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Strategies of Detection: Interpretive Methods in Experimental Particle Physics, 1930–1950

ABSTRACT

Between 1930 and 1950 experimental physicists used cloud chambers, coincidence counters, and nuclear emulsions to study both cosmic rays and radioactive processes. In order to identify what particles they were detecting and to measure their properties, these physicists employed a variety of interpretive strategies. Their choice of strategies depended upon what task they were trying to perform, and what instrument they were using. It is argued that different strategies could be employed using the same instrument, that the same strategy could be used with different instruments, and that different strategies could be used in combination with each other. Analyzing the history of the use of these strategies permits a deeper understanding of how physicists designed experiments and used evidence in drawing conclusions. Attending to the patterns of strategy use also permits new periodizations to be developed in the history of particle physics. In the timeframe considered, it is argued that inferential strategies were used to interpret single images of particle tracks, that evidence aggregation was crucial using all kinds of detectors, and that it was also common to use nuclear physics knowledge to narrow the range of possible interpretations. Beginning in the late 1940s, precision measurement, precision experiment design, and decay mode analysis became prominent strategies in the systematic search for new particles. This history builds on and revises Peter Galison's history of particle detection practices, which is based on the distinct epistemological ideals he supposes drove experimentation in the "image" and "logic" traditions of detector instrumentation.

KEY WORDS: cosmic radiation, mesotrons, particle detectors, epistemological ideals, Carl Anderson, Patrick Blackett, Robert Marshak, Giuseppe Occhialini, Cecil Powell

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The following abbreviations are used: AIP/NBLA, American Institute of Physics, Niels Bohr Library and Archives, College Park, MD; FAS, Federation of American Scientists; *PR*, *Physical Review*.

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Peter Galison's 1997 book *Image and Logic* stands as one of the great methodological statements of the recent historiography of science. Together its first and last chapters comprise 126 pages of ideas about how new histories of science might be written, and how those ideas inform the history of particle detection that the rest of the book contains.[1] Some of these ideas have been widely cited: scholars in science studies and beyond have made particularly extensive use of his "trading zone" metaphor to explore how ideas and practices move across epistemic boundaries.[2] However, few authors have substantially engaged with Galison's ideas concerning how epistemology and history relate to each other, how the historical analysis of practice can escape the limitations of the case-study format, and how histories of scientific practices can relate to intellectual histories of science. Moreover, in spite of Galison's attempts to open new paths in the history of particle physics, the organized historiography of the subject has more or less shut down following his contribution.[3] This article intensively engages with both the methodological elements of *Image and Logic* and the history the book presents by offering a deeper account of experimental and interpretive strategies employed in particle detection, and by suggesting a revised and refined periodization of the history of particle detection practices. First, however, a brief discussion of Galison's ideas will clarify how he assembled his history, and how the present history adds to and departs from it.

EPISTEMOLOGICAL IDEALS VERSUS STRATEGIES OF DETECTION

The synthetic history of particle detection found in *Image and Logic* had its roots in methodological insights Galison had developed a decade earlier in his book *How Experiments End*. That book sought to liberate historical accounts of physics experimentation from histories of the development of physical theory. According to Galison, existing accounts were overly constrained by philo-

1. Peter Galison, *Image and Logic: A Material Culture of Microphysics* (Chicago: University of Chicago Press, 1997).

2. A recent example is Michael E. Gorman, ed., *Trading Zones and Interactional Expertise: Creating New Kinds of Collaboration* (Cambridge, MA: MIT Press, 2010).

3. An important exception is the work of Daniela Monaldi; see esp. Daniela Monaldi, "Life of μ: The Observation of the Spontaneous Decay of Mesotrons and Its Consequences, 1938–1947," *Annals of Science* 62, no. 4 (2005): 419–55, and Daniela Monaldi, "The Indirect Observation of the Decay of Mesotrons: Italian Experiments on Cosmic Radiation, 1937–1943," *HSNS* 38, no. 3 (2008): 353–404.

sophical visions of how experiment related to theory development. On the one hand, positivistic accounts portrayed experiments as a raw source of experience, which theories could explain. On the other hand, anti-positivistic accounts portrayed experiments as resources to be used in the validation or falsification of theoretical claims. Even in more detailed, social constructionist accounts of experimental practice, the emphasis still seemed to be on what local factors caused experimenters to interpret underdetermined results in favor of one or another theoretical predilection. By contrast, Galison argued that experimenters' interpretations actually abided by standards that were not strongly linked to theoretical ideas. The nature of these standards could therefore not be derived from existing philosophical models. They could only be accessed through detailed historical research.[4]

Galison found from his own research that scientists working in the same field have actually held differing interpretive standards. In early work on the discovery of the muon, or μ particle, he used the observation that no single standard of discovery existed in order to criticize the idea that a particular moment of discovery could ever be philosophically ascertained.[5] Instead, in *How Experiments End*, he supposed that discovery could be described as a "circle of belief" surrounding a claim, which widened over time as individual scientists were persuaded of the discovery's reality by whatever evidence they found persuasive. Having rejected philosophical claims that experiments adjudicated theory choice, as well as the constructionist view of experimental interpretation as the product of "anarchic" circumstances, Galison instead embraced continuities in the subjective experience of being persuaded as the foundation for both a new history and epistemology of experiment. In particular, he supposed that scientists' propensity to be persuaded by evidence was heavily influenced

4. Peter Galison, *How Experiments End* (Chicago: University of Chicago Press, 1987). On positivism and anti-positivism and theory-centered history, see esp. 6–13; on the role of the historian, see esp. 277. For elaboration, see Galison, *Image and Logic* (ref. 1), ch. 9, pt. I.

