

# The Discovery of the Muon and the Failed Revolution against Quantum Electrodynamics

by

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- I Introduction
- II Birth Cries and Electromagnetic Cosmic Rays
- III Photons against Corpuscles
- IV Quantum Theories and Corpuscular Cosmic Rays:  
The Revolution against Quantum Electrodynamics
- V The Discovery of the Muon and the Resurrection of  
Quantum Electrodynamics
- VI Conclusion: Persuasive Evidence
- VII Epilogue

## *I. Introduction*

When was the muon discovered? According to G. Bernardini, “the mu meson as a peculiar ionizing fraction of the bulk of cosmic rays was revealed by an experiment by Bothe and Kolhörster in 1929.”<sup>1</sup> John Wheeler, however, considered that the theoretical work of Niels Bohr and E. J. Williams, together with the experimental work of C. D. Anderson and S. Neddermeyer, “established the existence of the muon” in 1936.<sup>2</sup> By contrast, Bruno Rossi spoke of “the discovery of the  $\mu$ -meson in 1937” by Anderson and Neddermeyer and J. C. Street and E. C. Stevenson.<sup>3</sup> Street himself credited J. F. Carlson and Robert Oppenheimer in 1937 as first arguing for the necessity of the existence of a new particle of mass intermediate between that of the proton and the electron.<sup>4</sup> Finally, A. Pais began an article on the development of

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particle physics by stating that the mu meson was discovered by C. F. Powell in 1947.<sup>5</sup>

The discrepancy of over eighteen years in these dates indicates some of the problems implicit in looking back at past experiments. It is often impossible to determine in retrospect which experiment “demonstrated” the existence of a new particle or effect. A more interesting question is what kind of evidence *at the time* convinced the experimentalists that they were looking at a real effect and not at an artifact of the machines or the environment. In other words, we want to know on what grounds the experimentalists decided to *end* their experiments.

In the case of the muon we need to consider two very different, and often competing lines of thought on the nature of cosmic ray research in the early 1930s. One line was pursued by R. A. Millikan, Anderson, and some of their colleagues and students, principally at the California Institute of Technology. The other line was developed on the theoretical side, *inter alia*, by Rossi, Street, and A. H. Compton. Only by understanding the experimental and theoretical framework within which the two groups were working can we understand how Anderson and Street eventually came to believe that a new particle needed to be admitted to physics, and, at the same time, rescued quantum electrodynamics from one of its first serious crises.

## II. *Birth Cries and Electromagnetic Cosmic Rays*

Carl Anderson was Robert Millikan’s Ph.D. student at CalTech while Millikan was actively pursuing his polemical campaign on the origin of cosmic rays.<sup>6</sup> After the younger physicist took his degree the two continued to collaborate on a series of cosmic ray projects. In retrospect many of Millikan’s ideas on the subject seem eccentric. Nonetheless they served to establish the theoretical and experimental ground rules under which Anderson began his career. Because these assumptions profoundly shaped the type of apparatus and demonstration Anderson used in his discovery of the positron and the muon, we need to understand Millikan’s program.

During the decade before his work with Anderson, Millikan had developed several guiding principles for the study of cosmic rays.<sup>7</sup>

First, he concluded that cosmic particles were  $\gamma$  rays entering the atmosphere isotropically from space. Generalizing from others' X-ray studies Millikan believed he could measure the rays' energy by studying their absorption. At the time, two absorption processes were known: Compton scattering and ionization. The rates of both, Millikan assumed, depended only on the photon energy and the density of the matter,  $\rho$ . The differential change in intensity  $I$  for rays of energy  $E$  could be compactly written:

$$\begin{aligned} dI &= -I(x)\mu dx \\ I(x) &= I_0 e^{-\mu x} \end{aligned}$$

where  $I_0$  is the intensity at the surface of the absorber. The strategy for measuring the cosmic ray energy was this: plot ionization rates measured in an electroscope against absorber thickness and fit an exponential curve to the measured one. The best fitting  $\mu$  would then reveal the photons' energy.

From bounds on cosmic ray energies obtained in this way Millikan began to speculate on the origin of the cosmic rays. Eddington and Jeans<sup>8</sup> had earlier proposed that protons and electrons might annihilate each other in the stars, producing very hard  $\gamma$ -rays which would radiate outwards.<sup>9</sup> In 1926 Millikan and Cameron objected to this because the rays would be too hard, instead suggesting that nuclear changes were "going on not in the stars but in the nebulous matter in space, i.e., throughout the depths of the universe."<sup>10</sup> The changes they had in mind would be either "(1) the capture of an electron by the nucleus of a light atom, (2) the formation of helium out of hydrogen, or (3) some new type of nuclear change, such as the condensation of radiation into atoms."<sup>11</sup> All of these hypotheses had in common the formation of organized states of matter out of chaotic ones. Though in this paper the idea is still relatively undeveloped, it is an early statement of what Millikan soon called his atom-building hypothesis, or the "Birth Cry of Atoms."

With specially designed electroscopes, Millikan and Cameron continued absorption studies by measuring the ionization rate as a function of depth in various lakes. This time<sup>12</sup> they had enough faith in the accuracy of their absorption curve to use it to determine a detailed spectrum of absorption constants employing the same theoretical model of absorption they had expounded in 1926. Millikan's new

confidence in fitting the parameters to fit the absorption curve was, he claimed, justified by the smoothness of the ionization-depth readings.<sup>13</sup> From various pieces of the ionization curve, the authors then obtained absorption constants by fitting exponential curves to each segment of the curve at one meter intervals.<sup>14</sup>

The two experimentalists chose three coefficients to be representative of the three simple exponential absorption curves they claimed composed the observed curve. According to the authors, the fact that the "mean drops suddenly to 0.11," at 11 meters, "obviously means that the cosmic rays are not at all continuously distributed between  $\mu = 0.2$  and  $\mu = 0.7$ ."<sup>15</sup> (See Table 1.) This was (to say the least) quite dubious, because there is no unique set of coefficients that will fit the curve. By judiciously choosing segments of this continuous absorption curve, one could find almost any average slope and therefore conclude that it was composed of any number of elementary exponential curves with similarly arbitrary values of  $\mu$ .

Table 1. Evidence for band theory. Depth below surface of atmosphere in "equivalent" meters of water versus best fitting absorption coefficient. Source: Millikan and Cameron, "Bands" (Ref. 12), p. 926.

Depth in meters of water beneath top of atmosphere	Absorption Coefficient $\mu$	Depth in meters of water beneath top of atmosphere	Absorption Coefficient $\mu$
8.45- 9.5	0.22	15-20	0.065
9.5-10.5	0.20	20-30	0.057
10.5-11.5	0.11	30-40	0.05
11.5-12.5	0.09	40-50	
12.5 -15	0.07	50-60	

According to Millikan and Cameron, their results were analyzed and presented on 16 February 1928, "entirely without the guidance of any theory."<sup>16</sup> Only then, according to the authors,

[A]fter we had made the foregoing empirical analysis, prepared the foregoing paper . . . and presented the results in detail to the physics seminar at the Norman Bridge Laboratory, our minds being up to this time completely unbiased by any knowledge as to whether bands might be expected, or if so where they might occur, we set at the task of seeing whether we could find any theoretical justification for their existence, or for their energy values.<sup>17</sup>



Millikan presented his theoretical justification on 23 April 1928<sup>18</sup> and elaborated on it in the *Physical Review*.<sup>19</sup> Taking Aston's measurements of the mass defect of various atoms (i.e., the difference between the mass of an atom and the mass of the number of hydrogen atoms supposed to constitute the atom), he calculated the energy released by the formation of nuclei by using Einstein's equation,  $E = mc^2$ . Then, using  $E = h\nu$ , Millikan determined the energy of the photon released in such "atom-building" (here  $m$  is the mass defect,  $h$  is Planck's constant, and  $\nu$  is the frequency of the light emitted). These photons constituted the primary cosmic rays; any other particles, he contended, were due only to secondary production in the earth's atmosphere. Proof of the fusion process consisted, in 1928, in showing that (1) the photons of these specific energies would have just the absorption coefficients found in the ionization/depth experiments, (2) that the cosmic rays were not associated with the sun or other stars, and (3) that atomic *disintegrations* provided photons of too little penetrating power to explain experiments.

On the first point, Millikan and Cameron found an extraordinary agreement between theory and experiment.<sup>20</sup> For the production of oxygen, nitrogen, helium and silicon (these being the most abundant elements on earth added to the most abundant elements in space), the authors found the following correspondence of theory to experiment:

Table 2. Atom-building processes. Millikan's theoretical and experimental values for the absorption coefficient of gamma rays. Source: Millikan and Cameron, "Origin" (Ref. 16), pp. 540–546.

Atom-Building Process	Absorption Coefficient per Meter of Water	
	Theory	Experiment
Oxygen and nitrogen produced by the fusion of hydrogen	0.08	0.08
Helium produced by hydrogen	0.30	{ 0.35 (lakes and atmosphere) 0.30 (lakes alone)
Silicon produced by hydrogen	0.041	0.04
Iron produced by hydrogen	0.019	("not inconsistent" with experiment)

Using the experimentally-determined coefficients of absorption in conjunction with the then-current spectroscopic data for the relative presence of the elements outside the earth and its atmosphere, the authors plotted the absorption curve they would predict for the resultant cosmic ray photons, and compared it with experiment. Again, the agreement was superb.

With hindsight, we may note that the two physicists used the wrong particle (the photon) produced in a process which does not occur (atom-building of nitrogen, oxygen, etc.), and then invoked an absorption law which is also incorrect (ignores pair production, electron binding effects, etc.). Nonetheless, Millikan and Cameron were able three times to produce a match between their theory and their experimental data (reduced in a quite arbitrary fashion) to one part in a hundred.

Soon after the Millikan and Cameron paper appeared in print, Millikan received what must have been a distressing letter from J. Robert Oppenheimer, then in Zurich. Oppenheimer, undoubtedly viewing Millikan's fast and loose atomic physics with some scepticism, pointed out that the highly touted numerical confirmation of the band measurements was spurious. He wrote,

Last year, when you were working on the interpretation of the absorption curves of the cosmic radiation, you asked me with what certainty the formulae of Dirac could be accepted. I answered, I think, that they could be taken as reliable, and that they could not be appreciably altered except by a fundamental change in the equations of physics. As you surely know, I was wrong to insist upon this reliability; the fundamental equations of the theory have in fact been altered; and there is a corresponding change for the absorption coefficient of hard radiation. The new formulae have been worked out by Klein and Nishina, and shown, in the region in which you are interested, to give an absorption differing by as much as fifty percent from that calculated on the older basis.<sup>21</sup>

Oppenheimer went on to warn Millikan that even the new formula referred only to the scattering of light from free electrons and might well require modification when one included nuclear effects. Once these nuclear effects were considered, he added, an extra-nuclear electron attached to a lead nucleus could behave quite differently from an extra-nuclear electron associated with the nuclei found in air. Oppenheimer's bad news hardly dissuaded Millikan from his theory.

I am, [he replied to Oppenheimer] of course a little disappointed that the Dirac formula,

which actually fits so well, has not the credentials which we thought it had a year ago. The quantitative fit, however, is only a part of the agreement, so that I do not think that the interpretations which I have given are as yet ready for the discard. There is no other interpretation that I can see for the sequence of frequencies which we observe quite independently of whether there is an exact quantitative fit as to the numerical values or not.<sup>22</sup>

Millikan continued in 1930 to try to match the data to the Klein-Nishina formula.<sup>23</sup>

Millikan and Cameron's 1926 paper was an early presentation of a theme Millikan would pursue vigorously for many years. Why was this theory, which almost no one accepted outside his immediate circle of students and colleagues, so attractive to Millikan? Several factors were undoubtedly at work. Robert Kargon has shown how Millikan's earlier interest in the structure of the nucleus and the transformation of elements played a role in the development of the Birth Cry theory.<sup>24</sup> Robert Seidel argued that during the 1920s and early 1930s, Millikan's dramatic claims served as an enticement to the foundations in their support of cosmic ray research.<sup>25</sup>

Yet another factor that must be taken into account regards the connection between Millikan's religious views and his theory of the origin of the elements. Like many American physicists of the time, such as Arthur Compton, Henry Rowland, and Edwin Kemble, Millikan was the son of a protestant minister. Indeed, Millikan considered his religious upbringing crucial for his later life and for the remainder of his days the attempt to reconcile God and science was a recurring theme in his writing. In his words, "The first fact which seems to me altogether obvious and undisputed by thoughtful men is that there is actually no conflict whatever between science and religion when each is correctly understood."<sup>26</sup>

Typical of Millikan's pronouncements on these matters is an excerpt from his book, *Science and Life*, where he asserted:

*There have been two great influences in the history of the world which have made goodness the outstanding characteristic in the conception of God. The first influence was Jesus of Nazareth; the second influence has been growth of modern science, and particularly the growth of the theory of evolution.*<sup>27</sup>

"Evolution" consequently occupied a singular place in Millikan's thought. By this term, Millikan understood not just the variation and

selection of Darwinian evolution, but the more general concept of progress in the world, as applicable to the inorganic as to the organic world. Thus, while Darwin was elevated to an exalted role as the discoverer of progressive change in species, Röntgen, Curie, and Becquerel were credited with the discovery of the evolution of the elements.<sup>28</sup> For Millikan, the importance of the discovery of radioactive decay was that it strongly suggested that the inverse process was occurring somewhere in the universe, preventing a "heat death." In 1921, Millikan had written: "[W]ith radium and with uranium we do not see anything but decay. And yet somewhere, somehow, it is almost certain that these elements must be continually forming. They are probably being put together now somewhere in the laboratories of the stars."<sup>29</sup> For, as he repeatedly argued, just as God intervenes in the process of the evolution of animals, so he does too in the evolution of the elements. Together, inorganic and organic evolution help usher in the highest stage in religious thought, for they contribute to the "conception of progress [which] has entered the world."<sup>30</sup>

Once Millikan and Cameron "established" the energy spectrum of the cosmic rays, they turned in 1927 and 1928 to a modification of their quantitative methods. Millikan likened this stage of the research to his earlier refinements of his determination of the charge of the electron in his oil-drop experiments.<sup>31</sup> In part, Millikan certainly was referring to the greater accuracy he claimed for each successive experiment. But the analogy to the oil-drop experiment probably goes deeper: Gerald Holton<sup>32</sup> has shown in his analysis of the Millikan-Ehrenhaft dispute that Millikan's oil-drop experiment involved a debate not only about technical questions regarding electric charge, but also about methodological and philosophical problems. Millikan supported atomic theory and in general a granular representation of nature. By contrast, Felix Ehrenhaft, then an Associate Professor at the University of Vienna, became increasingly antagonistic to atomic theory.<sup>33</sup> Along with this division lay another: Millikan spoke of "seeing" the electrons, his approach was pragmatic and his philosophy one of realism. Ehrenhaft, following Mach and Lampa, left the "reality" of electrons aside, preferring hypotheses to be judged by their predictive value alone.

