamental theory of nuclear forces was still wanting, and the conviction that mesons were agents of nuclear forces. Manufacturing mesons in accelerators was identified as the first goal of a new research programme, which galvanized interest on the conditions required for meson production. It was agreed that mesons were created in high-energy nuclear reactions and that the emission of a meson from a nucleus was the reverse process of capture by a nucleus. At the same time, cosmic-ray experimenters, advancing within the conceptual framework of Yukawa's theory, had moved on from confirming the existence of decays to using decay measurements to create a set of practices for the production and observation of a new phenomenon, namely, meson capture.

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92 L. Leprince-Ringuet and M. L'héritier, 'Existence probable d'une particule de masse 990 m_e dans le rayonnement cosmique', *Comptes Rendus de l'Academie des Sciences de Paris*, 219 (1944), reprinted in Cahn and Goldhaber, *The Experimental Foundations of Particle Physics*, 66–8 (note 85).

93 Brown, Dresden, and Hoddeson, 'Pions to quarks', 3 (note 7).

94 Philip Morrison, 'Physics in 1946', *Journal of Applied Physics*, 18 (1947), 133–52 (p. 136).

95 Morrison, 'Physics in 1946', 136 (note 94).

Already, in November 1945, Wheeler thought that the necessary follow-up to Conversi, Pancini, and Piccioni's experiment was a study of nuclear capture in absorbers of different atomic number. Tomonaga and Araki predicted that the capture probability did decrease with decreasing atomic number, but so slowly as to remain higher than the decay probability for negative mesotrons stopped in any medium. In Wheeler's view, however, experiments were to provide the basis for a future theory rather than to test an existing prediction, which there was no reason to believe anyway. There is no evidence that Conversi, Pancini, and Piccioni knew about Wheeler's list of outstanding experimental problems; there is, however, evidence that physicists in Rome were eagerly watching the developments across the Atlantic,[96] and it is plausible that the three young researchers were attentive to their work being discussed in international circles. In any case, they were the first to provide an answer to the atomic number test. Further investigations, pursued by means of modifications of their apparatus and along the lines of their previous results, were both feasible and advisable, and there is no doubt that questions about the mechanism of nuclear capture were in the air, in Rome as elsewhere, during 1946. An example is provided by a paper presented in July by Bruno Ferretti, a theoretician in the Rome group, at the first conference of European physicists after the war. Ferretti re-examined the calculations of nuclear capture, and argued that Yukawa and Okayama were wrong by several orders of magnitude because they treated the process as the absorption of a quantum by a single particle rather than by a composite system of particles.[97]

In all the experiments derived from the Montgomery et al. attempt, mesotrons had been stopped in 'absorbers' of lead, iron, or aluminum. Conversi, Pancini, and Piccioni took the step of replacing the iron absorber in their apparatus with one made of graphite.[98] They observed that in graphite both negative and positive mesotrons decayed at the same rate. After what Piccioni remembered as a 'via crucis' of experimental checks,[99] the three researchers reached the conclusion that their observation was incompatible with Tomonaga and Araki's calculation.[100] Fermi received communication of this result from Amaldi in November.[101] Piccioni was also able to spread the information personally at the same time, as he left Italy to join Rossi's group at M.I.T. as a visiting researcher. The news fell on prepared ground. Two similar experiments already in progress—one at Princeton University under the

[96] In 1945–46, the Rome physicists were planning the construction of a betatron. One of them, Bernardo Nestore Cacciapuoti, visited several laboratories in the USA from November 1945 to February 1946, where he collected information on the new accelerators. (Cacciapuoti to Amaldi e Bernardini, 8 December 1945, and Cacciapuoti to Amaldi, 1 January 1946, *Archivio Amaldi*, Box 136, file 3, Sezione Archivi del Dipartimento di Fisica dell'Università di Roma 1, 'La Sapienza'. See also Amaldi, Battimelli, and De Maria, *Da via Panisperna all'America*, 108 (note 70).)

[97] B. Ferretti, 'The Absorption of Slow Mesons by an Atomic Nucleus', in *Cambridge Conference Report*, 75–7 (note 75).