5. Peter Galison, "The Discovery of the Muon and the Failed Revolution against Quantum Electrodynamics," *Centaurus* 26, no. 3 (1982): 262–316. The concept of "discovery" was under substantial scrutiny at that time. The canonical reference is T. S. Kuhn, "The Historical Structure of Scientific Discovery," *Science* 136, no. 136 (1962): 760–64, reprinted in Thomas S. Kuhn, *The Essential Tension: Selected Studies in Scientific Tradition and Change* (Chicago: University of Chicago Press, 1977), 165–77. See also S. W. Woolgar, "Writing an Intellectual History of Scientific Development: The Use of Discovery Accounts," *Social Studies of Science* 6, nos. 3–4 (1976): 395–422; Augustine Brannigan, *The Social Basis of Scientific Discoveries* (New York: Cambridge University Press, 1981); and Simon Schaffer, "Discovery and the End of Natural Philosophy," *Social Studies of Science* 16, no. 3 (1986): 387–420.

by that evidence's conformity to one or another *epistemological ideal* to which the scientists were aesthetically committed.[6]

By *Image and Logic* Galison had assigned these epistemological ideals several important features. First, they are not abstract, philosophical entities. Rather, they are closely linked to scientists' lived experience: their education, their working practices, and their choice of instrumentation. Second, these ideals can be passed on and endure through history. Their durability allows them to establish consistent "constraints" on the interpretation of experiments, which prevents epistemological anarchy. Further, historians can establish "mesoscopic" periodizations of their endurance, thus transcending the limitations of localized microhistory. According to Galison, these periodizations will be independent of, and "intercalated" with, periodizations in histories of scientific theories. Third, differences in scientists' epistemological ideals can account for many scientific disagreements about the validity of an interpretation and about directions in which future scientific research should proceed. However, this same "disunity" of science is also a critical source of science's strength and progress. When individuals laboring under different ideals come to agreements about each other's claims—when those claims are stripped of their more localized, practice-embedded connotations—it testifies to the validity of those claims. Further, as scientists' practices evolve to accommodate changes in instrumentation and the kinds of problems they study, epistemological ideals can themselves evolve and hybridize.[7] For all

6. On "circle" or "circles" of belief, see Galison, *Experiments* (ref. 4), 276–77; "anarchic" on 258. The idea that idealist, aesthetic criteria can inform judgment between theories is argued in Gerald Holton, *Thematic Origins of Scientific Thought: Origins of Relativity and Other Essays* (Austin: University of Texas Press, 1973). Such aesthetic criteria informed even Galison's earliest work; see Peter Galison, "Minkowski's Space-Time: From Visual Thinking to the Absolute World," *HSPS* 10 (1979): 85–121. Galison has continued to suppose that these arguments describe theoretical practice as well as the criteria used for judging theory validity; see Peter Galison, "Feynman's War: Modelling Weapons, Modelling Nature," *Studies in History and Philosophy of Modern Physics* 29, no. 3 (1998), 391–434, as well as *Image and Logic* (ref. 1), 826–27, on Julian Schwinger's professed use of wartime theoretical practices in the development of renormalization.

7. On "constraints," see Galison, *Experiments* (ref. 4), 246–55; for elaboration, see Peter Galison, "Contexts and Constraints," in *Scientific Practice: Theories and Stories of Doing Physics*, ed. Jed Z. Buchwald (Chicago: University of Chicago Press, 1995), 13–41. On "mesoscopic" history, see Galison, *Image and Logic* (ref. 1), 55–62; for elaboration on philosophical "globalism" and minute "localism" in historiography, see Peter Galison, "Ten Problems in History and Philosophy of Science," *Isis* 99, no. 1 (2008): 111–24. On "intercalation," see *Image and Logic* (ref. 1), 14–19 and 797–803; on disunity, see ibid., 781–84, as well as Peter Galison and David J. Stump, eds., *The Disunity of Science: Boundaries, Contexts, and Power* (Stanford, CA: Stanford University Press,

these reasons, epistemological ideals become an attractive focus for historians' attention. It is unsurprising that, with Lorraine Daston, Galison has recently expanded on the idea, labeling these ideals "epistemic virtues," which define historically accepted concepts of "objectivity" across many disciplines, and so inform scientific figures' psychological and moral sense of what gives their work scientific integrity.[8]

In his history of particle detection, Galison specifically identified two central epistemological ideals, which he closely associated with "image" and "logic" traditions in detector instrumentation. According to him, physicists who used instruments in the image tradition—cloud chambers, nuclear emulsions, and bubble chambers—aspired to capture definitive evidence of a discovery in a single image, a "golden event." Physicists who used instruments in the logic tradition—coincidence counters, and their later successors such as spark chambers—aspired to capture statistically rigorous demonstrations of the reality of experimental phenomena. They were, therefore, unlikely to be persuaded by single observations, no matter their apparent clarity. It was only in the 1970s, with the development of new hybrid "postmodern" detectors such as the Time Projection Chamber, that the distinct epistemological strengths of the image and logic traditions were amalgamated.[9]

In this article, I build on two major elements of Galison's thinking. First, I agree that the ideas informing the design and interpretation of experiments are not well articulated, and require intensive scrutiny and characterization by historians. Second, I agree that these ideas endure and can be classified into a relatively small number of groups, and are therefore an attractive subject for mesoscopic historical accounts. However, I part ways with Galison in that I do not identify these ideas with aesthetic ideals. Instead, I argue that historians should attend to continuities in experimenters' work, and specifically to the *strategies of detection* they deemed critical for deriving legitimate—but not necessarily definitive—interpretations of results. Per Galison, these strategies are certainly tied to experimenters' choice of instrumentation and habits of work, but, contra Galison, I do not suppose any one-to-one-to-one correspondence between these strategies, experimenters' choices of instrumentation, and

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1996). On the stripping of meaning, see Galison's discussion of trading zones in *Image and Logic* (ref. 1), 46–55, and ch. 9, pt. II. According to Galison, "creole" languages that develop out of working "pidgins" in trading zones can become the basis for new hybrid traditions.

8. Lorraine Daston and Peter Galison, *Objectivity* (New York: Zone Books, 2007).

9. Galison, *Image and Logic* (ref. 1); for the "image and logic ideals," see section 1.4 (as well as section 2.7 for pre-image-and-logic ideals); "postmodern" on 554.

any evidentiary predilections they might have had. Instead, I argue that strategies are more modular: experimenters can employ different strategies using the same instrument; in some cases the same strategy could be employed using different instruments; and multiple strategies could be combined within a single experiment. Also, unlike Galison's ideals, these strategies have no intrinsic epistemological significance. Rather, experimenters must employ deeper (generally tacit) epistemological principles to choose which strategies should be used to accomplish particular tasks. For example, a different strategy might be used depending on whether an experimenter was trying to establish the existence of a particle, or the prevalence of that particle in the cosmic radiation.