Millikan's methodological presuppositions had direct consequences for his experimental physics. By the strength of his conviction, he set

aside many measurements not in accord with his atomistic hypothesis about electric charge. Others, working under different presuppositions, might have regarded these measurements as providing reliable data. In retrospect some of Millikan's data selection seems questionable. But had Millikan included all his measurements, he would have been confronted (like Ehrenhaft) with an undifferentiated mass of mostly invalid data with every conceivable value of  $e$ .

Given his earlier success, we can understand Millikan's fascination with discrete bands of cosmic ray energies. His remark that he was now approaching a stage of cosmic-ray work comparable with the later oil-drop experiments may well refer to a stage of experimentation where he expected the discreteness of the bands to emerge from the background just as the atomistic charge had become clear seventeen years earlier. Moreover, in Millikan's early methodological precepts we may find the clue for his support of an easily visualizable, intuitive model of the nucleus, as well as his antipathy for the highly abstract and seemingly idealistic theory of quantum mechanics with its wave functions and non-commuting algebra. Such considerations must have seemed a long way from the careful and pragmatic program Millikan had set out for himself and his colleagues.

The confluence of practical, religious, methodological, and scientific interests wed Millikan inseparably to the basic tenets of his "birth-cry" theory. But already opposition was mounting. One person whom Millikan could enlist into immediate defensive service was his Ph.D. student, Carl Anderson.

### *III. Photons against Corpuscles*

Anderson had been both an undergraduate and graduate student at CalTech, completing his doctoral dissertation in 1927 on the space-distribution and energy of  $X$ -ray photo-electrons using a cloud chamber. In 1930, Millikan suggested that Anderson undertake a study of the energy of corpuscular radiation emitted by the primary photons using the cloud-chamber techniques with which Anderson was already familiar.<sup>34</sup> These experiments, Millikan hoped, would provide better data on the primary energy of the photons than could be gathered from the absorption experiments; for, as Millikan by then

knew from Oppenheimer's letter, the relation between incident photon energy and absorption was not at all clear. Millikan must have expected that the secondary electrons which Anderson would observe in the cloud-chamber experiments would exhibit the supposed band structure of the primary cosmic-ray photons.<sup>35</sup>

Anderson's photographs soon brought unexpected results. Among the cosmic-ray cloud-chamber photographs he began to find positive particles. On 3 November 1931, he reported the findings to Millikan,<sup>36</sup> commenting that they showed the "presence of positive particles as well as electrons indicating nuclear disintegrations by cosmic rays." The positive particles were thought to be either  $\alpha$  particles or photons, and often to be simultaneously ejected from the nucleus with an electron. Finally, Anderson reported the "simultaneous ejection of three particles in at least one instance." Anderson concluded the letter by asserting that "A hundred questions concerning the details of these effects immediately come to mind . . . It promises to be a fruitful field and no doubt much information of a very fundamental character will come out of it."

The first paper published on these cloud-chamber experiments was a joint effort by Millikan and Anderson in 1932,<sup>37</sup> in which they discussed the positive particles found on cloud-chamber photographs. The authors interpreted these results as evidence of nuclear disintegrations, identifying the positive particles immediately with the proton. In this way, they introduced a new process by which radiation could be absorbed by matter in addition to Compton scattering and photoelectric emission. In part, Millikan's readiness to accept the role of nuclei in photo absorption processes might be due to the comments by Oppenheimer in his letter. Still, Millikan and Anderson's published discussion of the structure of the nucleus remained behind the times and non-quantum mechanical. Instead, Millikan maintained a *visualizable* concept of the nucleus: electrons, positrons, and protons bound together and occasionally released by a sufficiently energetic photon. If Millikan was antipathetic to the new quantum mechanical studies of the nucleus, he remained quite convinced of his own atom-building theory. Concluding the joint paper, the authors reiterated Millikan's 1926 claim, adding to it the nuclear disintegration they had observed: "In a word, then, on the assumption that the tracks are due in all cases either to protons or to electrons, *nine-tenths* of all



observed encounters yield energies which lie within the ranges computed from the Einstein equation and the atom-building hypothesis.”<sup>38</sup> So convinced was Millikan that his theory was correct that he speculated that even the “one-tenth” of the “secondary” protons and electrons above 216 MeV might well turn out to be only apparently so energetic, having been straightened out by turbulence in the cloud chamber.<sup>39</sup> In his recollections Anderson writes that he had (unsuccessfully) argued in favor of the existence of much more energetic particles.<sup>40</sup>

Anderson soon succeeded in obtaining clearer photographs by minimizing turbulence and improving the illumination in the chamber;<sup>41</sup> better measurements of curvature and ionization density therefore could be obtained. In this way, he calculated the mass of the positive particle to be the same as that of the electron.<sup>42</sup> Within a few months Anderson’s results were confirmed by P. M. S. Blackett and G. P. S. Occhialini.<sup>43</sup> The researchers from the Cavendish Laboratory argued that the positron fit naturally into Dirac’s pair-production scheme.

Anderson read and tentatively accepted Blackett and Occhialini’s account of the origin of positrons in pair production.<sup>44</sup> For this conclusion Anderson relied on the near equal numbers of observed positive and negative electrons. Still, a certain hat-tipping in Millikan’s direction was appropriate and Anderson concluded with encouraging words:

... The primary cosmic ray beam at sea level ... consist[s] in greater part of photons, a point of view held by Professor Millikan for several years, and now given additional support by the fact that hard gamma-rays of ThC’’ have been found to produce positrons as do the cosmic rays.<sup>45</sup>

As could be expected Millikan agreed, though with a very different interpretation. In 1933 he contended that “*both* (positive and negative) *tracks arise immediately from* the disintegration of the nucleus of an atom.”<sup>46</sup> No Dirac here. Instead Millikan simply cited Anderson’s energy measurements as “the most complete demonstration”<sup>47</sup> that primary cosmic rays were photons. (Anderson had shown that the vast majority of measured charged particles were below 600 million volts compatible with primary “birth cry” photons.)

On many fronts, then, Millikan’s theory seemed to embrace trium-



phantly the new discoveries, as well as the series of earlier successes. Yet another victory was claimed by Millikan when in 1931, he concluded a series of balloon tests with self-recording electroscopes; the highest flights had climbed to 16 km. This measurement, Millikan asserted in a speech to a Paris audience in November 1931, shows that the ionization rate reaches a maximum between 9 and 16 kilometers, which "is precisely what we would expect if the rays penetrating in the atmosphere were  $\gamma$ -rays which necessarily penetrated to a certain depth in the atmosphere before coming to equilibrium with their secondaries."<sup>48</sup>

There is a maximum of ionization in the upper atmosphere and part of Millikan's conclusion was ineluctable. If particles reach a maximum inside the atmosphere the primary rays must be producing secondary ones. However, (though this would be realized only several years later) the primaries were protons not photons. Bolstering Millikan's advocacy of the photon was another error: his group had failed to find the "latitude effect". Around the earth is a magnetic field that extends beyond the atmosphere into space. As a result, if the primary cosmic ray particles are charged then they will be deflected towards the poles and away from the equatorial latitudes. Conversely, if the primary particles were (neutral) photons, as Millikan believed, there should be no such geographical variation. Millikan and his colleagues made frequent attempts to test this "latitude effect" – for instance, by comparing flux rates in Churchill, Manitoba and Pasadena, California. By 1931, they had found "not a shred of evidence" for the latitude effect, and Millikan once again celebrated the success of the atom-building hypothesis. A. H. Compton did not.

Starting from an amicable relationship, Millikan and Compton entered into one of the most acrimonious and publicized scientific disputes of the century. It is evident from the Millikan archives that both men became personally involved, even casting aspersions on each other's scientific integrity. Obviously amused at the spectacle of two Nobel laureates engaged in such a "dogfight" (as Millikan sometimes referred to it), the press raised the issue to front-page news.<sup>50</sup> Paralleling Millikan and his colleagues' studies of the latitude effect, Compton collaborated with a group also conducting a large-scale geographical survey of cosmic ray intensity. They concluded, in a report to the *Physical Review*,<sup>51</sup> that the latitude effect indeed existed, and

sufficiently strongly to exclude Millikan's argument that all the charged particles seen at sea level were secondaries produced within the earth's atmosphere.<sup>52</sup>

The challenge to Millikan's theory presented by Compton was not the only difficulty Millikan faced. In 1929, W. Bothe and W. Kolhörster conducted an experiment with results that were potentially devastating to the "birth cry" theory.<sup>53</sup> Instead of focusing attention on the rate of discharge of an electroscope at different locations, the authors hoped to discover the nature of the rays themselves. To do this, they made use of the newly-developed Geiger-Müller tube, essentially a large cylindrical capacitor composed of a conducting hollow cylinder with a wire along its axis. The tube and the wire are then held at a constant, high potential difference. When a charged particle passes through the gas, it ionizes a few of the atoms. The ions, responding to the potential gradient between the center wire and the walls, then rapidly begin to migrate, ionizing some of the atoms they hit. As the ions cascade in this manner, a current flows, and the device discharges. The surge of current could then be exhibited, for example, by means of an electroscope.

Bothe and Kolhörster's plan was to use two such Geiger-Müller tubes, each attached to a separate electroscope, the two tubes separated by a block of gold. If the tubes discharged simultaneously more often than would be expected by random discharges, this would be evidence for the passage of a single charged particle through the intervening lead. The principal difficulty in drawing such a conclusion was the rather poor time resolution provided by the simple observation of two electroscopes. Nonetheless, Bothe and Kolhörster were convinced by their data that they had demonstrated that cosmic rays included charged particles.

Bruno Rossi, then working in Florence, concurred. Intrigued by Bothe and Kolhörster's work, Rossi worked to improve their experiment.<sup>54</sup> Rossi's ingenious contribution to technique was a vacuum tube circuit which would emit a pulse only when two or more other pulses were delivered to the circuit at the same time.<sup>55</sup> Such a device was precisely what was needed for the cosmic ray work; in modified form, the "coincidence circuit" has become one of the most widely used tools in experimental physics. Rossi wired three Geiger-Müller tubes to a coincidence circuit in such a way that only a charged particle

following a vertical path would discharge all three tubes and therefore cause a recording device to register the event. By inserting varying quantities of lead, he was able to reaffirm the Germans' results with considerably more certainty: some particles penetrated over a meter of lead.<sup>56</sup>

Both the motivating idea of corpuscular cosmic rays and the counter experiments that accompanied it were anathema to Millikan. "I have been pointing out for two years," he declaimed in February 1933, "that these counter experiments never in my judgment actually measure the absorption coefficient of anything."<sup>57</sup> Thus when Rossi's results appeared later that year Millikan was long ready to answer in print.

Anderson collaborated with Millikan in the composition of their December 1933 rebuttle to the counter/corpuscle program.<sup>58</sup> In his papers, Rossi had noted that secondary particles occurred in conjunction with the passing of a primary particle; unlike the primaries, the secondaries had a mean penetration of about one centimeter. (In retrospect, this fact could have provided the clue to the identification of the primary particles with a new type of particle and the secondaries with the electron. For a variety of reasons, this would take almost five more years.) Anderson and Millikan too had noted the occurrence of these showers of secondary particles, and with S. Neddermeyer (then a post-doctoral student) and W. Pickering (a graduate student) they used this fact in conjunction with an important experimental observation: the number of shower particles *increases* up to about one and one-half cm of lead. This is true; however they went on incorrectly to infer that the number of shower particles would increase indefinitely with the thickness of lead. Rossi's coincidences, the four authors concluded,

cannot in general be due to the passage of one charged particle through both counters and the intervening lead, but *must be due to some mechanism by which a photon can release successively along, or in the general neighborhood of, its path a number of different particles whose separate but practically simultaneous action on the two or more counters is responsible for the observed coincidences.*<sup>59</sup>

To argue for this position, the authors reiterated the kind of pre-quantum mechanical view of the constitution of the nucleus Millikan and Anderson had invoked many times before: positrons and elec-

trons were located in the nucleus and could be ejected by the impact of  $\gamma$ -rays. Their experiments militated against relativistic quantum mechanics as they claimed to have found many more ejected negatives than positives. This, they argued,

... seem[s] difficult to reconcile with the Dirac theory, as interpreted by Blackett and Occhialini, of the creation of electron-pairs out of the incident photons, and point[s] strongly to the existence of nuclear reactions of a type in which the nucleus plays a more active role than merely that of a catalyst.<sup>60</sup>

By espousing the Dirac theory, P. M. S. Blackett and G. P. S. Occhialini had doubly challenged Millikan and Anderson's cosmic ray program. Not only had Blackett and Occhialini interpreted their photographs of positrons as due to Dirac pair-production, their experimental apparatus made fundamental use of counters and coincidence circuits.<sup>61</sup> Pair creation jeopardized the fragile agreement Millikan had mustered between his experimental and theoretical band theory. Counter experiments threatened his contention that sea-level cosmic radiation was photonic.

The Anderson, Millikan, Neddermeyer, and Pickering challenge to the work of Rossi, Dirac, Blackett, and Occhialini in many ways represented the last stand of the Millikan theory, although Millikan himself would continue to hold to it with various modifications until his death. In their paper, the four authors reiterated one last time all the old themes Millikan had stressed for almost a decade – a non-quantum nucleus, nuclear electrons, and photons as the primary cosmic radiation. But the fortress was collapsing on many sides; within the next few months Dirac pair-production, the latitude effect, the very high energy electrons and the penetrating corpuscles were all widely accepted. Soon even Anderson would publically break with the predictions of Millikan's Birth Cry theory.

#### *IV. Quantum Theories and Corpuscular Cosmic Rays: The Revolution against Quantum Electrodynamics*

Millikan, his associates and students were not the only group interested in cosmic rays and related problems. In Europe physicists were deeply engaged in a very different style and type of research, both in theory and experiment. In particular, relativistic quantum mechanics claimed to explain the high energy behavior of charged particles. Among the participants were P. A. M. Dirac, W. Pauli, W. Heisenberg, Niels Bohr, M. Born, Hans Bethe, and others especially at Göttingen and Copenhagen. Because the problems of relativistic quantum mechanics often found their experimental tests in the behavior of high speed electrons and light, cosmic ray experiments became one of the proving grounds for the new physics, the other, of course, being spectroscopic data.