[98] Different points of view on whether they intended to investigate the atomic number dependence or not are offered by Piccioni and Conversi in their recollections (note 70). According to all accounts, the result that they obtained was completely unexpected.

[99] Piccioni, 'Discovery of the extended leptonic nature', 498 (note 70).

[100] M. Conversi, E. Pancini, and O. Piccioni, 'On the Disintegration of Negative Mesons', *Physical Review*, 71 (1947), 209–10; M. Conversi, E. Pancini, and O. Piccioni, 'Sull'assorbimento e sulla disintegrazione dei mesoni alla fine del loro percorso', *Il Nuovo Cimento*, Ser. 9, 3 (1946), 372–90; M. Conversi, E. Pancini, and O. Piccioni, 'Sul comportamento dei mesoni positivi e negativi alla fine del loro percorso', *Rendiconti dell'Accademia Nazionale dei Lincei*, Ser. 8, Vol. 2 (1947), 54–7.

[101] Amaldi to Fermi, 28 November 1946, in *Enrico Fermi Collection*, Box 9, Series II (Special Collection Research Center, The University of Chicago Library).

guidance of Wheeler[102], and the other at M.I.T., inspired and assisted by Rossi[103]—were soon to confirm the Rome result and extend it to other elements. Theorists were even more responsive. Wheeler integrated the data into a new model of interactions between mesotrons and nuclei of different electric charge, which he had already begun to formulate.[104] Fermi and Teller, in collaboration with Victor Weisskopf, examined the extent of the discrepancy between the Rome result and the existing model of nuclear capture. The Rome experiment showed that the time of capture of mesotrons in carbon was no lower than the time of decay, that is, of the order of 10^{-6} s, whereas according to 'conventional meson theories', the time of nuclear capture of mesotrons was 10^{-18} s or lower. The time disagreement meant that mesotron nuclear interactions were much less probable than expected, precisely 10^{12} times less probable. A disagreement like this could not be easily discounted; it demanded 'a very drastic change in the forms of meson interactions'.[105] Both Wheeler and Fermi et al. concluded with remarks on the significance of their analyses for the production of mesons with 100 MeV accelerators:

> Indeed, the creation of a mesotron by X-rays or fast protons is the reverse of processes (1) and (2) [i.e. processes of nuclear capture]. If the interaction according to these processes is much weaker than expected, one would conclude the same for the reverse process. Thus, one might be in doubt as to whether one can produce abundant numbers of artificial mesotrons with bombardment-energies only little above the threshold for single-mesotron-production.[106]

Wheeler and Fermi et al. took the Rome experiment as demonstrating that nuclear capture was just about as probable as spontaneous decay for mesotrons. It only became relatively more probable in materials of high atomic number because the nucleus was correspondingly larger. According to meson theory, in contrast, nuclear capture should have been vastly more probable than decay because of the great difference between the coupling constants for the two processes. Despite the shock effect of the large disagreement factor displayed by Fermi, Teller, and Weisskopf, this part of the reasoning was a straightforward consequence of Yukawa's two coupling constants. The gist of Wheeler's and Fermi's reasoning was that a mesotron, when slowed down to thermal speed (that is, a speed comparable to that of atomic electrons), would take a negligible time to descend into the lowest Bohr orbit of an atom, which fell into the range of action of the nuclear forces. This point was not self-evident and, indeed, was questioned by Niels Bohr and Herbert Fröhlich. Bohr called a mini-congress in Copenhagen to discuss the issue, and engaged Ferretti, who championed Fermi's view, in vigorous debate.

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102 T. Sigurgeirsson and A. Yamakawa, 'Decay of Mesons Stopped in Light Materials', *Physical Review*, 71 (1947), 319–20. See also Thorbjorn Sigurgeirsson and K. Alan Yamakawa, 'Electron Emitting Power of Stopped Mesons', *Reviews of Modern Physics*, 21 (1949), 124–32.

103 G. E. Valley, 'The Radioactive Decay of Slow Positive a Negative Mesons', *Physical Review*, 72 (1947), 772–83.

104 John A. Wheeler, 'Abstract A4. Mechanism of Absorption of Negative Mesons', *Proceedings of the American Physical Society,* in *Physical Review*, 71 (1947), 71; John A Wheeler, 'Mechanism of Capture of Slow Mesons', *Physical Review*, 71 (1947), 320–1.