We need not suppose that strategies of detection have fixed definitions, or that it is possible to enumerate a definitive set of them. They are simply a convenient description that historians can use to identify important continuities in experimenters' work. It is useful to identify them in the historical record because doing so forces us to attend to and debate what practices have been deemed critical to scientific work. Once we are satisfied that we have identified these practices, we can begin to develop narratives explaining why they arose, when they became prominent, what validated their utility, and whether and why they declined. In this article, I will examine three major strategies that were prominent between 1930 and 1950. The first two—inferring proper interpretations of individual observations, and drawing conclusions from a preponderance of evidence—somewhat resemble the epistemological ideals Galison associates with the image and logic traditions of instrumentation. The third—using knowledge of nuclear physics to narrow a range of possible interpretations—has no analogue in Galison's work. Identifying this third strategy allows compelling new links to be drawn between Galison's two instrumental traditions, as well as between nuclear physics and an incipient particle physics. Together, these three strategies also illuminate the peculiarities of interpretive practice in a period when physicists were reluctant to explain experimental results by supposing the existence of new particles. They also allow us to identify an important transition in experimental practice at the end of the 1940s, when new strategies associated with the analysis of particle "decay modes" arose. Using earlier strategies, experimenters usually operated under the assumption that new particles would not be found. Their new strategies aided them in the systematic hunt for large numbers of new particles using high-energy accelerators and increasingly sophisticated particle detectors.

STRATEGIES AT WORK: MESOTRONS, VARYTRONS, AND PIONS

Before establishing the critical features of the three strategies to be detailed in this article, it will be useful to illustrate how these strategies can matter by discussing circumstances surrounding the discovery of the pion, or π particle. It is well known that in 1935 the theoretical physicist Hideki Yukawa proposed a new particle that could serve as a carrier of a quantized force between the particles within an atomic nucleus. According to his theory, the particle would have to be short-lived and intermediate in mass between the proton and the electron. Yukawa's particle was suspected to be the same as an intermediate-mass particle identified in observations of cosmic rays two years later. Physicists quickly took it upon themselves to establish the identity of the theoretical "meson" with the observed "mesotron," to use a naming convention that was originally suggested by theorist Hans Bethe, and was widely followed through 1945.[10]

By the early postwar years, the situation had become quite complicated. During the war theorists had begun incorporating either a "mixed" meson, or a second, very short-lived meson into refined theories of nuclear forces.[11] Meanwhile, experimenters had begun to explain divergent measurements of mesotron mass in cloud chambers by supposing not that there was more than one intermediate-mass particle, but that the mesotron's rest mass might vary from instance to instance. This suggestion was rebuffed by Bethe, who insisted that the measurements could be reconciled with a particle of single mass. According to him, discovering a particle that did not have a constant rest mass would be "a tremendous deviation from previous experience," and that, as a matter of principle, the "burden of proof lies always with the discoverer of a *new* phenomenon."[12] Then, in 1947, experimenters in Italy produced measurements showing that negatively charged mesotrons did not strongly couple to positively charged nuclei as Yukawa's theory demanded.[13] This result led the

10. See especially Laurie Brown, "Yukawa's Prediction of the Meson," *Centaurus* 25, no. 1 (1981): 71–132. On the names of the particle, see Laurie M. Brown, "Nuclear Forces, Mesons and Isospin Symmetry," in *Twentieth Century Physics*, vol. 1, ed. Laurie M. Brown, Brian Pippard, and Abraham Pais (Woodbury, NY: American Institute of Physics Press, 1995), 357–420, on 396.

11. Visvapriya Mukherji, "A History of the Meson Theory of Nuclear Forces from 1935 to 1952," *Archive for History of Exact Sciences* 13, no. 1 (1974): 27–102.

12. H. A. Bethe, "Multiple Scattering and the Mass of the Meson," *PR* 70, nos. 11–12 (1946): 821–31.

13. The context of the mesotron-meson identity question for the Italian experiments is developed extensively in Monaldi, "Life" and "Indirect" (ref. 3); for further context on Bethe's admonition, see esp. Monaldi, "Life," 446.

theorist Robert Marshak to propose a two-meson theory at an elite conference held in very early June 1947 at Shelter Island, New York. In his theory, one long-lived meson, corresponding to Yukawa's particle, strongly coupled to nuclei; it decayed into another, even longer-lived meson, corresponding to the mesotron, which did not.

In a 1970 interview with the historian Charles Weiner, Marshak recalled that he had intended to write up a "note" on his two-meson hypothesis immediately following the conference. However, at the time he had also been serving as the chairman of the new Federation of American Scientists (FAS) and was sidetracked into atomic politics. In mid-June, en route to a conference on atomic affairs at Lake Geneva, Wisconsin, he ran into another conference attendee, theorist Philip Morrison. Morrison informed Marshak that in the May 24 issue of *Nature*, which had only just arrived from Britain, there was an article from Cecil Powell's experimental group at the University of Bristol.[14] The article described particle tracks indicating the presence of an intermediate-mass particle (later called the pion) that decayed into a "secondary" intermediary-mass particle. Marshak recalled that upon seeing the paper for himself after the Lake Geneva conference, "I decided, chairman of the FAS be damned, I was going to get that paper written up."[15] Working with Bethe, he quickly sent an account of his hypothesis and the expected characteristics of the two mesons to the *Physical Review*.[16]

Marshak and Bethe gladly cited the evidence from the Bristol group in support of the two-meson hypothesis, but they also mentioned in a footnote alternative evidence for multiple intermediate-mass particles that had recently arrived from an experimental group in the Soviet Union. That group had used vertically separated "trays" of coincidence counters to make a rough track of particles moving through a magnetic field at high altitudes, permitting some four thousand mass measurements to be made that, the Soviets claimed, could not be interpreted as mesotrons or any other known particle. They grouped these particles under the unitary name "varytron."[17]

14. C. M. G. Lattes, H. Muirhead, G. P. S. Occhialini, and C. F. Powell, "Processes Involving Charged Mesons," *Nature* 159, no. 4047 (1947): 694–97.