In 1936 W. Heisenberg and W. Pauli became engrossed in an attempt to develop a radical theory, introducing a fundamental length scale to physics. Their work led to a better understanding of H. Yukawa's<sup>62</sup> and E. Fermi's 1934 work on non-electromagnetic quantum forces. But despite their high hopes, their program remained outside the mainstream of quantum field theory and cosmic ray experimental work. An excellent historical study of this work exists<sup>63</sup> and for this reason it will not be treated here.

Instead, this section will focus on that part of quantum theory that bore directly on the particulate interpretation of cosmic ray experiments. In particular we trace a line of thought from Bohr to Bethe on the energy loss of fast heavy charged particles as they passed through matter. For it is this work that set the theoretical background to the crisis of quantum electrodynamics in the mid 1930s.

Fast heavy charged particles revealed the structure of the atom. It was with  $\alpha$  rays that Rutherford had attacked the nucleus. And in Rutherford's laboratory young Bohr set out a study of  $\alpha$ -ray stopping that led to his old quantum theory.<sup>64</sup> But Bohr's absorption studies, developed in 1913 and 1915, provided more than a stepping stone to the problem of atomic structure and spectra. Practically every attempt in the following years to identify new particles was predicated on the properties of charged particles passing through matter. As we will see this was pre-eminently so in the discovery of the muon.

Bohr developed a clear classical approximation scheme in which to calculate  $\alpha$ -particle energy loss.<sup>65</sup> Schematically, his analysis was as follows.<sup>66</sup> A charged particle scattering from a (much heavier) nucleus will change direction but lose little of its energy. Conversely, a charged particle scattering from an atomic electron will hardly be jarred from its original trajectory but will lose energy. Thus only collisions between the charged particle and atomic electrons need be considered.

Bohr's argument then divides into two parts. In one he shows that distant encounters between charged particles and atomic electrons involve only small transfers of energy. He justified this claim by Fourier analyzing the electric field of the projectile. He had thus reduced the problem to an exercise in classical electrodynamics: calculate the energy transfer of each plane wave component on a harmonically bound electron. Summing the contributions Bohr showed rigorously that the total energy transfer from *distant* collisions was small.

Next Bohr could analyze close encounters, in which he assumed the atomic electron could be considered as if free. This approximation was valid if the time of the projectile's passage was short compared to the electron's orbit time, i.e. if

$$b_{\max} \sim \frac{\alpha v}{\omega},$$

where  $\alpha \equiv (1-\beta^2)^{-1/2}$ ,  $\beta \equiv v/c$ ,  $c$  is the speed of light,  $v$  is the speed of the projectile,  $\omega$  is the orbital frequency of the electron, and  $b_{\max}$  is the outer bound of close encounters. Figure 1 illustrates the problem. The impact parameter  $b$  is the closest distance the projectile comes to the electron;  $e$ ,  $m$  are the charge and mass of the electron and  $ez$ ,  $M$ , and  $\vec{v}$  are the charge mass and velocity of the projectile.

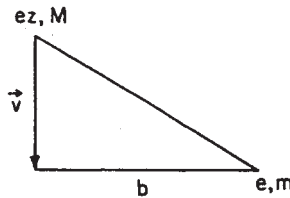


Figure 1. Close encounter between fast projectile and atomic electron at rest.



In such close encounters if the electron does not move too much during the projectile's passage, the time integrated momentum transfer to the electron along the direction of the projectile's motion will be zero. Thus the electron will be accelerated only by the electric field  $E_{\perp}$  perpendicular to the projectile's motion.  $E_{\perp}$  takes its maximal value when the particle is closest (a distance  $b$ ) from the electron. Thus

$$E_{\perp \max} \sim \frac{\gamma e Z}{b^2}.$$

We now approximate the time  $\Delta t$  during which the projectile effectively influences the electron by the time it takes the projectile to travel a distance  $b$ :

$$\Delta t \sim \frac{b}{v\gamma}.$$

Thus the momentum transfer  $\Delta p$  to the atomic electron is given by

$$\Delta p = \int_{-\infty}^{\infty} e E_{\perp}(t) dt \sim 2 e E_{\perp \max} \Delta t = \frac{2 z e^2}{b v}$$

whence 
$$\Delta E = \frac{(\Delta p)^2}{2 m} \sim \frac{2 z^2 e^4}{m v^2} \left( \frac{1}{b^2} \right).$$

This expression becomes infinite as  $b$  goes to zero. To avoid this we introduce a lower cutoff consistent with our approximation; in particular our approximation only holds good if the electron recoils much less than  $b$  in the time  $\Delta t$ . Thus if  $\frac{\Delta p}{2m}$  is the electron's average velocity during the collision and  $\Delta t \sim b/v\gamma$  is the time of the collision then,

$$b \gg \frac{\Delta p}{2m} \Delta t = \frac{z e^2}{\gamma m v^2} \equiv b_{\min}.$$

We therefore replace  $1/b^2$  by  $1/(b^2 + b_{\min}^2)$ ; this makes  $\Delta E$  finite. Finally, if  $N$  is the density of atoms and  $Z$  is the number of electrons per atom we can integrate over all allowable values of  $b$ .



$$\begin{aligned} \frac{-dE}{dx} &= 2\pi N Z \int_{b_{\min}}^{b_{\max}} \Delta E b db = \\ \frac{2\pi N Z z^2 e^4}{mv^2} \int_1^B \frac{1}{b} db &= \frac{4\pi N Z e^4 z^2}{mv^2} \ln B \end{aligned}$$

with 
$$B = \frac{b_{\max}}{b_{\min}} = \frac{\gamma^2 mv^3}{z e^2 \omega}.$$

In fact Bohr's more careful analysis differs from the simplified version just given only by small corrections. Some years later (1925) a result identical to Bohr's was obtained by R. H. Fowler<sup>68</sup> who replaced the oscillating electron with an orbiting one. The more sophisticated atomic model of Bohr's old quantum theory led to a (mistakenly) smaller stopping power when G. H. Henderson<sup>69</sup> proposed that the electron should be allowed to receive only discrete quantities of energy. If the classical transfer fell between two allowed energy transfers only the smaller energy was absorbed by the electron. Therefore the remaining classical energy was simply ignored. Such an unsatisfactory proposal could be reasonably jettisoned only after the development of the new quantum theory in 1926.

With these new theoretical tools J. A. Gaunt<sup>70</sup> reworked the problem, treating the projectile classically and the atom quantum mechanically. But why not treat the whole system (projectile and atom) as quantum mechanical?

There were two reasons a *fully* quantum mechanical treatment of energy loss was needed. First, if the projectile was to have a definite momentum (necessary to calculate a meaningful energy loss) it could not have a definite position. Therefore the impact parameter could not validly be used to describe the collision. Second, any process, including the energy transfer between the projectile and the electron is understood in quantum mechanics to take place in discrete jumps but averaged statistically over many collisions. To handle both of these difficulties required a mastery of both the newly developed approximation techniques of Born, Fermi and others *and* a familiarity with what was then known of quantum electrodynamics. Bethe had both.

In fact the problem of the passage of electrons through matter had dominated Bethe's theoretical work from his very first contact with physics. In 1926 when he approached A. Sommerfeld for a problem to work on, Bethe was assigned the task of accounting for some anomalies in the electron diffraction in crystals.<sup>71</sup> To solve it, Sommerfeld suggested that Bethe look for analogies with the diffraction of *X*-rays by crystals. Sommerfeld's advice could not have been more helpful; the pursuit of analogies between electron and light scattering became a hallmark of Bethe's work for the next decade.

After the wave-mechanical exercise, Bethe turned to the more complete quantum mechanical analysis of the problem for his doctoral dissertation. For this project, Bethe again looked to the diffraction of *X*-rays by crystals for analogical guidance, especially to the work of Paul Ewald's treatment of *X*-ray scattering. When he had taken his degree, Bethe went to Frankfurt and then on to Stuttgart where Ewald was a professor of theoretical physics. There he began work on what he later considered his best work,<sup>72</sup> "Towards a Theory of the Passage of Fast Corpuscular Radiation through Matter".<sup>73</sup>

In this paper and the relativistic follow-up paper of 1934 Bethe applied the Born approximation to the Schrödinger equation to study the effect of atomic electromagnetic potentials on the passing electrons. As in his pre-thesis and thesis research, Bethe displayed the close analogy between a charged projectile and light scattering from matter. The parallel is as follows:<sup>74</sup>

Charged Particle Scattering	Light Scattering
<i>Elastic Scattering</i>	<i>Coherent Scattering</i>
a) change in projectile's direction without significant change in speed	a) no significant change in photon wavelength
b) no change in excitation of atomic electrons	b) stationary target particles
c) interference important	c) interference important
<i>Inelastic Scattering of Projectile</i>	<i>Incoherent Scattering of Photon</i>
a) small change in projectile's direction with significant change of projectile's speed	a) small change in wave (photon) direction with significant change in wavelength
b) excitation or ionization of target atom	b) acceleration of target atom (Raman or Compton scattering)
c) projectile wave inference not important	c) electromagnetic interference not important
<i>Bremsstrahlung</i>	<i>Photoelectric Effect</i>
slowing of electron in atomic field with emission of radiation	absorption of photon with excitation of electron from atom

Bethe brought these parallels out in his calculation of the cross sections.

Soon after Bethe's work was submitted, in the fall of 1930, he went to Cambridge, England, where he discussed the problem with P. M. S. Blackett. Blackett, by then immersed in cosmic ray experiments, encouraged him to calculate a range-energy relation for electrons for comparison with observation.<sup>75</sup> The next year Fermi brought Bethe to Rome for several months where he extended his energy loss work to the relativistic case.<sup>76</sup>

Simultaneously E. J. Williams and C. F. von Weizsäcker<sup>77</sup> pushed Bohr's old impact parameter approach to the limit. Like Gaunt they approximated by treating the projectile classically, but by simplifying the exposition many physical features of the scattering process became clearer. Since their results (in the low-momentum transfer limit)

agreed with Bethe's rigorous calculation the theoretical community looked forward to quick experimental confirmation.

The experimentalists were not of one mind. Bethe attended a Milikan "Birth Cry" lecture in Munich that "quite evidently did not make any sense."<sup>78</sup> More promising, he felt, were new experiments conducted by Bruno Rossi using Geiger's ionization counters. At Rossi's request Bethe joined him in Florence to discuss recent experimental and theoretical developments in cosmic ray physics.<sup>79</sup>

By late 1932 Bethe had found a job in Tübingen. But as elsewhere in Germany the Nazis imposed their presence through ever more frequent marches and protests. Shortly after the Nuremberg race laws of April 1933 excluded Jews from state jobs, Bethe decided to emigrate,<sup>80</sup> leaving for an opening at the University of Manchester.

From Manchester, Bethe frequently went to Cambridge, where he participated in monthly physics seminars with Blackett, J. D. Cockcroft, R. Peierls, W. Heitler, and others.<sup>81</sup> It was in this series of talks that Heitler presented his work on the stopping of fast particles in matter, making use for the first time of the cross section of the Dirac pair creation process.<sup>82</sup> Surprisingly, Heitler found the cross section to *increase* (logarithmically) with energy. Such behavior could not continue unchecked or the probability of interaction would become infinite as one considered higher and higher energies. "Naturally," Heitler wrote Bohr, "this shows that for very high energies the theory becomes false."<sup>83</sup>

Falsification threatened as well from the experimental quarter. After Heitler used his cross section to calculate the energy loss per centimeter he argued that

The theory seems to be here in disagreement with experiment. On the other hand, perhaps one should not expect the theory to give correct results for energies greater than  $137 mc^2$ , since the wavelength for them becomes smaller than the classical electron radius  $e^2/mc^2$  and Dirac's wave equation probably no longer applies.<sup>84</sup>

When Bethe heard Heitler's gloomy conclusions in an oral presentation, he began to wonder if both the energy loss discrepancy with cosmic ray experiments and the increasing cross section might be accounted for by taking into consideration the screening effect inner electrons would have on the nuclear electromagnetic field.<sup>85</sup> Together in late February 1934, Bethe and Heitler submitted their calculation

of the energy loss of a charged particle as it passed through matter including the effects of screening and pair creation.<sup>86</sup> Their first-order calculation avoided discussion of higher-order infinities, and was typical of the first-order, relativistically correct approximations that characterized quantum electrodynamics before its reformulation in the 1940s by Richard Feynman, Julian Schwinger, and Freeman Dyson. The authors reasoned as follows:

A particle of momentum  $\mathbf{p}_0/c$  and energy  $E_0$  makes a transition to a momentum  $\mathbf{p}/c$  and energy  $E$ . A light quantum is emitted of frequency  $\nu$  such that  $h\nu = E_0 - E$ . The perturbing potential consists of an electrostatic part  $V = Ze^2/r$  and a magnetic part,  $H = -e\boldsymbol{\alpha} \cdot \mathbf{A}$ , where  $\boldsymbol{\alpha}$  is the Dirac matrix vector and  $\mathbf{A}$  the magnetic potential. There are, in all cases, two events involving the electron: a photon is emitted, and the electron interacts with the nuclear field. Between these two events the electron is in an intermediate state. There are two possible such states: either (1) no light quantum is yet present in which case the electron's intermediate momentum is  $\mathbf{p}'/c = \mathbf{p}/c + \mathbf{k}/c$ , or (2) a light quantum of momentum  $\mathbf{k}/c$  is present and the electron's intermediate momentum is  $\mathbf{p}''/c = \mathbf{p}_0/c - \mathbf{k}/c$ . Then Fermi's golden rule gives the rate of the combined process as:

$$W = \frac{2\pi}{h} \rho_E \rho_K \left| \sum \frac{H_{EI} V_{IA}}{E' - E_0} + \sum \frac{V_{EI} H_{IIA}}{E'' - E} \right|^2$$

where  $\rho_E$ ,  $\rho_K$  are the density of final states.

$A$  is the initial state of electron with momentum  $\mathbf{p}_0/c$ . No light quantum is present.

$E$  is the final state of electron with momentum  $\mathbf{p}/c$ . A light quantum is present.

$I$  is intermediate state with no light quantum present.

$II$  is intermediate state with light quantum present.

when the matrix elements are inserted for a coulomb field, cross sections and thus energy loss can be derived.