105 E. Fermi, E. Teller, and V. Weisskopf, 'The Decay of Negative Mesotrons in Matter', *Physical Review*, 71 (1947), 314–5.

106 Fermi, Teller, and Weisskopf, 'Decay of Negative Mesotrons', 315 (note 105).

He also wrote a letter to Fermi and Teller.[107] The question could not be settled within the received theoretical framework because the theory of energy losses dealt only with fast particles and nuclear theory only with particles within the range of nuclei. Substantiating the Fermi et al. interpretation required the development of a new theoretical model for the interactions of slow mesotrons with atoms.[108]

At the beginning of June 1947, a select group of American and adopted American physicists gathered in a resort on Shelter Island, an islet at the eastern tip of Long Island, to take part in a meeting called 'Fundamental Problems of Quantum Mechanics'. A summit of theoreticians, the conference was designed as a tribute to the men who had contributed so effectively to the war effort, and as a way to consolidate the intellectual and social value of pure physics.[109] It was the first of three historic conferences—Shelter Island, 1947; Pocono, 1948; and Oldstone, 1949—that set the basis for the post-war development of quantum field theory. Oppenheimer, one of the 'discussion leaders', brought to the table the irreconcilability between the high rate of meson creation in the upper atmosphere and the scarcity of subsequent interactions of mesons in matter. As Oppenheimer saw it, the problem lay in the 'formal correspondence between the creation of a particle and the absorption of an antiparticle', which was regarded as the expression of a general principle, the principle of microscopic reversibility. Oppenheimer wondered whether this principle should be 'abandoned to accord with the experimental facts'.[110] Weisskopf, another discussion leader, listed 'Discrepancies in the interaction of mesons with matter (Piccioni experiment)' among the problems of nuclear and meson theories to be confronted by the conference.[111] According to Robert E. Marshak's recollection,

> [t]he discussion of the Italian experiment became very animated at the first Shelter Island conference, but there was very little inclination to support Oppenheimer's suggestion that one should consider surrendering microscopic reversibility.[112]

The opinion prevailed that mesons were not generated directly from disintegrations of nuclei but through some intermediate process that accounted for the apparent asymmetry between emission and absorption. Marshak put forward the hypothesis that met with most approval. He argued that admitting the existence of mesons with two different masses would suffice to salvage the principle of reversibility: the heavier mesons might be generated by primary protons in the outmost

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[107] Ferretti, personal communication. See also B. Ferretti, 'Sulla cattura atomica dei mesoni lenti', *Il Nuovo Cimento*, 5 (1948), 325–66 (p. 326, footnote 5). For Fröhlich's argument, see H. Fröhlich, 'Decay of Negative Mesons in Matter', *Nature*, 160 (1947), 255.

[108] E. Fermi and E. Teller, 'The Capture of Negative Mesotrons in Matter', *Physical Review*, 72 (1947), 399–408.

[109] S. S. Schweber, 'A Short History of Shelter Island I', in *Shelter Island II. Proceedings of the 1983 Shelter Island Conference on Quantum Field Theory and the Fundamental Problems of Physics*, edited by R. Jackiw et al. (Cambridge, MA, 1985), 302–43.

[110] J. R. Oppenheimer, 'The Foundations of Quantum Mechanics. Outline of Topics for Discussion', in Schweber, 'Shelter Island', Appendix, p. 339 (note 109).

[111] V. F. Weisskopf, 'Foundations of Quantum Mechanics. Outline of Topics for Discussion', in Schweber, 'Shelter Island', Appendix, p. 338 (note 109).

[112] R. E. Marshak, 'Particle physics in rapid transition', 381 (note 56).

atmospheric layer, with rates compatible with the intensity of the nuclear force. They might then decay into the lighter kind, which interacted only weakly with nuclei and would therefore constitute the penetrating particles observed in the lower atmosphere.