15. Robert Marshak, interview by Charles Weiner, 16 Jun 1970, transcript at AIP/NBLA, online at http://www.aip.org/history/ohilist/4760_2.html (accessed 29 Jul 2012).

16. R. E. Marshak and H. A. Bethe, "On the Two Meson Hypothesis," *PR* 72, no. 6 (1947): 506–09.

17. A. Alichanian, A. Alichanow, and A. Weissenberg, "On the Existence of Particles with a Mass Intermediate between Those of Mesotron and Proton," *Journal of Physics (USSR)* 11, no. 1 (1947): 97–99.

Marshak and Bethe's paper observed that "this evidence appears less convincing than that of the British, and will not be used in our discussion."[18] Marshak elaborated to Weiner that mentioning the Soviet work "was just a courtesy." He explained, "The Russians were constantly finding lots of intermediate mass particles … but they had small numbers of particles with all kinds of masses." He allowed, "Maybe some of the heavier mesons are pions but they had many more [particle masses] than are known even now." Even had they detected the new meson, the lack of control over their measurements would have made the evidence impossible to interpret meaningfully: "It was pretty poor stuff."[19]

Cecil Powell would receive the 1950 Nobel Prize in Physics for his discovery of the pion, and the pion would be only the first of a host of particles discovered after the war. However, it is not immediately clear why, in that moment of postwar confusion, the Bristol group's evidence was able to bring new clarity as swiftly as it did. The emulsions that they used were hardly an established technology: the pion discovery was the technology's first high-profile success. Further, the group was unable to settle on any sort of definitive statement as to how many different particles appeared in their emulsions, and they certainly had no means of providing precise mass measurements. To understand what exactly distinguished the Bristol results from the Soviets' "pretty poor stuff" for Marshak, and how it set the stage for subsequent discoveries, it is necessary to understand how the group made use of—and were in the process of transforming—the strategies of detection that had been developed to that point.

INFERENCE IN INDIVIDUAL OBSERVATIONS

The most critical strategy of detection is to infer what is, or might be, taking place within any particular observation. The development of cloud chambers (or "Wilson chambers," as they were often called up to the 1930s) established a means of photographing the tracks that individual charged particles had followed. As Clinton Chaloner has argued, in the 1910s and '20s the astounding clarity of cloud chamber images made the device ideal for corroborating ideas about the physical properties of subatomic particles and ionizing radia-

18. Marshak and Bethe, "Hypothesis" (ref. 16).
19. Marshak, interview (ref. 15).

tion, which were otherwise studied using the flashes of scintillation counters. Peter Galison has similarly argued that at that time cloud chambers established an enduring "homomorphic" ideal in nuclear and subatomic physics, that is, they were understood to represent natural processes faithfully when regarded with a "tutored eye."[20] This section argues that as cloud chambers were turned to the study of cosmic ray particles in the 1930s, their photographic integrity actually proved insufficient to establish crucial measurements of those particles. Instead, not only measurements, but the identification of particles, relied deeply on assumptions about what those particles were. Conscious of these assumptions, experimenters were fully willing to acknowledge the tentativeness of their conclusions. This attitude allowed experimenters to gain confidence in their ideas not by definitively verifying them, but by seeing how well they served as assumptions in subsequent experiments.

As an example of how this sort of inference worked in practice, we can look to Carl Anderson's early research on cosmic rays, and how his discovery of the positron fit in with them. When Anderson took up a research fellowship at the California Institute of Technology in 1930, having just completed his PhD there, the nature of cosmic rays was in serious dispute. Robert Millikan, the doyen of physics at the institute, believed measurements of the composition and energy spectrum of the rays would support his theory that they were high-energy photons created in the interstellar synthesis of elements: they were the elements' "birth cry."[21] Therefore, at Millikan's recommendation, Anderson constructed a cloud chamber expressly to test these ideas.[22] If photons were created in elemental synthesis, they would be found to occupy characteristic energy "bands," which could be observed by measuring the energies

20. Clinton Chaloner, "The Most Wonderful Experiment in the World: A History of the Cloud Chamber," *British Journal for the History of Science* 30, no. 3 (1997): 357–74; on the realism of photography and the "tutored eye," see Galison, *Image and Logic* (ref. 1), section 2.6, as well as Lorraine J. Daston and Peter Galison, "The Image of Objectivity," *Representations* 40 (1992): 81–128; and Daston and Galison, *Objectivity* (ref. 8), ch. 6.

21. Robert Kargon, "Birth Cries of the Elements: Theory and Experiment along Millikan's Route to Cosmic Rays," in *The Analytic Spirit: Essays in the History of Science in Honor of Henry Guerlac*, ed. Harry Woolf (Ithaca, NY: Cornell University Press, 1981), 309–29.

22. The definitive account of the events leading up to Anderson's discovery is Michelangelo De Maria and Arturo Russo, "The Discovery of the Positron," *Rivista di Storia della Scienza* 2, no. 2 (1985): 237–86. They expand on Norwood Russell Hanson, *The Concept of the Positron* (Cambridge: Cambridge University Press, 1963), which is a more philosophical account querying why the positron had not previously been identified, and emphasizes the importance of pairing it with Dirac's theory. Incidentally, their paper, like Galison, "Discovery" (ref. 5), concludes with a criticism of the concept of discovery.

of particles scattered when these photons interacted with atmospheric molecules.[23]

However, cosmic rays had only been photographed in a cloud chamber for the first time in 1927 by Dmitri Skobeltsyn, and the techniques for studying them were still very much in flux. In principle, it was not supposed to be too difficult to identify particles by their charge and mass, and to measure their energy.[24] Alpha particles and other heavy ions can usually be identified by the short range they travel before stopping. Longer range particles can thus be assumed to have a magnitude of charge of 1. With the magnitude of a particle's charge known, its momentum can be ascertained by measuring the curvature of its track in a magnetic field made to pervade the chamber. Particles of higher momentum will be curved less by the magnetic field than will particles of lower momentum. Because it had already been demonstrated that, independent of their mass, particles of the same magnitude of charge will ionize more gas if they are traveling more slowly, velocity can be measured by measuring ionization, either by the thickness of a track or by actually counting droplets of vapor. Since momentum and kinetic energy are both functions of velocity and mass, mass and energy can then be calculated from measured values without having assumed anything about the particle except its magnitude of charge.