In modern terms, the two terms correspond to the two Feynman diagrams (Figure 2). Comparing the result with Bethe's earlier work, the authors could show that radiative and collision energy losses for

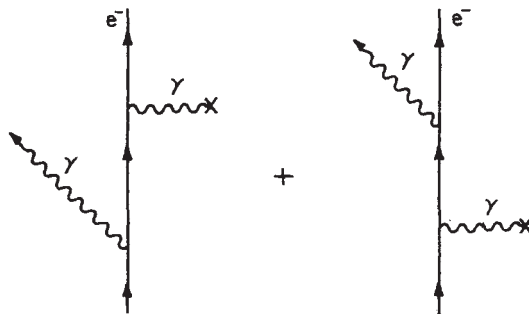


Figure 2. Feynman diagrams representing the two ways in which an electron can scatter from a potential and radiate a photon.

lead became equal for electron energies of about 10 MeV. In general, they found:

$$\frac{(dE/dx \text{ due to Bremsstrahlung})}{(dE/dx \text{ due to ionization and excitation})} = E_0 Z / (1600 mc^2)$$

Unfortunately, Bethe and Heitler's rigorous treatment showed that theory still disagreed with experiment. Indeed, the first sentence of the section on comparison of the theory with experiments was underlined, "*The theoretical energy loss by radiation for high initial energy is far too large to be in any way reconcilable with the experiments of Anderson.*"<sup>87</sup> By way of explanation, the authors offered the following:

The de Broglie wavelength of an electron having an energy greater than  $137 mc^2$  is smaller than the classical radius of the electron,  $r_0 = e^2/mc^2$ . One should not expect that ordinary quantum mechanics which treats the electron as a point-charge could hold under these conditions. It is very interesting that the energy loss of fast electrons really proves this view and thus provides the first instance in which quantum mechanics apparently breaks down for a phenomenon outside the nucleus. We believe that the radiation of fast electrons will be one of the most direct tests for any quantum electrodynamics to be constructed.<sup>88</sup>

In October of 1934, an International Conference on Cosmic Rays was held in London. Present were the advocates of the atom-building hypothesis, Millikan, Bowen, and Neher, as well as Anderson and Neddermeyer, who, although drifting from the conclusions of Mil-

likan, continued to pursue the program of measuring cosmic ray absorption coefficients and energies. Also present was the author of the most complete quantum treatment of the physics of cosmic ray absorption, Hans Bethe, as well as the experimentalist who had most advanced the program based on corpuscular cosmic radiation, Bruno Rossi.

By the time of the London conference, Anderson and Neddermeyer were comparing their experimental results not with Millikan's atom-building theory, but with the new quantum calculations. Shortly before the conference, they submitted an abstract to the *Physical Review* in which they asserted:

measurements of the energies of secondary electrons produced in plates of lead and carbon by cosmic ray electrons . . . have shown that the distribution in energy of secondary negatrons is, within experimental certainty, in agreement with the distribution calculated from the theoretical cross section given by Carlson and Oppenheimer.<sup>89</sup>

Even this much agreement, however, did not last long. At the London conference, Anderson and Neddermeyer hardened their conclusions, and by so doing helped precipitate one of the first of the many theoretical crises quantum electrodynamics would face in the years to come. They argued that,

While the [absorption] data presented above give evidence for the existence of rather large radiative losses, they constitute as well strong evidence for the breakdown of the theoretical formula in the energy range above 100 MeV.<sup>90</sup>

Privately, however, Anderson offered Bethe somewhat more conciliatory judgment of the agreement between theory and experiment. In a letter to Bethe of 7 June 1935, Anderson remarked that his experiments with Neddermeyer,

so far are very incomplete and not accurate, and there is not a great deal that can be said about the validity of the theoretical formulae. It looks as though the theory and experiment do not conflict badly for electron energies below 100 MeV, but that for higher energies the formulae give too high values for the absorption.<sup>91</sup>

At the time these remarks were written, only the Heitler-Sauter for-



mula was available, which as mentioned earlier, was an approximation that left out the screening of the nucleus due to inner electrons. In a footnote added after the conference, the authors noted that even the Bethe-Heitler theory (which included screening) predicted radiative losses, "too high to be reconciled with our experimental data, although the latter contain as yet too few cases where accurate measurements are possible, for a satisfactory comparison to be made."<sup>92</sup> The only explanation for the discrepancy seemed to the authors to be that the penetrating particles were protons and not electrons. Bethe and A. H. Compton came independently to the same conclusion.<sup>93</sup>

The proton hypothesis seemed to clash directly with two other results. First, if the secondary electron energy distribution was calculated on the assumption that the primary cosmic rays were composed of protons, it was found to be incompatible with the measured distribution. Second, if the primaries were protons, some of them should arrive at sea-level with low energy. And while at high energies, it was difficult to distinguish positive electrons from protons, at low energies it was easy. The absence of any low energy protons thus presented a further argument against the primary proton hypothesis.

The above considerations, [Anderson and Neddermeyer concluded] which are of a statistical nature, and necessarily subject to the gathering of further data, tend to favor the view that most of the high energy cosmic ray particles at sea-level have electronic mass. If further data prove this view to be correct then it is obvious that the present theory of radiative losses by electrons must be inapplicable in the range of very high energies.<sup>94</sup>

Despite their qualifications, the effect of Anderson and Neddermeyer's announcement added to the discouragement of many of the theorists. In retrospect, of course, we can say that they were getting their first (statistical) glimpse of the muon which we now know constitutes some 90% of sea-level cosmic ray charged particles. At the time, however, the existence of a new particle was not discussed. Instead, it was the theory, quantum electrodynamics, that was being put to the test (and failing) at high energies.<sup>95</sup>

Bethe, who attended the talk by Anderson and Neddermeyer, immediately conceded that the experiments boded ill for the Bethe-Heitler theory. In the discussion period after the talks, Bethe commented:

The experiments of Anderson and Neddermeyer on the passage of cosmic-ray electrons through lead are extremely valuable for theoretical physics. They show that a large fraction of the energy loss by electrons in the energy range round  $10^8$  volts is due to emission of  $\gamma$ -radiation rather than to collisions, but still the radiative energy loss seems far smaller than that predicted by theory. Thus the quantum theory apparently goes wrong for energies of about  $10^8$  volts.<sup>96</sup>

Future experiments, Bethe concluded, would be necessary mainly to determine precisely at what energy the alleged breakdown became significant.

Word of Bethe's change of heart over the quantum theory was soon received in Germany, where Carl Friedrich von Weizsäcker, Werner Heisenberg, and other physicists continued to do cosmic-ray related research. Von Weizsäcker wrote to Bethe in December of 1934, asking:

Do you now actually believe in your radiation formula for  $E > 137 mc^2$  or not? Anderson's London report was not too clear to me on this point, but you have in fact spoken with Anderson himself. In the meantime, you published a note according to which it seems you now believe in the calculations [of the Bethe-Heitler theory], but I could not determine with any certainty whether the reversal was partial or total. Heisenberg mentioned to me that he wrote you that Weisskopf has found a hole-theoretical argument against the Fourier analysis.<sup>97</sup>

For Bethe the concession that quantum electrodynamics would break down was not easy, as the Bethe-Heitler theory was in his eyes a great success precisely by sidestepping the more "philosophical" objections of many other physicists stemming from less practical issues than energy loss measurements. Oppenheimer, in particular, even before the specific energy loss measurements, was worried about the theory. Since the discovery of the positron in 1932, many theorists had re-examined the Dirac theory, taking its solutions more seriously than previously. During the spring of 1933, Bohr lectured at CalTech, where Oppenheimer spent time with him discussing problems related to the problem of pair-creation.<sup>98</sup> After Bohr's lecture, Oppenheimer wrote that he was working on the theory of pair creation,<sup>99</sup> and again to his brother, Frank Oppenheimer, in October, that he was still at it.<sup>100</sup>

By the fall of 1933, Oppenheimer was convinced that something was deeply wrong with the theory. "I think . . .," he wrote to George

Uhlenbeck, “that the methods of the radiation theory give completely wrong results when applied to wave lengths of the order of the electron radius.”<sup>101</sup> In fact, by June of 1934, Oppenheimer was on the verge of despair about the state of physics when he wrote his brother:

As you undoubtedly know, theoretical physics – what with the haunting ghosts of neutrinos, the Copenhagen conviction, against all evidence, that cosmic rays are protons, Born’s absolutely unquantizable field theory, the divergence difficulties with the positron, and the utter impossibility of making a rigorous calculation at all – is in a hell of a way.<sup>102</sup>

Oppenheimer was thus already deeply pessimistic about the state of quantum electrodynamics *before* all of Anderson’s results were finished and presented at the London conference. In November, when these last figures were available, Oppenheimer submitted an article to the *Physical Review*, entitled, “Are the Formulae for the Absorption of High Energy Radiations Valid?”<sup>103</sup> His answer to the title question was a resounding “no.” The problem, Oppenheimer stressed, was two-fold: first, Bethe-Heitler theory predicted an increase of cloud chamber ionization with energy that was not observed by Anderson and Neddermeyer, or by Kunze. Second, the specific energy losses measured by Anderson and Neddermeyer also seemed too low to be compatible with Bethe-Heitler theory. Consequently, Oppenheimer argued that, “It is . . . possible to do justice to the great penetration of the cosmic rays only by admitting that the formulae are wrong, or by postulating some other and less absorbable component of the rays to account for their penetration.”<sup>104</sup>

Oppenheimer concluded that the formulae break down at high energies; he then sought to explain why this should occur. The argument he offered was based on considerations of classical (Lorentzian) electron theory. Suppose that an electron is given as a spherically symmetric distribution of charge in a sphere with the classical electron radius,  $r_0 = e^2/mc^2$ . Then  $F = ma$  will give the correct motion of the electron only if we include the reaction of the electron as it emits radiation:

$$F_{\text{ext}} = m \ddot{X} + F_{\text{rad}}.$$

Following Lorentz we determine  $F_{\text{rad}}$  by demanding conservation of

energy over a time characteristic of the distance,  $r_0$ , i.e.  $\Delta t = r_0/c$ . The energy radiated away should equal the negative of the work done by  $F_{\text{rad}}$  on the electron.

$$\int \mathbf{F}_{\text{rad}} \cdot \mathbf{v} dt = - \int P(t) dt.$$

Using Larmor's formula for the power radiated by an accelerated charge  $P(t) = \frac{2}{3} e^2/c^3 (\ddot{X})^2$  and integrating by parts we get for any periodic motion:

$$F_{\text{rad}} = \frac{2}{3} \frac{e^2}{c^3} \ddot{X}.$$

In order for classical electron theory to apply unambiguously, this and higher order terms had to be small. Roughly,

$$F_{\text{ext}} \approx ma$$

if

$$\frac{F_{\text{rad}}}{F_{\text{ext}}} = \frac{\ddot{X}}{\dot{X}} \left( \frac{r_0}{c} \right) \ll 1$$

and if similar restrictions suppressed higher derivative terms. Frequencies of electron motion greater than  $(c/r_0)$  would clearly violate these conditions. In the quantum case the dangerous frequencies were those above  $c/r_{qm} = mc^2/h$ . The theory of energy loss as developed by Weizsäcker and Williams involved Fourier decomposing the projectile's electric field, and examining component's effect on the atomic electron. If a significant portion of the decomposition consisted of frequencies above the critical frequency, Oppenheimer foresaw disaster.

In fact the ordinary Weizsäcker-Williams theory indicated such frequencies were not important. No matter, an alternative electrodynamics recently developed by Bohr<sup>105</sup> suggested there might be a very large high frequency component. Here, Oppenheimer argued, was the tombstone of quantum electrodynamics.

Oppenheimer's students, among them Wendell Furry, also adopted a highly skeptical stance towards the validity of quantum elec-

rodynamics at high energies. After recalculating the number of pairs produced by photons of different energies, Furry and J. F. Carlson asserted that at low energies their results agreed well with experiments on beryllium (which produces  $\gamma$ -rays of about 5 MeV). However,

For energies above twenty million volts the predicted pair production is even greater than that computed by Oppenheimer and Plessett, and hence even more irreconcilable with experiment. It seems possible to connect this discrepancy with the fundamental inadequacies of quantum electrodynamics.<sup>106</sup>

In sum, the high frequency components of an external field put into question the validity of the new quantum electrodynamics if the field could not be Fourier decomposed in the usual way. And this was just the claim of Oppenheimer on the grounds of Born's non-linear electrodynamics, and of Weisskopf on the basis of his interpretation of Dirac's hole theory. Other physicists, like Nordheim, Furry, Carlson, Bohr, and Bethe, were also for some time convinced that the quantum theory must break down at high energies because of the new experimental results. The choice was clear: quantum electrodynamics or a new less absorbable ionizing radiation. For the moment at least, the entire circle of physicists interested in these problems opted for the demise of quantum electrodynamics. In a letter to E. J. Williams, Bohr wrote: "I am ever more and more inclined in the experimental results to see an indication of a new fundamental aspect of electron theory, for which the limitation of classical theory may well leave room, but for which it offers no guide whatsoever."<sup>107</sup> A radically new theory seemed to be needed.

#### *V. The Discovery of the Muon and the Resurrection of Quantum Electrodynamics*

Oppenheimer's choice (Bethe-Heitler theory or a new particle) was complicated by the uncertainty as to which cloud chamber tracks the Bethe-Heitler theory ought to apply. Such was the confusion during this time that Anderson and Neddermeyer began to speak, among themselves, of "red and green electrons," where the red ones were especially absorbable and the green ones passed easily through matter.<sup>108</sup> By far the most dramatic phenomena observed in the cloud chamber and counter devices were the showers. Almost immediately

the question arose whether the constituent particles of the showers were ordinary electrons, some other "red" type of electron, or a new particle altogether. Both Rossi and Curry Street later recalled that during this time in 1935, it was commonly assumed that the shower particles were the "new" type of particle and that the penetrating particles were ordinary high energy electrons that simply did not obey Bethe-Heitler theory. Such a conclusion was natural given the seemingly strange behavior of showers in which it often seemed that many particles were ejected from a single site.<sup>109</sup>

Current theory offered no explanation of multiple simultaneous emission of particles. "Thus existing theory is . . . quite unable to explain shower formation," concluded P. M. S. Blackett in June 1936. He continued, "Now shower formation seems to be observed to start to occur just about for those energies at which the predictions of quantum electrodynamics begin to be false. Clearly therefore some radical new theoretical step will be required before showers can be explained."<sup>110</sup> As mentioned earlier, some theorists, especially Heisenberg, Bohr and Pauli, were pursuing such 'radical' theoretical approaches.<sup>111</sup> Meanwhile in the United States Anderson continued his cloud chamber energy loss analysis of the mysterious shower particles. Street took a different tack.

During his last year at the Bartol Institute (1931–32), Street had constructed logic circuits, based on Rossi's publications, in order to experiment on showers and related phenomena.<sup>112</sup> All of Street's investigations were based on varying the geometric arrangement of counters that were wired to coincidence and anti-coincidence circuits. Since the counters could be separated by large distances, they were easily adapted to measuring the particle flux coming from a certain direction. It was therefore relatively easy for Street to compare the charged particle flux arriving from different directions. His technique was soon applicable because in 1930 Rossi had noticed that if the incoming cosmic ray particles were charged, and predominantly of one type, the earth's magnetic field would create an asymmetry between the flux from the east and that from the west.<sup>113</sup> Like the latitude effect, Rossi's "East–West Effect" became a test of whether the primary particles were photons or charged corpuscles. And unlike the latitude effect, the *sign* of the incoming particles would be revealed in the direction of the asymmetry.