Marshak's two-meson hypothesis was a version of the possible existence of mesons of different masses, which had been variously entertained ever since the onset of intermediate particles, sometimes on the basis of mass and lifetime data, other times for theoretical reasons, and still other times to repair mismatches between theory and observation. At the 1946 Cambridge conference, for instance, Christian Møller and Abraham Pais had presented considerations directed at explaining the 'large spread of mass values' of the penetrating cosmic rays. According to Møller and Pais, all fundamental particles did not have single masses but mass spectra, and were classified into three groups: nucleons, mesons, and light particles. For the light particles in all their mass states, Møller proposed the name 'leptons', from the Greek word for 'small'.[113] This was a radical version of the multi-meson scenario, the kind of creative theorizing that Bethe stigmatized as a cheap remedy for phenomenal diversity. A more moderate hypothesis was discussed in early 1947 by Ferretti as a possible solution of the discrepancy between theoretical and experimental lifetimes. Ferretti postulated that besides the observed mesotrons, there existed another kind of meson, having a mean lifetime of the order of 10^{-8} s. This short-lived meson would be Yukawa's particle, the one which allowed a unified description of nuclear forces and β-radioactivity.[114] Short-lived mesons, to be found only in the outer atmospheric layer, had also been the subject of one of Wheeler's questions. And Rossi had discussed them at the 'Cosmic-Ray and Subnucleonic Physics' meeting as a possible explanation of the increase with altitude of the electrons to mesons ratio, assuming that they decayed into electrons.

Marshak's hypothesis was conceived as a response to framing the mesotron problem as a conflict between high-rate creation and low-rate destruction processes. Formulating the question in these terms precipitated the awareness, already latent in meson theory, that some (putative) meson interactions were 'strong' and others 'weak', and led to the realization that there was no evidence of mesotron participation in 'strong' interactions in the low atmosphere.[115] As Schwinger's and Sakata and Inoue's two-meson theories, Marshak's hypothesis postulated a parent–daughter relationship between two mesons, which implied not only an explicit generalization of the spontaneous instability of particles, but also the admission that both the decay of heavy mesons into light mesons and the decay of light mesons into electrons were phenomena independent of nuclear radioactivity. Yet, in its original formulation, Marshak's idea was more conservative than it was made to appear in later descriptions. Although Marshak made no reference to Yukawa's theory, he

[113] Møller, in *Cambridge Conference on Fundamental Particles*, 184 (note 75). See also Pais, *Inward Bound*, 449–50 (note 5). According to Pais, Møller also invented, in 1941, the term 'nucleons' for the heavy particles.

[114] B. Ferretti, 'Sull'ipotetico mesone di vita media molto breve', *Il Nuovo Cimento*, Ser. 9, 3 (1946), 307–19.

[115] The absence of strong interactions, even in experiments prior to the Rome experiment, was emphasized by Weisskopf. See Victor F. Weisskopf, 'On the Production Process of Mesons', *Physical Review*, 72 (1947), 510, and reference therein.

attempted to preserve the integrity of Yukawa's picture by admitting that the heavy mesons, in addition to disintegrating into light mesons, also underwent β-decay.[116]

The participants of the Shelter Island conference were not aware that a team of cosmic-ray researchers at the University of Bristol, led by C. F. Powell, had already published evidence of mesons transforming into other mesons.[117] Powell and his team had exposed a group of photographic plates—coated with emulsions that had been especially developed for the detection of subatomic particles—to high-altitude cosmic radiation in the Observatory of Pic du Midi in the Pyrenees. As they carefully analysed the plates, they found new phenomena, which they published in successive reports, formulating their interpretations in connection with current developments in meson physics.[118] In February, Powell and G. P. S. Occhialini had communicated observations of intermediate particles coming to rest in the emulsion. Some of the intermediate particles generated 'stars', that is, groups of track interpretable as fragments from a disintegrating nucleus, while others did not.[119] Relating these findings to the experiments of Rasetti and followers, Powell and Occhialini interpreted the star-ending tracks as evidence of the nuclear capture of negative mesons, and the starless tracks as accompanying evidence of the non-capture of positive mesons, in accord with the Tomonaga–Araki prediction. The authors stressed the difficulty of measuring the masses of intermediate particles with sufficient precision; nevertheless, they found that the data gave no basis for questioning the assumption of a single mass. Once again—but for probably the last time—the coherence afforded by the nuclear connection was used to reduce the experimental uncertainty surrounding mass measurements. In May, Powell and Occhialini, in collaboration with C. M. G. Lattes and H. Muirhead, published two pictures, each showing a secondary meson track emitted from the end of a primary meson track. They saw no other way to interpret these images but as a demonstration of the existence of mesons of different masses transforming into one another. The authors scrupulously emphasized that measuring the masses with precision was as difficult as ever. No mass difference between the two tracks was detectable by the method employed to distinguish intermediate particles from heavy particles. That the two mesons had to have different masses was deduced from the principle of conservation of energy, on the assumption that the secondary meson was ejected from a nucleus that had captured the primary meson. The Bristol experimenters knew of only one theory admitting the existence of mesons of different masses, the two-meson theory proposed by Schwinger in 1942. It was this theory—not the