In practice, however, actual photographs of tracks in cloud chambers often yielded far more ambiguous measurements. In particular, ionization did not vary much when particles were moving at high velocities, making accurate velocity measurements difficult to achieve. To circumvent this problem, one could make the ostensibly safe assumption that all particles creating tracks in the chamber were either protons or electrons. Protons and electrons were somewhat harder to distinguish from each other. In principle, measuring the particle's curvature could easily settle the matter, except that some high-energy particles did not appreciably curve, and it was always possible that assumptions about the direction in which the particle traveled could be in error. As most cosmic ray particles could be safely assumed to travel downward, and as ionization measurements were often sufficient to at least estimate velocity, Anderson was usually able to determine to his satisfaction whether particles were protons or electrons. With a reliable momentum

23. In addition to De Maria and Russo, "Discovery" (ref. 22), see Galison, "Discovery" (ref. 5), or Galison, *Experiments* (ref. 4), ch. 3.

24. The following description of Anderson's methods is assembled primarily from Carl D. Anderson, "Energies of Cosmic-Ray Particles," *PR* 41, no. 4 (1932): 405–21.

measurement and mass safely assumed, precise velocity and energy measurements followed.

Because Anderson had well-developed expectations of what types of particles he was likely to find in his chamber, when he did encounter minimally ionizing particles with an apparently positive charge, the only thing he felt it necessary to verify was that they were not *heavier* than a proton. But the most important result Anderson saw as coming from his observations was that positively charged particles existed in the cosmic radiation at all. Using Millikan's assumptions that interstellar cosmic rays were photons, Anderson felt safe in suggesting that they not only scattered electrons from atmospheric molecules, but that they also dislodged protons from atomic nuclei.[25] To support this conclusion, it was only necessary to direct readers' attention to the obviously safe conclusion that the downward-traveling positively charged particles he had photographed could not all be upward-traveling electrons.

Another result that Anderson reported was his verification of the existence of "associated tracks," which were first reported by Skobeltsyn in 1929, and eventually came to be viewed as a missed early opportunity for the positron discovery. Anderson took these tracks to be evidence that cosmic rays could eject multiple particles from the same atomic nucleus (Fig. 1, left, where a proton and an electron had apparently been ejected; at that time it was considered possible that electrons were also bound inside nuclei). In one paper he reported on some regularities that he was able to measure in these associated tracks: "In general, for paired tracks, the energy of one of the associated pair is considerably less than the other, in some instances 10^6 volts and less. One of the associated pair is also in all cases definitely an electron." Although Anderson did not emphasize it, his data also showed that whenever at least one "proton" was present in associated tracks, it was *always* the more energetic of the pair—typically ten to twenty times as energetic as the accompanying electron.[26] But,

25. See also Robert A. Millikan and Carl D. Anderson, "Cosmic-Ray Energies and Their Bearing on the Photon and Neutron Hypotheses," *PR* 40, no. 3 (1932): 325–28.

26. Anderson, "Energies" (ref. 24), "definitely an electron" on 419. De Maria and Russo, "Discovery" (ref. 22), 248–49, likewise note that the "protons" were always more energetic, but with strikingly similar ionization, and quote Millikan in 1935 as observing that if not for the existence of one photograph indicating divergent ionization, the positron would have been identified earlier. They also indicate that there was significant tension between Anderson and Millikan prior to the positron identification regarding the identification of tracks that appeared to be

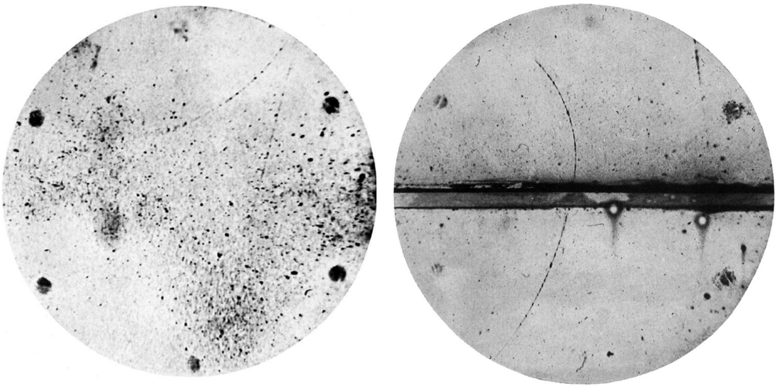

Fig. 1 Left: “associated” tracks that Anderson identified as an electron and a proton (which he measured to be almost twenty times as energetic as its partner). *Source:* Carl D. Anderson, “Energies of Cosmic-Ray Particles,” *PR* 41, no. 4 (1932): 405–21, on 411. Figure reprinted with permission. Copyright 1932 by the American Physical Society. Right: the first track Anderson identified as belonging to a positron, as suggested by the tight curvature of the particle as it emerged above the plate, combined with a long range precluding interpretation as a proton track. *Source:* Carl D. Anderson, “The Positive Electron,” *PR* 43, no. 6 (1933): 491–94, on 492. Copyright 1933 by the American Physical Society. Figure reprinted with permission.

for much of 1932, Anderson did not countenance the possibility that the tracks might represent the passage of positively charged particles lighter than protons.

By the second half of 1932, however, it had become impossible for Anderson to explain all apparently positively charged particles as protons, alpha particles, or oppositely traveling electrons. He had found a fine track of a particle that lost energy in a lead plate, apparently confirming its direction, but with a tight, positive curvature, and a range of 5cm—over ten times the distance a proton exhibiting that curvature could be expected to traverse (Fig. 1, right). Conceivably, though, the particle could have been two electrons, one originating in the gas, another originating in the lead. Anderson later recalled:

lightweight positive particles, with Anderson supposing they could be upward-moving electrons, and Millikan insisting they were protons; also see Carl D. Anderson, *The Discovery of Anti-Matter* (River Edge, NJ: World Scientific, 1999), 29–30.