For this reason, and because the east-west effect was free of the problems associated with transporting equipment to climatically and geographically diverse points, after 1931 Rossi's work focused on this effect rather than on the latitude effect. In 1933, Thomas Johnson,<sup>114</sup> Luis Alvarez and A. H. Compton<sup>115</sup> and Rossi<sup>116</sup> found the effect, indicating to their surprise that the incoming particles were positive. When Street confirmed their results in late 1933, it left him with the firm conviction that primary particles were charged and not, as Millikan had claimed, photons.<sup>117</sup>

By the time Street came to Harvard in the fall of 1933, he was convinced that the coincidence circuits with Geiger tubes were the most effective way to study cosmic rays. Ionization chambers, by contrast, did not seem to him to be of great interest.<sup>118</sup> In addition, from other preliminary measurements of showers, again using counters, he knew that non-ionizing radiation could produce secondary ionizing radiation. At Harvard, Street repeated these counter experiments with his students, E. C. Stevenson, and later, with a student from M.I.T., L. Fussell. They were able to improve the apparatus and to make an absolute calibration of the counters. With this equipment, the Harvard group repeated more of Rossi's counter experiments showing that coincidences occurred between counters separated even by large thicknesses of lead (tens of centimeters). Thus when the Anderson, Millikan, Neddermeyer, and Pickering paper appeared in 1934 attacking Rossi's assertion that these coincidences were due to the passage of single particles, Street's work was implicated as well. Indeed, much later, Street recalled Millikan's absolute conviction that "nothing was going through all this thick material," since this would have conflicted with his Birth Cry theory. "So," Street remembers, "we thought we better learn how to do cloud chambers."<sup>119</sup>

Building the chamber was not easy, but when the project was completed several important technical innovations made its use considerably easier. All cloud chambers work on the same principle: when a particle passes through a gas it ionizes atoms in its path. If the gas is subsequently suddenly expanded, the temperature drops and the gas will condense around the ionized particles, leaving a visible track. Ordinarily the expansion was triggered at a random time; this was, for instance, the technique of Anderson *et al.* But C. T. R. Wilson had explored the use of counter-controlled expansions, thus vastly in-



creasing the number of useful photographs. By 1934, Street and his group were familiar with the use of logic circuits and Geiger tubes; the next step was to combine the chamber with their logic circuits. In so doing, they created a device they called a *hodoscope*: a chamber sandwiched between two counters connected through a coincidence circuit. With this apparatus they were able to vindicate both their own and Rossi's work by showing conclusively that individual charged particles were passing through at least 45 cm of lead. In their words, "at least 90% of the coincident counts for such an arrangement are directly due to the passage of single electrons through the apparatus."<sup>120</sup>

Almost simultaneously with the above publication, Street and R. H. Woodward (another of Street's graduate students) submitted an abstract that appeared in the same issue of *Physical Review*.<sup>121</sup> Before this abstract was printed, it was well known that showers increased to a maximum in about 1.5 cm of lead. From this fact, some authors had incorrectly concluded that the shower particles themselves had a *peak* in their penetration length of about 1.5 cm. Street and Woodward showed that this was not the case; individual shower particles were simply absorbed exponentially. By so doing, for the first time the authors focused attention on the properties of shower particles as distinct from the showers themselves. Again, the key to their success was their coordination of counter-controlled logic circuits and the cloud chamber.

Yet another of Street's articles, in collaboration with Woodward and Stevenson, appeared in the same volume of *Physical Review*.<sup>122</sup> This time the authors used their apparatus definitively to rule out the conclusion of the Anderson, Millikan, Neddermeyer, and Pickering paper, i.e., that coincidences of Geiger tubes surrounding large thicknesses of lead were due only to shower effects. Furthermore, Street, Woodward, and Stevenson combined their new and better absorption curves based on studies of individual particles with the energy distribution of Anderson, Millikan, Neddermeyer, and Pickering. Taken together, the two results determined a specific energy loss curve given as a function of energy. Still, as before, they invariably referred to the *penetrating* particle as the electron.

Since the penetrating particles were thought to be electrons, Street continued to search systematically for peculiarities in the character of the shower particles. By January 1936, Street and Stevenson had

results; the authors contrasted the probability of an “electron”, taken at random from the cosmic rays producing a shower, to the probability of a “shower electron” producing a shower. While the former was but two in a thousand, the latter was almost twenty-five percent.<sup>123</sup>

These “electrons taken at random” were also measured by Anderson and Neddermeyer. Thus by June of 1936, Anderson, Neddermeyer, Rossi, Stevenson, and Street all knew that the electrons of Bethe-Heitler theory could not be reconciled with experiment. Anderson and Neddermeyer by this time had withdrawn all qualification from this judgment, and wrote: “It is obvious that either the theory of absorption breaks down for energies greater than about 1000 MeV, or else that these high energy particles are not electrons.”<sup>124</sup> Again in a letter Anderson assured Heitler, “As we all know, the high energy particles are certainly more penetrating than the theory permits; and since all the evidence indicates that they can not have protonic mass, apparently the theory breaks down at energies somewhere above 400 MeV.”<sup>124</sup> On the one hand, the authors recognized the success of the Bethe-Heitler theory in explaining the existence of large showers with altitude, and the strong dependence of shower particle behavior on thickness and type of material traversed. On the other hand, there remained a “large fraction of the sea-level particles” that were more penetrating than the theory possibly could permit.

To discover what these particles were, Anderson and Neddermeyer continued to search for penetrating particles of low enough energy to determine if they were in fact protons. By now asking “*what are the penetrating particles?*” (and assuming the shower particles were electrons), they reversed the earlier question, “*what are the shower particles?*” (assuming the penetrating particles were electrons). Because the theory now seemed to be reasonably successful at describing what at first had seemed a qualitatively new phenomenon (showers), the new physics increasingly seemed to be associated with what had previously seemed to be the “ordinary” phenomena, namely the penetrating particles. Unfortunately, the much sought-after photographs of penetrating particles of sufficiently low energy remained difficult to obtain. In sum, by mid-1936, both the east and west coast groups had made a conceptual separation of shower and penetrating particles, and there was a growing suspicion that it was the penetrating particles that were problematic by not conforming to the Bethe-Heitler theory of electrons’ passage through matter.



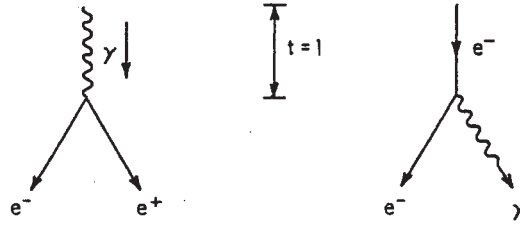


Figure 3. Pair creation and Bremsstrahlung.

The shower will come to an end after losing all its initial energy  $E_0$ , i.e., when it has traversed a distance  $T$  such that:

$$\int_0^T dE = E_0.$$

If  $\partial E / \partial t = \beta$  is approximately constant, then

$$dE = E_0 = \left(\frac{1}{2}\right) \beta \int_0^T 2^t dt = \left(\frac{1}{2}\right) \beta 2^T / \ln 2,$$

or

$$2 E_0 \ln 2 = \beta 2^T.$$

Therefore (i) the shower's length  $T$  increases only logarithmically with  $E_0$ , (ii) the number of particles  $2^T$  will increase approximately linearly with  $E_0$ , and (iii) for showers of approximately 30 particles,  $T = \ln_2 30 \sim 5$ . For lead, one interaction length is about  $(1/2)$  cm, so the maximum would be around  $5/2$  cm of lead, which is in good accord with the observed maximum.

These (and the more precise predictions of the diffusion equations that followed) showed that the quantum electrodynamic view of showers as a compound of elementary processes could accurately represent many of the qualitative features of showers. Thus the showers seemed well accounted for. However, if the penetrating particles were electrons, according to these calculations they should be almost totally absorbed by 20 cm of lead; this was manifestly not the case.

From this, [Carlson and Oppenheimer wrote] one can conclude, either that the theoretical estimates of the probability of these processes are inapplicable in the domain of cosmic-ray energies, or that the actual penetration of these rays has to be ascribed to the presence of a component other than electrons and photons. The second alternative is necessarily radical; for the cloud chamber and counter experiments show that particles with the same charge as the negative electron belong to the penetrating component of the radiation; and if these are not electrons, they are particles not previously known to physics.<sup>127</sup>

Indeed, since the success of the multiplicative shower theory, these particles “not previously known to physics” now were the main problem. As Oppenheimer understood, all his work on showers was valid “only if [one] admits the presence of another component [of cosmic radiation] to which the analysis is not at all applicable.”<sup>128</sup> Oppenheimer and Carlson’s paper was received on 8 December 1936. Just four days later Anderson delivered his Nobel Prize address on the discovery of the positron. He too remarked on the peculiar, non-electronic, properties of the penetrating radiation. Without elaboration, Anderson suggested that “These highly penetrating particles, although not free positive and negative electrons, appear to consist of both positive and negative particles of unit charge, and will provide interesting material for future study.”<sup>129</sup>

The contrast between the two kinds of particles that had been pointed to by Street and Stevenson was then taken up again by Anderson and Neddermeyer in an article received 30 March 1937.<sup>130</sup> Instead of looking at the specific energy loss of cosmic ray particles in general, they made separate measurements of shower and penetrating particles. (See Figures 4a, 4b.) To explain this separation between the two clusters of data points, the authors offered the following choice:

Interpretations of the penetrating particles encounter very great difficulties, but at present appear to be limited to the following hypotheses: (a) that an electron (+ or -) can possess some property other than its charge and mass which is capable of accounting for the absence of numerous large radiative losses in a heavy element; or (b) that there exist particles of unit charge, but with a mass (which may not have a unique value) larger than that of a normal free electron and much smaller than that of a proton. This assumption would also account for the absence of numerous large radiative losses, as well as for the observed ionization. Inasmuch as charge and mass are the only parameters which characterize the electron in the quantum theory, assumption (b) seems to be the better working hypothesis.<sup>131</sup>

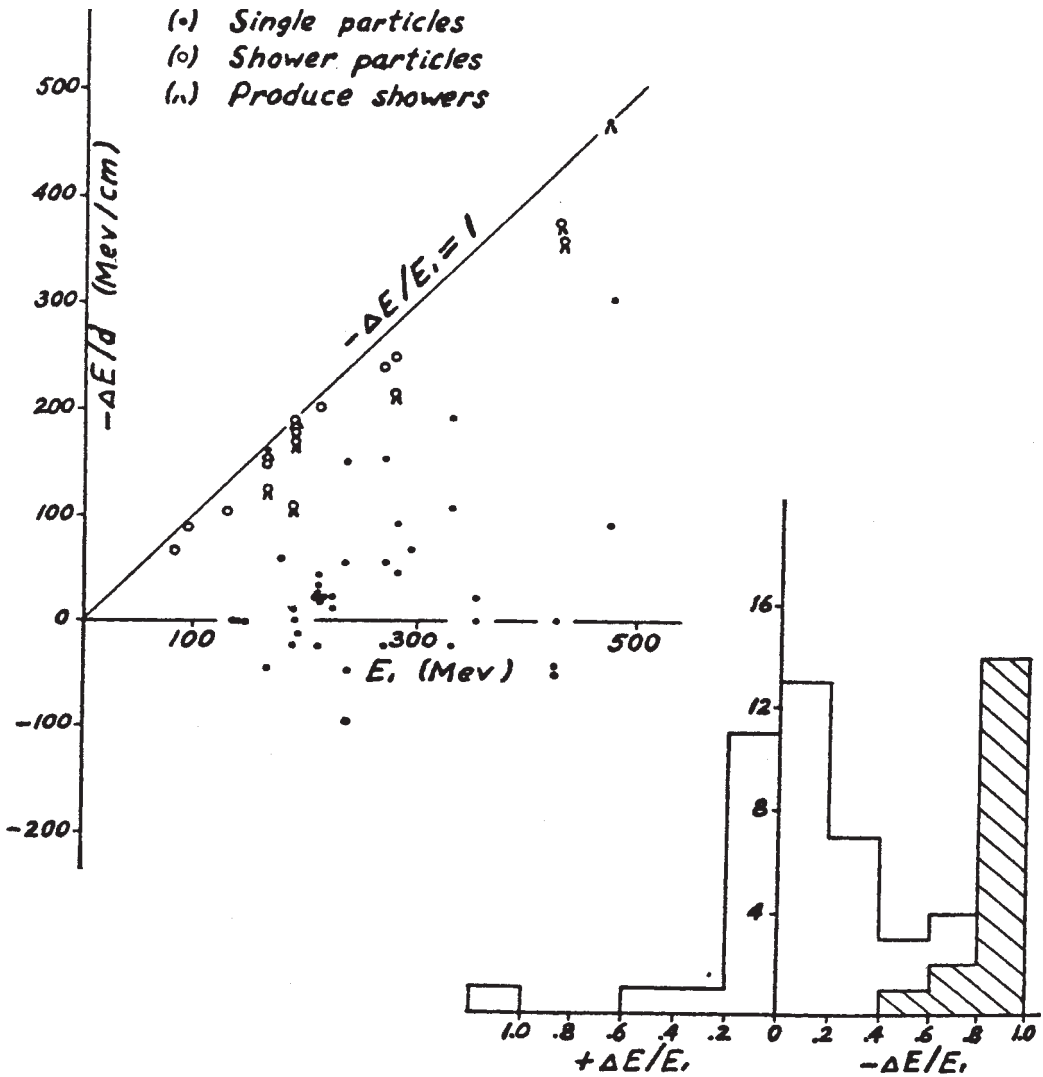


Figure 4. Energy loss evidence for a new particle. Both Figures 4a and 4b show that shower and non-shower particles behave differently.

4a. Energy loss per cm,  $\Delta E/d$  in 1 cm platinum as a function of initial energy,  $E$  in MeV.

4b. Frequency distribution of fractional energy loss,  $\Delta E/E$ . Shaded area indicates particles entering accompanied by others or themselves producing showers in the platinum. Negative  $\Delta E/E$  from statistical spread and upwardly moving particles. Source: Anderson and Neddermeyer, "Nature of Particles," (Ref. 130), p. 884.