[116] This was the hypothesis discussed at Shelter Island and then developed by Marshak in collaboration with Bethe, as reported in R. E. Marshak and H. A. Bethe, 'On the Two-Meson Hypothesis', *Physical Review*, 72 (1947), 509 and footnote 18. For an example of how the β-decay of the heavy meson was left out of later accounts, see Robert E. Marshak, 'The Multiplicity of Particles', *Scientific American*, 186 (1953), 22–7 (p. 25).

[117] C. M. G. Lattes et al., 'Processes Involving Charged Mesons', *Nature*, 159 (1947), 694–7.

[118] The interpretive practices of the nuclear emulsion specialists are discussed in Galison, *Image and Logic*, 196–210. Galison concentrates on the creation of a 'visual language' through interaction between the instrumental images and the observers' 'tutored eye'. Here, I only wish to emphasize the dialogical relation between these experiments and other experiments and theoretical analyses.

[119] G. P. S. Occhialini and C. F. Powell, 'Nuclear Disintegrations Produced by Slow Charged Particles of Small Mass', *Nature*, 159 (1947), 186–90. The first picture of a meson-induced star had been published shortly before by Donald H. Perkins, of Imperial College, London, who had had his nuclear emulsion plates flown at 9000 m of altitude by an RAF airplane. (D. H. Perkins, 'Nuclear Disintegration by Meson Capture', *Nature*, 159 (1947), 126–7.)

data—that suggested to the experimenters the possibility that the meson-to-meson transformation was a spontaneous decay rather than a nuclear reaction. Perhaps, it is also not entirely coincidental that they had now learned that nuclear capture of mesons at rest was much less likely than expected. Quoting the Rome result and the analyses by Fermi et al. and by Wheeler, the Bristol experimenters suggested that their evidence of a hitherto unknown mode of decay might contribute to the solution of the meson 'difficulties'.[120]

Marshak was informed of the Bristol paper after leaving Shelter Island. He recounted being 'immediately convinced' that the two meson-to-meson events unequivocally supported his two-meson hypothesis.

> I decided to enlist Bethe's help in writing the paper because of his extensive knowledge of the cosmic-ray data. Our paper was sent to the *Physical Review* during July.[121]

Bethe, who less than a year before had so disparaged the idea of particles having a spread of masses, was then persuaded to co-author an article proposing the existence of two mesons. Maybe he experienced a little paradigm shift when he saw the 'two excellent photographs' published by the Bristol group. Or, perhaps, considering multiple mesons in the light of different interaction and decay properties, he felt that beneath the untidy scattering of masses, some underlying structure was waiting to be uncovered. Still, he felt it appropriate to gloss, 'Of course, many more experiments must be performed before the existence of the heavy meson and, in particular, the proposed identification can be accepted'.[122]