> I worried a great deal about the simultaneous occurrence of independent tracks, which is always a possibility—two different electrons which happen to have this orientation—and felt that it was so extremely unlikely because we had stereoscopic cameras and could make fairly precise measurements of the position in the chamber in all three dimensions, and the lining up was just fantastically accurate. So that caused the publication.[27]

In short order Anderson found other photographs with associated tracks, each with a positive particle exhibiting a curvature and range precluding it from being a proton. Assuming a mass equal to an electron, he could now measure associated tracks to have similar rather than disparate energies. He submitted his discovery in a paper to *Science*, indicating from his measurements that the particles had "a mass comparable with that of an electron."[28] In a follow-up paper replicating photographic evidence, he said the particle's mass was "of the same order of magnitude," and declared the particle to be a "positive electron" or "positron."[29] The first paper was published six days after Anderson's twenty-seventh birthday; four years later he would receive half the Nobel Prize for Physics for this work.

Although there was as yet no firm measurement of the positron's mass, its identification as a positively charged electron was also arrived at by a pair of experimenters working with a cloud chamber at the Cavendish Laboratory in Cambridge University: Patrick Blackett and the visiting Italian physicist Giuseppe Occhialini. Writing about their own photographs in the wake of Anderson's first paper—but before the publication of his second—they sometimes echoed his noncommittal descriptions of positively charged particles "of small mass" and "whose mass is much less than a proton." But, like

27. Carl Anderson, interview by Charles Weiner, 30 Jun 1966, transcript at AIP/NBLA, online at http://www.aip.org/history/ohilist/4487.html (accessed on 29 Jul 2012). Even after initial publication, Anderson recalled, "I'm sure that there were days when I was worried about the thing because there was a period there when not very much new really came in or any other cases that were as convincing as this." However, we must contrast Anderson's doubts with his own personal conviction, which arose rapidly. According to Anderson, *Discovery* (ref. 26), 30: "An experienced scientist, just by looking at the photograph, can readily come to the conclusion that the 'thin curved line' represents the path of a new, hitherto unknown type of subatomic particle. Although only a twenty-seven year old post doctoral research fellow, I actually reached that conclusion as I looked at the still wet film just after it had been put on the drying rack."

28. Carl D. Anderson, "The Apparent Existence of Easily Deflectable Positives," *Science* 76, no. 1967 (1932): 238–39.

29. Carl D. Anderson, "The Positive Electron," *PR* 43, no. 6 (1933): 491–94. Anderson proposed the name "negatron" to distinguish the "negative electron" from the new positive one, but the traditional name of "electron" soon came to refer only to the negative variety.

Anderson, they also assumed these particles were indeed counterparts to the electron. Unlike Anderson, they also hastened to identify the particle with the negative energy states predicted by Paul Dirac's quantum electrodynamical theory of the electron, posited a few years prior. One interpretation of Dirac's theory was that oppositely charged (but otherwise identical) positive and negative particles might be produced and annihilated in pairs. Blackett and Occhialini supposed the production process was initiated by a high-energy photon's nondestructive interaction with matter. This mechanism would explain positrons' presence in the cloud chamber soon after their production in the atmosphere. It also explained their lack of existence in the broader world since as soon as they encountered an electron they would cease to exist.[30]

THE AGGREGATION OF EVIDENCE

Existing accounts of the discovery of the positron generally conclude the narrative with the particle's association with Dirac's quantum electrodynamical theory. If, however, we allow ourselves to view the Nobel Prize–worthy discovery as merely an unexpected incident in the larger project of studying cosmic rays, we can see that it had the potential to complicate considerably the inferential strategies that had originally allowed that project to proceed. Henceforth, it might have been necessary to distinguish positrons from protons in individual images. In fact, though, this problem did not immediately arise, because it was obviated by Blackett and Occhialini using another strategy of detection: the aggregation of evidence.

Where Anderson, defending his discovery against possible objections, had identified *only* those positively charged particles that could not be protons to be positrons, Blackett and Occhialini became wary of the implication that tracks left by positively charged particles were, nevertheless, "mainly due to protons." They found the often similar ionization of associated positive and negative tracks to be a "striking feature" of their own photographs, and found it implausible that one could identify high-energy positive tracks with protons,

30. P. M. S. Blackett and G. P. S. Occhialini, "Some Photographs of the Tracks of Penetrating Radiation," *Proceedings of the Royal Society of London: Series A, Mathematical and Physical* 139, no. 839 (1933): 699–726. See also P. M. S. Blackett, "The Positive Electron," *Nature* 132, no. 3346 (1933): 917–19. De Maria and Russo, "Discovery" (ref. 22), provides extensive discussion of the Cambridge work.

while finding very few corresponding low-energy proton tracks. They explained, "the stream of descending particles must certainly be fairly heterogeneous at the bottom of the atmosphere, whatever it may be at the top." Where they did find some low-energy protons in their images, these did not seem to be associated with descending radiation, "but rather with some local nuclear disintegration process." Equipped with this empirical data and Dirac's quantum electrodynamical explanation for positron creation and annihilation—and unwedded to Millikan's idea that descending charged particles originated in nuclear disintegrations—Blackett and Occhialini had no further need to see protons in their images of cosmic rays. To them it was "justified" to conclude that "the main beam of downward moving particles consists chiefly of positive and negative electrons." They did allow, "Some protons are probably also present."[31]

Of course, Blackett and Occhialini's reinterpretation of all positively curved tracks as positrons had no firmer *metrical* basis in 1933 than others' interpretations of them as protons had had a year earlier. What they *did* have was a preponderance of measurements made possible in part by their innovation of a "counter-controlled" chamber, which would be partially responsible for Blackett's winning the 1948 Nobel Prize. Traditionally, there had been no problems obtaining measurements of particles from radioactive sources, which ejected a continuous stream of known particles into the chamber. The study of cosmic rays was much more difficult, because they passed through the chamber in much smaller numbers and at irregular intervals. With cosmic rays, the chamber had to be activated at random in the hope that it would nearly coincide with a particle's passage and a usable photograph of the vapor trail left behind could be obtained. Responding to this frustration, Blackett and Occhialini found that they could more or less guarantee a usable picture by placing Geiger counters above and below the chamber and connecting them to a device that would activate the chamber whenever the two counters were triggered nearly simultaneously. This allowed them to