At approximately the same time (April 1937) J. C. Street and R. T. Young, Fussell and Stevenson arrived at the same conclusion. Their demonstration of the existence of a new particle had two parts. First, Fussell conducted studies of showers using a series of very thin (down to 0.07 cm) plates.<sup>132</sup> By so doing, the complex showers could be shown experimentally to follow the multiplicative pair production schema set out by Carlson and Oppenheimer. One could actually see the constituent pair-production build-up of complex showers. Fussell concluded that his "observations give strong support to the radiation, pair formation theory of showers . . ." It was then possible for Street to study the range, energy and shower production of the other, penetrating component of cosmic rays, assured that the Bethe-Heitler-Carlson-Oppenheimer theory accounted for the electrons observed in showers.<sup>133</sup> Street's new apparatus was built as indicated in Figure 5.

The upper cloud chamber indicated whether the particle was single or part of a shower; the counter showed whether the particle continued through the apparatus; the lower cloud chamber exhibited whether or not the particle produced showers. When the results were tabulated, it became clear that a much greater fraction (on the order of  $10^4$ ) more non-shower particles were penetrating over 6 cm of lead than would be permitted by Bethe-Heitler theory for electrons of similar momentum. Moreover many of Street's non-shower particles were in the same energy range as the electrons of Anderson's 1936 measurements.<sup>134</sup> Since the two groups of particles had radically different penetrating power Street concluded his were not electrons. Finally the non-shower particles had too small an ionization to be protons.

James Bartlett, a physicist from the Institute for Advanced Study at Princeton, immediately challenged Street's results: "It seems to me rather risky, in general to exclude protons merely on the basis of a theoretical stopping-power curve . . ."<sup>135</sup> In response Street reiterated his argument against electrons and calculated explicitly the energy of several proton tracks. These ionized much more than similarly energetic non-shower particles. "I believe," Street concluded, "that these observations make a very strong case for a new particle."<sup>136</sup>

Although it was the range-energy experiment that convinced Street and Stevenson that a new particle existed, it is not for this work that

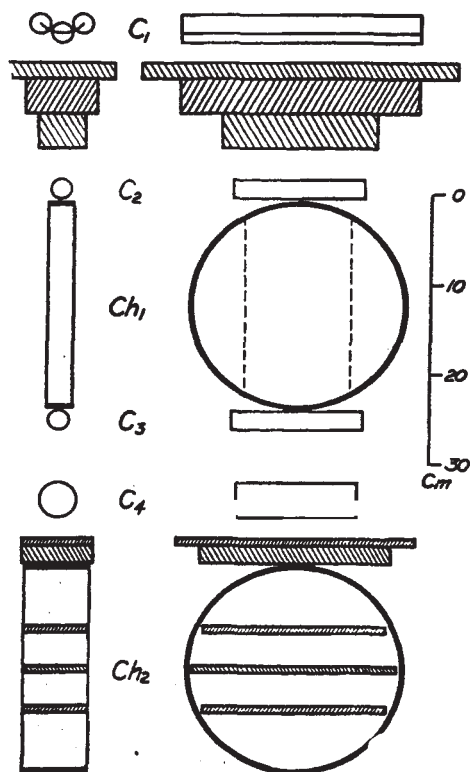


Figure 5. Range energy loss apparatus of Street and Stevenson. Left is side view, right is end-on.  $c_1$ ,  $c_2$ ,  $c_3$ ,  $c_4$  are Geiger-Müller tubes that trigger the simultaneous expansion of two cloud chambers,  $ch_1$ ,  $ch_2$ .  $ch_1$  measures particle momentum because it is in a 7-kilogauss horizontal magnetic field.  $ch_2$  has three (shaded) lead plates to identify shower-producing particles. Source: J. C. Street, "Showers" (Ref. 4), p. 783.

they are usually remembered. Many physicists (such as Bethe) were persuaded by<sup>137</sup> a remarkable photograph of a dense track, with an ionization and curvature indicating a mass of about 130 electron masses. This photograph has subsequently been reproduced in several texts.<sup>138</sup> (See Figure 6.)

This striking photograph was shown at many meetings. Furry recalls bringing a copy of it to England where in conferences it created quite an impression on the audiences; for many people it was the most

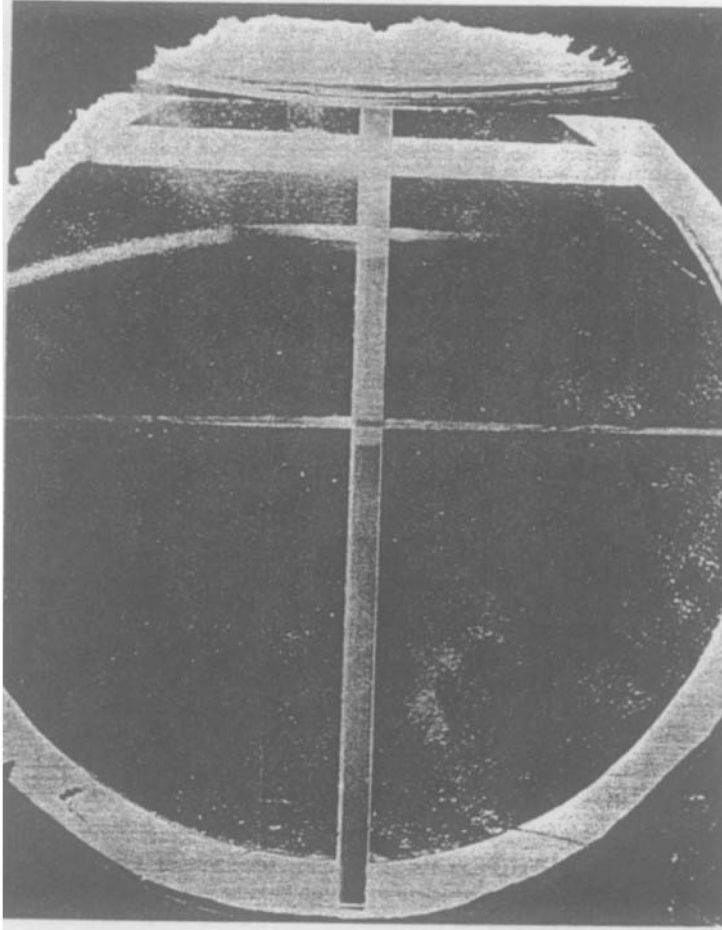


Figure 6. Stopping muon. First photograph of a muon travelling slowly enough to allow measurement of its ionization and momentum. From these quantities Street and Stevenson deduced the muon mass:  $M_\mu \sim 130 M_e$

Source: Harvard Physics Department photo archives. A detail from this picture appeared in Street and Stevenson, "New Evidence", (Ref. 138).

impressive piece of evidence for the new particle. However, for Street and Stevenson the energy-range relation remained the conclusive evidence. In Street's words, this

was the approach I always considered the soundest and the most convincing but it wasn't the most convincing to the uninitiated listener. You had to have studied the subject and

thought about it and figured out a way to have measured the mass. But it will convince you if you take the time to study it.<sup>139</sup>

As for the photograph,

The picture we did of a dense track near the end of its range was really just a thing you did for a demonstration lecture – it had to be there. It was just a question of getting it done . . . [Still] my reason for not depending on it too much was that we never had but one or two of those [photographs] and anything can happen once, so we weren't too happy with our experiments on that.<sup>140</sup>

In part Street's suspicions derived from a few tracks that suddenly and discontinuously lost their energy. As he wrote to Furry in 1938,<sup>141</sup> about one in twelve of the stopping particles did so in violation of the new particle's energy-range relation. But, Street continued,

I believe that these apparent stoppings are due to imperfections in lighting, poor geometry, scattering, etc. and are not significant. The new apparatus should answer this. In any event I have no faith in the accuracy of these observations of *stoppings* unless they occur with considerably greater frequency than one in twelve. Of course the observations of *penetration* are safe.

Convincing evidence for Street and Stevenson could only come by the large statistics garnered from the carefully calibrated counters set in coincidence circuits with the cloud chambers.

## VI. Conclusion: Persuasive Evidence

The question posed at the outset of this essay, "When was the muon discovered?" has no unique chronological answer. Since almost all sea-level ionizing cosmic radiation is composed of muons, there is a sense in which the first eighteenth-century observer of a spontaneously discharging electroscope "discovered" the muon, since he was the first to observe their effects. With similar cogency, one could argue that the Bothe and Kolhörster counter coincidence experiment indicated the passage of particles that in retrospect we know to be muons. And, of course, there is a sense in which Carlson and Oppenheimer discovered the muon, since they first suggested the existence of a particle of intermediate mass in 1936. Anderson and Neddermeyer first presented good data showing that energy loss measurements of

shower particles fit the Bethe-Heitler theory. This implied (though we can see this only in retrospect) that the penetrating particles must be other than electrons. Or Street and Stevenson could be credited with the discovery for having shown that there was a characteristic difference between the shower-producing power of shower particles and of penetrating particles.

Most authors now give the credit to Anderson and Neddermeyer's March 1937 energy loss argument and/or Street and Stevenson's April 1937 range-momentum argument. Both of these experiments showed that the charged cosmic ray particles fell into two distinct groups *in the same momentum range*. Of course, one could equally well attribute the discovery to Street and Stevenson's November 1937 photograph of a stopping track, for this provided the first quantitative analysis of the penetrating particles's mass. There are many other points that could be chosen as the "moment of discovery." However, I hope to have shown that such a term – perhaps valuable for prize committees and physics textbooks – corresponds to little or nothing in the historical development of cosmic ray physics. In large part the systematic investigation of the character of the rays began with Millikan's interest in the origin and fate of the elements. When the muon was finally separated from the cosmic rays as a new kind of matter, the study of elementary particle physics had begun.

Instead of looking for a "moment of discovery" we should envision the evolution of cosmic ray physics as a progressively refined articulation of the properties of the rays. At each stage of the process new characteristics could be attributed to them: they discharged electroscopes; the discharge rate varied in a certain fashion with depth in matter; the shower particles were more easily absorbed than the single particles. In fact the final "demonstration experiments" by Anderson, Neddermeyer, Street and Stevenson rest their persuasive force on a great number of these earlier "auxiliary experiments". Some of these experiments tested the apparatus. When counters and cloud chambers were first combined the reliability of counters was at issue. Other auxiliary experiments served to forge a stronger link between theory and experiment. Such was the object of Fussell's cloud chambers with thin plates to exhibit the elementary pair production underlying complex showers.

Finally theory itself played an essential role by highlighting the

difference between the quantum electrodynamical “red” shower particles and the puzzling penetrating “green” electrons. Of course without experimental studies of showers the theorists would have had little incentive to study complex processes. But without the shower calculations the conceptual wedge between the green and the red would have taken much longer to develop. And it was precisely the clarity of this distinction which made it manifest to Anderson, Street, and their co-workers that it was the *penetrating* “green” particles that demanded an explanation – not the “red electrons” of the showers that were accounted for by the Bethe-Heitler theory. (See Table 3.)

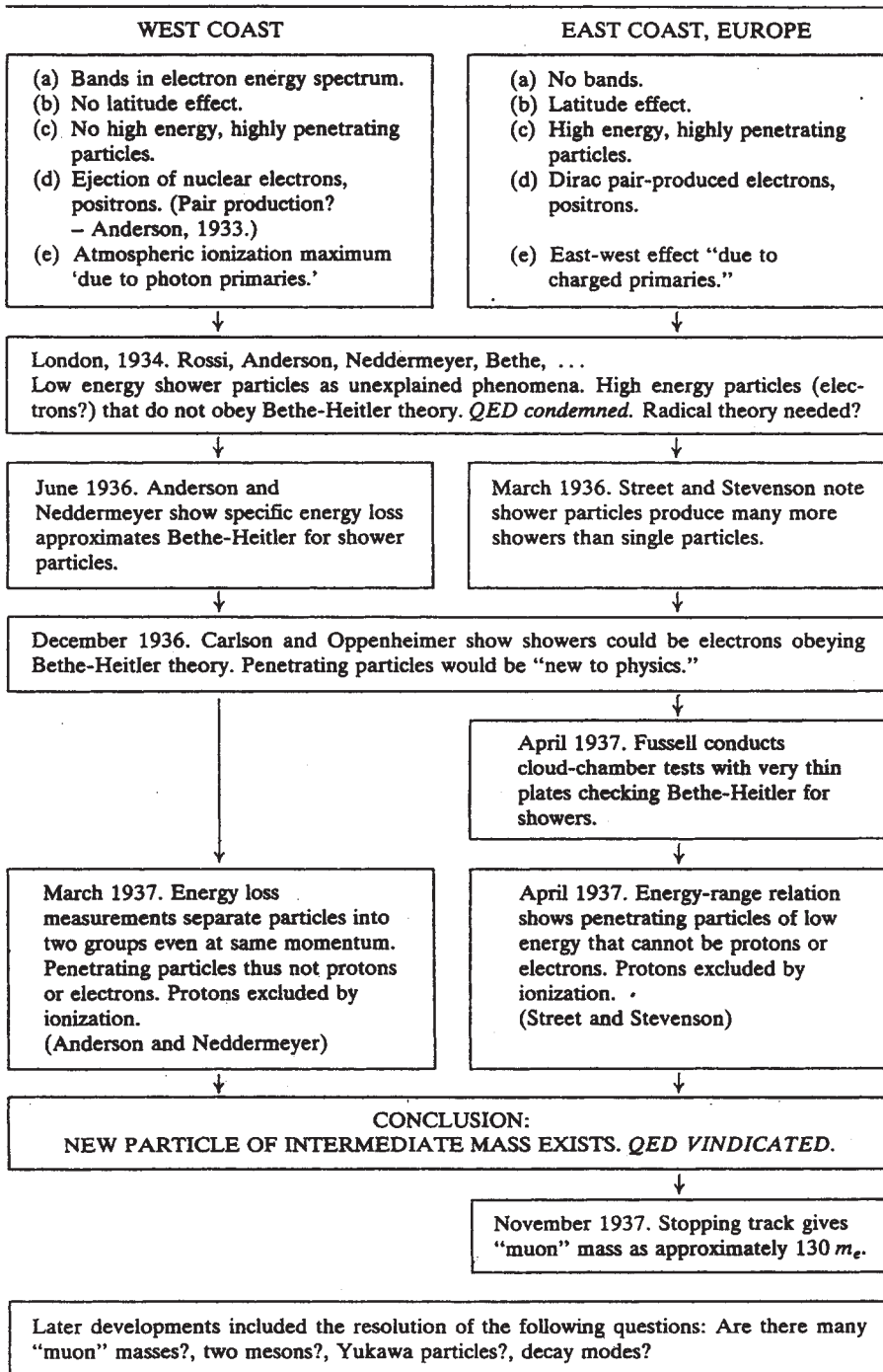
The shifting of attention from the red to the green particles flagged the end of the failed revolution against quantum electrodynamics. For as long as showers remained mysterious, too complex to be tractable using quantum electrodynamics, all speculation was possible. Heisenberg, Bohr and Pauli expected their revision of fundamental concepts to rescue them from experimental difficulty as surely as the quantum program of 1926. Revolution was not the order of the day a decade later. At least not in physics. The required theory demanded only the pragmatic and persistent application of quantum mechanics and relativity.