9. Epilogue

The newly found heavy mesons, soon named π-mesons by the Bristol researchers,[123] began to be 'artificially' produced in the 184-inch Berkeley cyclotron, where they were first detected in 1948. They were observed to be often associated with nuclear disintegrations at the end of their tracks in materials of any atomic number. As for the light mesons, now called μ-mesons, it was confirmed that they were the decay product of the heavier ones, that they decayed into electrons and something invisible, and that they were sporadically captured by nuclei but did not produce nuclear disintegrations. It became apparent that the π-mesons were better candidates to participate in nuclear structure than the μ-mesons. Nevertheless, since they decayed into μ-mesons, they could not be responsible for β-decay. The unavoidable outcome of this state of affairs was that a unified theory of nuclear forces was no longer sustainable. The original meson theory could be salvaged only by amputating one-half of it, and re-defining as 'Yukawa's theory' the remaining half. It became customary to recall that Yukawa had predicted the existence of particles of intermediate mass as the heavy quanta responsible for the short-range force that held nuclei together. It was only by a twist of fate that the wrong intermediate particles had been discovered first, and had been for some time mistakenly associated

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[120] Lattes et al., 'Processes Involving Charged Mesons', 696 (note 117).
[121] Marshak, 'Particle physics in rapid transition', 382 (note 56).
[122] Marshak and Bethe, 'On the Two-Meson Hypothesis', 507 (note 116).
[123] C. M. G. Lattes, G. P. S. Occhialini, and C. F. Powell, 'Observation of the Tracks of Slow Mesons in Photographic Emulsions', *Nature*, 160 (1947), 453–6.

with meson theory. The Conversi–Pancini–Piccioni experiment, followed by the discovery of the π-meson, set things straight. 'Yukawa's conjecture was fully confirmed, but only after more than 10 years',[124] wrote Murray Gell-Mann and E. P. Rosenbaum in 1957. And according to Luis W. Alvarez, 'Powell and his collaborators discovered in 1947 a singly charged particle (now known as the pion) that fulfilled Yukawa's prediction, and that decayed into the "mesotron", now known as the muon. Sanity was restored to particle physics'.[125]

Yet, Yukawa's theory intrinsically entailed the prediction that the heavy quantum would decay into electron–neutrino pairs. That prediction was just as novel as the prediction that intermediate particles existed, and it directed physicists to observe for the first time the spontaneous decay of a particle. For this reason, mesotron disintegrations were long regarded as validating Yukawa's theory more securely than the highly uncertain value of the mass. The empirical validation meant the adoption of the nuclear connection as a working hypothesis, not a certified fact. The mounting difficulties of extracting from the theory quantitative predictions in 'reasonable agreement' with the experimental numbers had led theorists to question the correctness of both the theory itself and the nuclear connection. But from the experimentalists' point of view, the nuclear connection had continued to provide directions to new phenomena with impressive, albeit imprecise, predictive power. It was the unity and coherence of the explanatory framework for nuclear and mesotron phenomena that had constituted the theory's enduring appeal.

During the 1950s, when the distinction between strong and weak force was becoming established, some editing took place in physicists' narrative of their recent past. The identification of the penetrating particles with the heavy quanta was remembered as a naïve mistake induced by inexperienced views about the possible existence of new particles, while the decay association was effectively obliterated. The erasure left no track, for it was possible to extract both the notion and the observation of meson decay from the perimeter of Yukawa's theory. From the theoretical point of view, the spontaneous decay of particles could be recast as having belonged all along in a more general level of theory. From the experimental point of view, the causal explanation of instrumental outputs in terms of decay required a theory of electromagnetic interactions of particles in matter (and a theory of time dilation for particles in motion) but not a theory of decay. The cost of this process was explanatory coherence. The process was favoured by the milieu of post-war physics, in which quantitative representations of separate phenomena became more urgent desiderata than a qualitatively satisfactory synthesis.

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[124] Murray Gell-Mann and E. P. Rosenbaum, 'Elementary Particles', *Scientific American*, July (1957), 72–88 (p. 78).

[125] Luis W. Alvarez, 'Recent Developments in Particle Physics. Nobel Lecture, December 11, 1968', in *Evolution of Particle Physics*, edited by M. Conversi (New York, 1970), 1–49 (p. 2).

and Michelangelo De Maria, the curators of the Marcello Conversi and Edoardo Amaldi Archives at the Istituto di Fisica of the University 'La Sapienza' of Rome, for their work and hospitality. I owe special thanks to Bruno Ferretti, who shared with me his recollections and offered useful references. Thank you to an anonymous referee for helpful criticism. This work was made possible by the financial support of the Social Sciences and Humanities Research Council of Canada.