31. Blackett and Occhialini, "Photographs" (ref. 30), on 708. De Maria and Russo, "Discovery" (ref. 22), note this conclusion, but do not expand on its implications for interpreting observations. According to Anderson, *Discovery* (ref. 26), 35, he also had wanted to establish that all positively charged cosmic rays were positrons: "There was no proof of this assumption, however, and since we were working in a hitherto unexplored area of physics, we wanted to obtain as much information as possible to establish their identity. To accomplish this, Seth [Neddermeyer] and I took thousands of photographs with the cloud chamber unobstructed with any plates and measured the energies of the particles."

gather many more measurements of particle tracks than others observing cosmic rays.[32]

Anderson later recalled that he was immediately convinced by Blackett and Occhialini's arguments about the origins of positrons: "I very well remember reading that paper and being wholly convinced on the first reading that [pair production] was the proper explanation."[33] He also recalled that he had been immediately impressed by the Cambridge instrumentation. It "was a big step forward technically. I mean it multiplied the amount of data gathered per hour by a factor of 50; I don't know exactly but a very large factor." Therefore, "the minute we learned that Blackett and Occhialini had this counter control, we immediately set about building one." But aggregating evidence was not only attractive because it helped establish claims that were doubtful when based on only one observation. It was also the *only* way to accomplish tasks such as the one on which Anderson had originally been working, the measurement of the energy spectrum of cosmic rays. Thus, armed with new physical insights and a counter-controlled chamber, he returned to his original work of measuring the energy spectrum of cosmic rays, now using the assumption that positively charged particles were of electronic mass:

> We kept collecting more pictures and trying to improve the energy measuring capability by cutting down distortions in the gas and sharpening up the tracks and improving the light source—higher intensity, shorter time after the passage of the particle and so on. And then [we] did make energy measurements of the spectrum as we saw it in our chamber of the cosmic ray particles.[34]

Of course, no experimenter ever expected the cosmic ray energy spectrum to follow easily. Anderson delivered new results at a conference in 1934. These were modified by Blackett (now working at Birkbeck College in London with

32. P. M. S. Blackett, "On the Technique of the Counter Controlled Cloud Chamber," *Proceedings of the Royal Society of London: Series A, Mathematical and Physical* 146, no. 857 (1934): 281–99. Also see Patrick M. S. Blackett, "Cloud Chamber Researches in Nuclear Physics and Cosmic Radiation," in *Nobel Lectures, Physics, 1942–1962* (Amsterdam: Elsevier, 1964), 97–119, online at http://www.nobelprize.org/nobel_prizes/physics/laureates/1948/blackett-lecture.html (accessed on 29 Jul 2012).

33. Anderson, interview (ref. 27). Some accounts note that Anderson's published statements indicate he remained unconvinced for the course of 1933. However, Anderson gave some credence to the theory in Carl D. Anderson, "Cosmic-Ray Positive and Negative Electrons," *PR* 44, no. 5 (1933): 406–16, on 411, published in the early autumn. Either Anderson's memory is faulty here, or, more probably, he preferred to maintain a conservative public posture, particularly in view of Millikan's objections to the Dirac theory.

34. Anderson, interview (ref. 27).

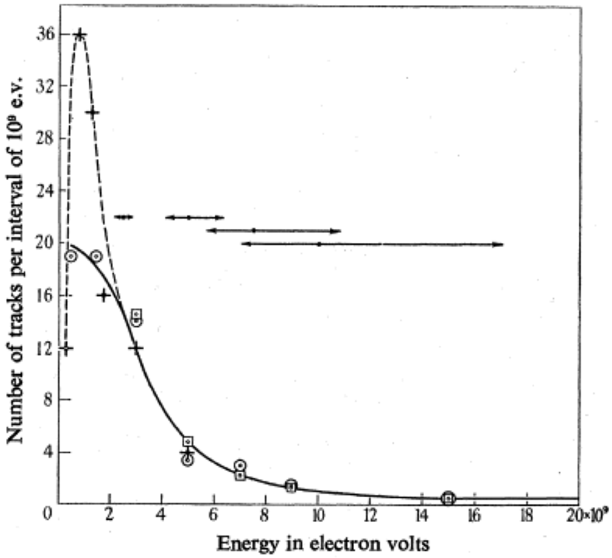

FIG. 5—Energy spectrum of cosmic rays. The arrows denote the probable errors of the energy determination. ——◎ new measurement; + - - - Anderson; ⊡ calculated from $g(E) \propto 1/E^2$.

Fig. 2 Patrick Blackett and Robert Brode modify the cosmic ray energy spectrum measurements offered by Carl Anderson. *Source:* P. M. S. Blackett and R. B. Brode, "The Measurement of the Energy of Cosmic Rays II: The Curvature Measurements and the Energy Spectrum," *Proceedings of the Royal Society of London: Series A, Mathematical and Physical Sciences* 154, no. 883 (1936): 573–87, on 584.

visiting Berkeley experimenter Robert Brode) in a 1935 paper, following his own extensive investment in apparatus refinement (Fig. 2). However, those measurements were themselves based on less than a hundred photographs, and could still only be considered highly tentative.[35]

The search for the cosmic ray energy spectrum proceeded at different altitudes and different spots on the globe, and was pursued by a large number of researchers. It would be further complicated as the assumptions that provided numbers were overturned by the discovery of first one and then a whole array of irregular-mass particles. And, of course, the energy spectrum was only one of a number of problems that were studied by accumulating data at that time.

35. Carl D. Anderson and Seth H. Neddermeyer, "Fundamental Processes in the Absorption of Cosmic-Ray Electrons and Photons," *International Conference on Physics (1934: London and Cambridge), Papers and Discussions* (London: Physical Society, 1935), 171–87; P. M. S. Blackett and R. B. Brode, "The Measurement of the Energy of Cosmic Rays II—The Curvature Measurements and the Energy Spectrum," *Proceedings of the Royal Society of London: Series A, Mathematical and Physical* 154, no. 883 (1936): 573–87.