In the above I have stressed the closeness of experiment and theory for both the east and west coast groups. Yet there were, despite their often parallel development, marked differences in their motivations, equipment, and styles of demonstration. On the west coast, the systematic study of cosmic rays was originally fueled by Millikan’s conviction that elements were being formed throughout space. This led, via  $E = mc^2$  and  $E = h\nu$ , to his belief that photons at specific energy bands constituted the primary cosmic radiation. In turn, Millikan’s theory of photons as primary cosmic rays determined his emphasis on the study of the absorption curves of the radiation. Indeed, only by understanding Millikan’s original project can one see the real origin of his experimental program of measuring discharge rates in lakes, on mountains, and under different thicknesses of lead. So too, can one only understand Millikan’s mistakes such as his commitment to the “band theory,” and his persistent attacks on the reality of the latitude effect.

Along with Millikan and his co-workers’ study of absorption curves came perhaps his greatest cosmic ray success: his assignment to Carl



Table 3. Summary of the discovery of the muon.



Anderson of the problem of measuring the energy of the "secondary electrons." This too can only be understood as an offshoot of Millikan's original goals, for he fully expected Anderson to find corroborative evidence of bands. Finally, as Anderson progressed, discovering nuclear disintegrations and pair production, Millikan appropriated the results as additional evidence for the Birth Cry of atoms.

By 1934 Anderson had publicly separated himself from Millikan's views and had begun to entertain the emerging theory of quantum electrodynamics. Anderson's experimental techniques, however, remained altogether continuous with his earlier work. Millikan's assignment to Anderson in 1931 had been to measure electron energies. In demonstrating the existence of the positron, Anderson had made use of the energy difference of a particle above and below a lead plate. Finally, as he approached the problem of testing the predictions of quantum electrodynamics, he did so by improving his cloud chamber techniques for measuring energy losses of particles passing through lead. Even the apparatus was the same as that used in 1932. Clear photographs of tracks through lead plates indicating energy loss constituted persuasive evidence for Anderson.

Quantum electrodynamics had come to cosmic ray physics in several ways. First, it had arrived through Bethe's cross section calculation for the passage of fast electrons through matter. Bethe maintained close contact with some experimentalists (especially Rossi) ever since Bethe's time in Italy. This was natural since Rossi's experiments were well suited to, indeed designed for, the investigation of corpuscular cosmic radiation. Similarly, in the United States, Furry (who was also working on quantum electrodynamics) consulted frequently with Street at Harvard. Furry's help, along with Street's long-standing interest in Rossi's work, contributed to the Harvard group's focus on the use of logic circuits and counter equipment to garner statistical evidence. Street found these statistical arguments much more compelling than the few individual photographs he and Stevenson later obtained. Years of experience with counter coincidence apparatus made the range-energy experiments persuasive to Street. Thus, while both the east and west coast groups found their way to the muon, they did so with very different experimental styles. Consequently, they found different kinds of evidence convincing.

Thus by examining the two groups' decision to end their experiments and to announce the discovery of a new particle we can see the workings of their two traditions. In each three stages are present. Roughly speaking the first stage comprises the original theoretical motivations: Birth Cry theory in the west, quantum mechanics in the east and Europe. These provided above all a natural choice of objects to investigate: banded photons in one, charged corpuscles in the other. The second stage arises in part out of the first. A commitment to a type of instrumentation develops that is appropriate to the objects of investigation: electroscopes and cloud chambers in the case of the west coast, counters and coincidence circuits in that of Europe and the east coast. Finally, the instrumentation helps fashion the nature of persuasive evidence. Working in the corpuscle/counter tradition it is easy to see how Street and others could come to rely on statistical argumentation, shying away from the "golden event" exhibited in exceptional photographs. Similarly, given Anderson's spectacular success with the positron it is natural that he found the greatest demonstrative force in energy loss photographs. It also becomes clearer why he and Millikan argued so forcefully against the exclusive use of counters *à la* Rossi, Bothe and Kolhörster.

It would be of great interest to see how these two competing traditions continue their rivalry through the subsequent developments in particle physics: bubble chamber versus spark chambers, for example.<sup>142</sup>

I do not offer this three-layered analysis,

theory → instruments → style of demonstration,

as progressing in one direction only. It very often happens that the arrows need be drawn differently. In many cases instruments designed for one purpose led to revisions of the principles that motivated the instruments' construction.

In the present case the two traditions finally converged in their 1937 conclusion that there existed a new particle, previously unknown, of mass intermediate between that of the electron and of the proton. When the two groups reached a final consensus, it was at once a statement of theoretical and experimental physics. The discovery of the muon was inseparably bound to the resurrection of quantum electrodynamics.

## VII. Epilogue

The acceptance of Anderson and Neddermeyer's, and Street and Stevenson's new particle was greatly facilitated by an incorrect theoretical development. Yukawa had conjectured in 1935 that the nuclear forces might be due to the exchange of a heavy particle, by analogy to the exchange of the massless photon in quantum electrodynamics.<sup>143</sup> Since the mu-meson had approximately the same mass as the Yukawa particle, Oppenheimer and others suggested they were in fact the same particle.<sup>144</sup> A confirmation of this identification seemed to come in 1938–39 when observations of mu-meson flux at different altitudes indicated the mesons were decaying in flight approximately as they were predicted to do in Yukawa's theory. During the war, the decay of the mu-meson was confirmed in laboratory experiments.

Only after the war was over, when new methods of particle detection had been developed, were the mu-meson and the pion (the "true" Yukawa particle) conclusively distinguished. With this last development and the almost simultaneous transformation of quantum field theory by Feynman, Schwinger, and Tomanaga, elementary particle physics had begun. The cosmic ray process then was understood to be: protons produce pions in nuclear collisions, pions decay to muons and muons to electrons and neutrinos.

After the discovery of the muon, Rossi, Street, and Anderson continued to work on cosmic ray problems. Millikan, however, never fully abandoned his Birth Cry theory. By 1939, he was forced to acknowledge the existence of cosmic ray particles too energetic to be accounted for by energy conversion from atom-building. At the same time he inaugurated a new transformation in the cosmos to account for the cosmic ray particles, in which whole atoms were continually annihilated throughout the universe, producing high energy pairs of photons and electrons. Millikan's subsequent modifications of this last theory continued until his death, but he was drifting further and further from the mainstream of modern physics.

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### REFERENCES

1. G. Bernardini, talk given at the International Symposium on the History of Particle Physics, Fermilab, May 28–31, 1980.
2. J. A. Wheeler, "Some Men and Moments in the History of Nuclear Physics: The Interplay of Colleagues and Motivations," in R. H. Stuewer, ed., *Nuclear Physics in Retrospect*, Minneapolis 1977, pp. 213–322, on p. 242.
3. B. Rossi, *Cosmic Rays*, New York 1964, p. 109.
4. J. C. Street, "Cloud Chamber Studies of Cosmic Ray Showers and Penetrating Particles", *Journal of the Franklin Institute*, 227 (1939), pp. 765–788, on p. 778.
5. Abraham Pais, "Particles", *Physics Today*, 21, No. 5 (1968), pp. 24–28.
6. For more on Millikan's cosmic ray work see the following helpful sources:  
 Robert Kargon, "Birth Cries of the Elements: Theory and Experiment along Millikan's Route to Cosmic Rays", in *The Analytic Spirit*, ed., H. Woolf, Ithaca 1981.  
 Daniel Kevles, *The Physicists*, New York 1978, esp. pp. 179–80, 231 ff; *idem*, "Robert Millikan", *Scientific American*, January 1979, pp. 142–151.  
 Robert W. Seidel, *Physics Research in California: The Rise of a Leading Sector in American Physics*, Berkeley 1978. Unpublished Ph.D. dissertation Univ. of Calif., esp. ch. VII, "Cosmic Rays".
7. Millikan and his collaborators outlined their program for studying the energy and direction of cosmic rays in a three part series:  
 R. A. Millikan and I. S. Bowen, "High Frequency Rays of Cosmic Origin I. Sounding Balloon Observations at Extreme Altitudes", *Physical Review*, 27 (1926), pp. 353–361.  
 R. A. Millikan and R. M. Otis, "High Frequency Rays of Cosmic Origin II. Mountain Peak and Airplane Observations", *Physical Review*, 27 (1926), pp. 645–658.  
 R. A. Millikan and G. H. Cameron, "High Frequency Rays of Cosmic Origin III. Measurements in Snow-Fed Lakes at High Altitudes", *Physical Review*, 28 (1926), pp. 851–868.

8. J. H. Jeans, *Problems of Cosmogony and Stellar Dynamics*, Cambridge 1919, p. 286; A. S. Eddington, *The Internal Constitution of the Stars*, Cambridge 1926, ch. XI. Both cited in R. A. Millikan and G. H. Cameron, "Evidence for the Continuous Creation of the Common Elements out of Positive and Negative Electrons", *National Academy of Sciences, Proceedings*, 14 (1928), pp. 445–450; continued in R. A. Millikan and G. H. Cameron, "Evidence that the Cosmic Rays Originate in Interstellar Space", *Ibid.*, pp. 637–641, on p. 639. Also see Kargon, "Birth Cries", (ref. 6) for more on Millikan's early ideas on radioactivity.
9. For more on early, pre-quantum mechanical ideas about fusion see J. Bromberg, "The Concept of Particle Creation Before and After Quantum Mechanics", *Historical Studies in the Physical Sciences*, 7 (1976), pp. 161–191, esp. pp. 165 and 171 ff.
10. Millikan and Cameron, "Origin III" (ref. 7), p. 868.
11. *Ibid.*
12. R. A. Millikan and G. H. Cameron, "New Precision in Cosmic-Ray Measurements; Yielding Extension of Spectrum and Indications of Bands", *Physical Review*, 31 (1928), pp. 921–930.
13. *Ibid.*, p. 926.
14. *Ibid.*, p. 927.
15. *Ibid.*
16. R. A. Millikan and G. H. Cameron, "The Origin of the Cosmic Rays", *Physical Review*, 32 (1928), pp. 533–557.
17. *Ibid.*
18. R. A. Millikan and G. Cameron, "Interstellar Space", (ref. 8).
19. Millikan and Cameron, "Origin" (ref. 16).
20. Millikan and Cameron replaced the Compton formula (modified to give a mass absorption law for light) with an analogous mass absorption law based on Dirac's equation. Otherwise the theory of absorption was the same.
21. Letter from Oppenheimer to Millikan, 12 February 1929. Millikan microfilm, roll 23, file 22.1, American Institute of Physics, Niels Bohr Library.
22. Letter from Millikan to Oppenheimer, 11 March 1929. Millikan microfilm (ref. 21), roll 23, file 22.1.
23. Interview of C. D. Anderson, 30 June 1966, by C. Weiner, Transcript at American Institute of Physics, p. 7.
24. R. Kargon, "Birth Cries" (ref. 6).
25. Robert W. Seidel, *Physics in California* (ref. 6).
26. R. A. Millikan, *Science and Life*, Boston 1924, p. 43. See also D. J. Kevles, "Robert A. Millikan" (ref. 6).
27. R. A. Millikan, *Science and Life* (ref. 26) p. 59. (Emphasis in original.)
28. This kind of speculation about inorganic evolution was not unique to Millikan; Eddington, for instance, thought along similar lines. See the discussion in J. Bromberg, "Particle Creation" (ref. 9) note 39 and R. Kargon, "Birth Cries" (ref. 6).
29. R. A. Millikan, "The Significance of Radium", Address to National Museum, Washington, D.C. reprinted in *Science and Life* (ref. 26) pp. 13–37, on pp. 27–28.
30. R. A. Millikan, *Evolution in Science and Religion*, Port Washington 1973, p. 81.
31. Millikan and Cameron, "Bands" (ref. 12), p. 922.
32. G. Holton, "Subelectrons, Presuppositions, and the Millikan-Ehrenhaft Dispute", in *The Scientific Imagination*, Cambridge 1979, pp. 25–83. For another discussion of Millikan's



- treatment of data see A. Franklin, "Millikan's Published and Unpublished Data on Oil Drops", *Historical Studies in the Physical Sciences*, 11 (1981), pp. 185–201.
33. *Ibid.*, pp. 78–79.
  34. C. D. Anderson, "Early Work on the Positron and Muon", *American Journal of Physics*, 29 (1961), pp. 825–830, on p. 825.
  35. *Ibid.*
  36. Letter from Anderson to Millikan, 3 November (1931). Millikan microfilm, roll 23, file 22.3 (ref. 21).
  37. R. A. Millikan and C. D. Anderson, "Cosmic-Ray Energies and their Bearing on the Photon and Neutron Hypotheses", *Physical Review*, 40 (1932), pp. 325–328, on p. 327.
  38. *Ibid.*
  39. *Ibid.*
  40. C. D. Anderson, "Unravelling the Particle Content of the Cosmic Rays, including Discovery of the Positron and Mu Meson", Talk given at the International Symposium on Particle Physics, Fermilab, 28–31 May 1980. Also, interview with C. D. Anderson, 27 October 1982.
  41. Anderson interview with C. Weiner, *op.cit.* (ref. 23).
  42. C. D. Anderson, "The Apparent Existence of Easily Deflectable Positives", *Science*, 76 (1932), p. 238; C. D. Anderson, "Cosmic Ray Positive and Negative Electrons", *Physical Review*, 44 (1933), pp. 406–416.
  43. P. M. S. Blackett and G. M. S. Occhialini, "Some Photographs of the Tracks of Penetrating Radiation", *Royal Society of London, Proceedings*, 139A (1933), pp. 699–720.
  44. C. D. Anderson, "Positive Electrons" (ref. 42).
  45. *Ibid.*, p. 415.
  46. R. A. Millikan, "New Techniques in the Cosmic-Ray Field and Some of the Results Obtained with Them", *Physical Review*, 43 (1933), pp. 661–666, on p. 662.
  47. *Ibid.* Emphasis in original.
  48. R. A. Millikan, "Sur les Rayons Cosmiques", *Annales de l'Institut Henri Poincaré*, 3 (1932), pp. 447–464, on pp. 451–452.
  49. *Ibid.*
  50. *New York Times*, 5 February 1933, p. 1, cited in Kevles, *Physicists* (ref. 6), p. 242.
  51. A. H. Compton, "A Geographic Study of Cosmic Rays", *Physical Review*, 43 (1933), pp. 387–403.
  52. Millikan later contended that his theory too could admit some latitude effects, since the primary photons could knock off some electrons from interstellar matter. As he wrote to Compton in late November of 1932, "Without modifying in any way anything I have ever written I can admit the possibility of some equatorial latitude effects so that we can appear before the public as not having got contradictory experimental findings." Letter from Millikan to Compton, 30 November 1932. Millikan microfilm, roll 23, file 22.18. Nonetheless, as late as 1936, Millikan commented that Neher's results "make it look as though there were no latitude effect at all, or if any a very small one . . ." Letter from Millikan to Victor Neher, 12 September 1936. Millikan microfilm, roll 24, file 22.15. (ref. 21). Neher had been a Ph.D. student of Millikan's at Cal Tech, and in 1936 was an instructor in physics.
  53. W. Bothe and W. Kolhörster, "Das Wesen der Höhenstrahlung", *Zeitschrift für Physik*, 56 (1929), pp. 751–777, on p. 751.
  54. Interview with Bruno Rossi, 25 September 1980.

55. B. Rossi, "Method of Registering Multiple Simultaneous Impulses of Several Geiger's Counters", *Nature*, 125 (1930), p. 636.
56. Rossi's results are summarized in "Über die Eigenschaften der durchdringenden Korpusskularstrahlung im Meeresniveau", *Zeitschrift für Physik*, 82 (1933), pp. 151–178.
57. Millikan, "New Techniques" (ref. 46), p. 663.
58. Carl Anderson, R. A. Millikan, Seth Neddermeyer, and William Pickering, "The Mechanism of Cosmic-Ray Counter Action", *Physical Review*, 45 (1934), pp. 352–363.
59. *Ibid.*, p. 352 (Emphasis in original.)
60. *Ibid.*
61. Blackett and Occhialini, "Photographs" (ref. 43).
62. L. Brown, "Yukawa's Prediction of the Meson", *Centaurus*, 25 (1981), pp. 71–132. See also articles by J. Wheeler and H. Bethe in *Nuclear Physics in Retrospect* (ref. 2).
63. D. C. Cassidy, "Cosmic Ray Showers, High Energy Physics, and Quantum Field Theories: Programmatic Interactions in the 1930's", *Historical Studies in the Physical Sciences*, 12 (1981), pp. 1–39.
64. See the excellent article by J. L. Heilbron, "The Scattering of  $\alpha$  and  $\beta$  Particles and Rutherford's Atom", *Archive for History of Exact Sciences*, 4 (1968), 247–307. For a discussion of the role of Bohr's work and its relation to his development of the Bohr atomic theory, see J. L. Heilbron and T. S. Kuhn, "The Genesis of the Bohr Atom", *Historical Studies in the Physical Sciences*, 1 (1969), pp. 211–290, esp. 237 ff.
65. N. Bohr, "On the Theory of the Decrease of Velocity of Moving Electrified Particles on Passing Through Matter", *Philosophical Magazine*, 25 (1913), pp. 10–31, and "On the Decrease of Velocity of Swiftly Moving Electrified Particles in Passing Through Matter", *Philosophical Magazine*, 30 (1915), pp. 581–612.
66. Bohr made use of the earlier work by J. J. Thomson and C. G. Darwin:  
J. J. Thomson, "Ionization by Moving Electrified Particles", *Philosophical Magazine*, 23 (1912), pp. 449–457.  
C. G. Darwin, "A Theory of the Absorption and Scattering of the  $\alpha$ -Rays", *Philosophical Magazine*, 23 (1912), pp. 901–920. See Heilbron, " $\alpha$  and  $\beta$ " (ref. 64) and Kuhn and Heilbron, "Bohr" (ref. 64).
67. This discussion is based on a condensation of pp. 10–16 of N. Bohr, "Theory of Decrease" (ref. 65) and sections 1 and 3 of N. Bohr, "Decrease of Velocity" (ref. 65). See also J. D. Jackson, *Classical Electrodynamics*, New York 1975, pp. 619ff and S. P. Ahlen, "Theoretical and Experimental Aspects of the Energy Loss of Relativistic Heavily Ionizing Particles", *Reviews of Modern Physics*, 52 (1980), pp. 121–173. Compare J. A. Wheeler, "Men and Moments" (ref. 2), pp. 242 ff.
68. R. H. Fowler, "A Theoretical Study of the Stopping Power of Hydrogen Atoms for  $\alpha$ -Particles", *Cambridge Philosophical Society Proceedings*, 22 (1925), pp. 793–803.
69. G. H. Henderson, "The Decrease of Energy of  $\alpha$  Particles on Passing Through Matter", *Philosophical Magazine*, 44 (1922), pp. 680–688.
70. J. A. Gaunt, "The Stopping Power for  $\alpha$ -Particles According to the New Quantum Theory", *Cambridge Philosophical Society Proceedings*, 23 (1925–7), pp. 732–754.
71. J. Bernstein, *Hans Bethe, Prophet of Energy*, New York 1980, p. 20.
72. *Ibid.*, p. 25.
73. H. Bethe, "Zur Theorie des Durchgangs schneller Korpuskularstrahlen durch Materie", *Annalen der Physik*, 5 (1930), pp. 325–400.

74. *Ibid.*, pp. 325–6.
75. Interview with H. Bethe, 11 December 1980.
76. H. Bethe, “Bremsformel für Elektronen relativistischer Geschwindigkeit”, *Zeitschrift für Physik*, 76 (1932), pp. 293–299.
77. E. J. Williams, “Application of the Method of Impact Parameter in Collisions”, *Royal Society Proceedings*, 139A (1933), pp. 163–186. C. F. v. Weizsäcker, “Ausstrahlung bei Stößen sehr schneller Elektronen”, *Zeitschrift für Physik*, 88 (1934), pp. 612–625.
78. E. Bethe Interview (ref. 75).
79. *Ibid.*
80. Bernstein, *Bethe* (ref. 71), p. 38.
81. Bethe Interview (ref. 75).
82. W. Heitler, “Über die bei sehr schnellen Stößen emittierte Strahlung”, *Zeitschrift für Physik*, 84 (1933), pp. 145–167; W. Heitler and F. Sauter, “Stopping of Fast Particles with Emission of Radiation and the Birth of Positive Electrons”, *Nature*, 132 (1933), p. 892. Before Heitler’s work, Bethe had only considered two stopping processes: ionizing/excitation and nuclear scattering. Both processes became less likely with increasing projectile energy.
83. Letter: Heitler to Bohr, 16 October 1933, BSC roll 20. BSC = Bohr Scientific Correspondence, microfilm copy on deposit in the American Institute of Physics.
84. Heitler and Sauter. “Stopping” (ref. 82), p. 892.
85. Bethe interview (ref. 75). Bethe’s idea was that the weaker (screened) nuclear electric field would cause the projectile to radiate less than was previously estimated – the particle could therefore penetrate further through matter.
86. H. Bethe and W. Heitler, “On the Stopping of Fast Particles and on the Creation of Positive Electrons”, *Proceedings of the Royal Society, A* 146 (1934), pp. 83–112.
87. Bethe and Heitler, “Stopping and Creation” (ref. 86), p. 103.
88. *Ibid.*, pp. 104–105.
89. C. D. Anderson and S. Neddermeyer, “Energy-Loss and the Production of Secondaries by Cosmic-Ray Electrons”, *Physical Review*, 46 (1934), p. 325.
90. C. D. Anderson and S. H. Neddermeyer, “Fundamental Processes in the Absorption of Cosmic-Ray Electrons and Photons”, *International Conference on Physics London 1934*, Cambridge 1935, pp. 171–187, on p. 181.
91. Letter: C. D. Anderson to H. Bethe 7 June 1935, Bethe Collection, Cornell University Archives.
92. Anderson and Neddermeyer, *London* (ref. 90), p. 181.
93. A. H. Compton and H. A. Bethe, “Composition of Cosmic Rays”, *Nature*, 134 (1934), pp. 734–735.
94. Anderson and Neddermeyer, *London* (ref. 90), p. 182.
95. At low energies, the results of Chadwick, Blackett and Occhialini and the early results of Anderson were now seen by Anderson as “combin[ing] to show the success of the Dirac theory as developed by Oppenheimer and Plessett and by Heitler and Sauter in interpreting the results obtained for photon energies of 2.6 MeV.” Anderson and Neddermeyer, (ref. 90), p. 183. Compare Anderson’s earlier papers with Millikan where they criticized the Blackett and Occhialini work and supported the Millikan “Birth Cry Theory” of nuclear positron and electron emission (ref. 58).
96. Discussion in *London* (ref. 90), p. 250.

97. Letter: Weizsäcker to Bethe, 5 December 1934, Bethe Archives (ref. 91). Bethe's "reversal" was this: soon after the conference, he and Compton concluded that the latitude effect, east west effect, and penetrating particles all could be accounted for if one supposed the charged cosmic rays to predominantly include protons. Quantum electrodynamics would thus have been vindicated. See Compton and Bethe, "Composition" (ref. 93).
98. A. Smith and C. Weiner, *Robert Oppenheimer, Letters and Reflections*, Cambridge 1980, p. 161.
99. Letter: Oppenheimer to Bohr, 14 June 1933, *Ibid.*, pp. 161–162.
100. Letter: Oppenheimer to F. Oppenheimer, 7 October 1933. *Ibid.*, p. 164.
101. Letter: Oppenheimer to Uhlenbeck, Fall 1933. *Ibid.*, pp. 167–168.
102. Letter: Oppenheimer to F. Oppenheimer, 4 June 1934. *Ibid.*, pp. 181.
103. J. R. Oppenheimer, "Are the Formulae for the Absorption of High Energy Radiation Valid?", *Physical Review*, 47 (1934), pp. 44–52.
104. *Ibid.*, p. 45.
105. M. Born, "On the Quantum Theory of the Electromagnetic Field", *Royal Society Proceedings*, 143 (1934), pp. 410–437.
106. W. H. Furry and J. F. Carlson, "Production of High Energy Electron Pairs", *Physical Review*, 45 (1934), pp. 237–238, on p. 237.
107. Letter: N. Bohr to E. J. Williams, 11 February 1935. B.S.C. (ref. 83), microfilm roll 26.4.
108. C. D. Anderson, "Early Work" (ref. 34), p. 820.
109. Interview with J. C. Street, October 1979; Rossi Interview (ref. 54).
110. P. M. S. Blackett, *Cosmic Rays*, The Halley Lecture 5 June 1936, Clarendon 1936, p. 23.
111. See Cassidy "Showers" (ref. 63).
112. Street Interview (ref. 109).
113. B. Rossi, "On the Magnetic Deflection of Cosmic Rays", *Physical Review*, 36 (1930), p. 606.
114. T. H. Johnson, "The Azimuthal Asymmetry of the Cosmic Radiation", *Physical Review*, 43 (1933), pp. 834–835.
115. L. Alvarez and A. H. Compton, "A Positively Charged Component of Cosmic Rays", *Physical Review*, 43 (1933), pp. 835–836.
116. B. Rossi, "Directional Measurement on the Cosmic Rays Near the Geomagnetic Equator", *Physical Review*, 45 (1934), pp. 212–214. Cf. Rossi, *Ricerca Scientifica*, IV(2) (1933), p. 365.
117. Street Interview (ref. 109).
118. *Ibid.*
119. *Ibid.*
120. E. C. Stevenson and J. C. Street, "Nature of the Penetrating Cosmic Radiation at Sea Level", *Physical Review*, 47 (1935), p. 643.
121. J. C. Street and R. H. Woodward, "Production and Absorption of Cosmic-Ray Showers", *Physical Review*, 47 (1935), p. 800.
122. J. C. Street, R. H. Woodward, and E. C. Stevenson, "The Absorption of Cosmic-Ray Electrons", *Physical Review*, 47 (1935), pp. 891–895.
123. J. C. Street and E. C. Stevenson, "Design and Operation of the Counter Controlled Cloud Chamber", *Physical Review*, 49 (1936), pp. 425–428.
124. C. D. Anderson and Seth H. Neddermeyer, "Cloud Chamber Observations of Cos-

- mic-Rays at 4300 Meters Elevation and Near Sea-Level", *Physical Review*, 50 (1936), pp. 263–271.
125. Letter: C. D. Anderson to W. Heitler, 21 May 1936 Bethe Archives (ref. 91).
  126. J. F. Carlson and J. R. Oppenheimer, "On Multiplicative Showers", *Physical Review*, 51 (1936), pp. 220–221. Shower calculations were independently done by H. J. Bhabha and W. Heitler, "Passage of Fast Electrons through Matter", *Nature*, 138 (1936), p. 401.
  127. Carlson and Oppenheimer, "Showers" (ref. 126), p. 220.
  128. *Ibid.*, p. 221.
  129. C. D. Anderson, "The Production and Properties of Positrons", *Nobel Lectures Physics 1922–1941*, Amsterdam 1965, pp. 365–377, on p. 372.
  130. S. H. Neddermeyer and C. D. Anderson, "Note on the Nature of Cosmic-Ray Particles", *Physical Review*, 51 (1937), pp. 884–886.
  131. *Ibid.*, p. 886.
  132. L. Fussell, "Production and Absorption of Cosmic-Ray Showers", *Physical Review*, 51 (1937), pp. 1005–1006.
  133. J. C. Street and E. C. Stevenson, "Penetrating Corpuscular Component of the Cosmic Radiation", *Physical Review*, 51 (1937), p. 1005. Also in 29 April *Bulletin of the American Physical Society*, 12 (1937), p. 13, abstract 40.
  134. Anderson and Neddermeyer, "4300 meters" (ref. 124).
  135. Letter: James H. Bartlett to J. C. Street, 5 May 1937. Street private files.
  136. Letter: J. C. Street to James H. Bartlett 11 May 1937. Street private files.
  137. Bethe Interview (ref. 75); Interview with W. Furry 11 November 1980.
  138. J. C. Street and E. C. Stevenson, "New Evidence for the Existence of a Particle of Mass Intermediate Between the Proton and Electron", *Physical Review*, 52 (1937), pp. 1003–1004. Reproduced, for example, in Rossi *Cosmic Rays* (ref. 3), p. 105.
  139. Street Interview (ref. 109).
  140. *Ibid.*
  141. Letter: J. C. Street to W. Furry, no date. Street private files.
  142. This is discussed in a detailed case study on the discovery of neutral currents in the 1970's. See P. Galison, "How the First Neutral Current Experiments Ended", *Reviews of Modern Physics*, 55 (1983), pp. 477–509.
  143. See L. Brown, "Yukawa's Meson", (ref. 62).
  144. J. R. Oppenheimer and R. Serber, "Note on the Nature of Cosmic-Ray Particles", *Physical Review*, 51 (1937), p. 1113.