Notably, the establishment of the existence of the positron immediately established the demand that its properties be measured many times over to confirm the assumption that the particle was a single, positively charged equivalent to the electron, and to ascertain whether the characteristics of its creation and annihilation satisfied the quantitative expectations demanded by Dirac's quantum electrodynamical theory.

As far as the measurement of positron properties was concerned, older methods of generating masses of data would prove more productive than capturing cosmic rays using counter controls. In early 1933, it was discovered that positrons were artificially produced by bombarding matter (usually lead) with γ-rays emitted from radioactive beryllium and thorium.[36] Experimenters immediately began to conduct new measurements with the artificial positrons. At Cambridge, Blackett and Occhialini, joined by James Chadwick (fresh off his neutron discovery), were among them. They reported in early 1934 on a long series of energy measurements they made over the previous year, which continued "until about 4000 tracks of electrons and about 400 tracks of positrons had been obtained, giving a body of evidence sufficient to justify quantitative conclusions." These conclusions included confirmation of the mass of the positron and the rate of its production versus the production of other radiations in the radioactive processes under study.[37]

36. These experiments were conducted more or less simultaneously by Irène Curie and Frédéric Joliot; James Chadwick, Patrick Blackett, and Giuseppe Occhialini; Lise Meitner and Kurt Philipp; and Carl Anderson. For an overview of the relevant papers, see J. Chadwick, P. M. S. Blackett, and G. P. S. Occhialini, "Some Experiments on the Production of Positive Electrons," *Proceedings of the Royal Society of London, Series A* 144, no. 851 (1934): 235–49, on 235. De Maria and Russo, "Discovery" (ref. 22), 273–78, discuss the production of artificial positrons in some detail. In James Chadwick, interview by Charles Weiner, 17 Apr 1969, transcript at AIP/NBLA, online at http://www.aip.org/history/ohilist/3974_3.html (accessed on 29 Jul 2012), Chadwick noted that prior experiments with radioactive thorium ought to have led to the positron discovery, except that the instrumentation used had been insufficiently sophisticated.

37. Chadwick, Blackett, and Occhialini, "Some Experiments" (ref. 36), "quantitative conclusions" on 236. Blackett and his collaborators did not calculate positron mass by the aforementioned method of measuring ionization and thus velocity, which remained highly unreliable. Instead, they accumulated more reliable energy measurements of produced electrons and positrons, and then calculated the mass of the positron using the known energy spectrum of the radiation that produced the particles in lead and the known mass of the electron. While allowing that their measurements of kinetic energy remained burdened by various technical difficulties and expeditious methods, and that their treatments of data employed strong assumptions, they nevertheless felt that their evidence tentatively supported a positron mass equal to the electron as well as the reality of the proposed mechanism of pair production "under the influence of an atom." They also inferred from particular photographs wherein positrons appeared to disappear

The aggregation of evidence was also crucial in establishing the existence and properties of the "mesotron." That particle was not identified by its anomalous mass—mass measurements remained unreliable—but by the failure of experimental measurements to confirm quantum electrodynamical predictions of particle disintegration rates. By 1936, Carl Anderson and Seth Neddermeyer, among other experimental groups, were able to draw a distinction between particles that were readily absorbed by lead and obeyed quantum electrodynamical predictions, from a large component of the cosmic radiation that upset those predictions by penetrating significant masses of lead. In this case there were no difficulties verifying the existence of the phenomenon. Contrasting it to the positron identification, Anderson later recalled, "I think one difference is that with the positron work there was much less data available. Now with the meson work, there was a great deal of information. There was no question at all but that there were highly penetrating particles, both positive and negative, of unit charge."[38] The trouble was in properly interpreting the phenomenon. In 1937 Anderson and Neddermeyer (who by now had received his PhD and was working as a research fellow), and independently Curry Street and Edward Stevenson at Harvard University, were able to gather and parse enough data to suggest confidently that the ability of particles to penetrate lead was not simply an unexpected failure of quantum electrodynamics to describe electron and positron behavior at high energies. The penetrating particles were an entirely separate class of particle (and their antiparticle partners), which apparently had an intermediate mass.[39]

NUCLEAR PHYSICS KNOWLEDGE AND COINCIDENCE COUNTERS

In addition to inferring the nature of events in particular observations, and aggregating evidence from multiple observations, the experimenters discussed in the prior two sections also made use of a third strategy of detection: using

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"when still possessing a large amount of energy," that the phenomenon might be visual evidence of the still-speculative pair-*annihilation* mechanism (p. 247).

38. Anderson, interview (ref. 27).

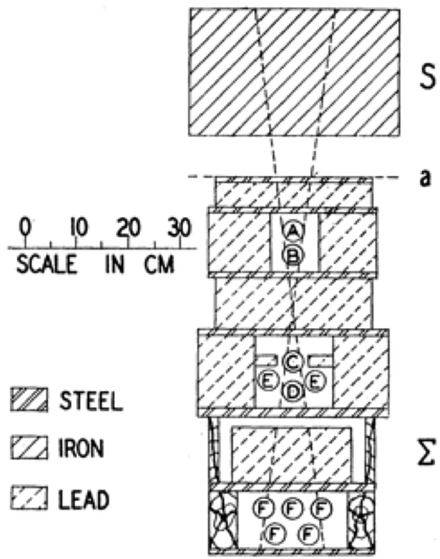
39. Seth H. Neddermeyer and Carl D. Anderson, "Note on the Nature of Cosmic-Ray Particles," *PR* 51, no. 10 (1937): 884–86. For a detailed account of the discovery, see Galison, "Discovery" (ref. 5), and Galison, *Experiments* (ref. 4); see also David C. Cassidy, "Cosmic Ray Showers, High Energy Physics, and Quantum Field Theories: Programmatic Interactions in the 1930s," *Historical Studies in the Physical and Biological Sciences* 12, no. 1 (1981): 1–39.

assumptions about nuclear physics processes that they did not directly witness to narrow the range of possible interpretations of things they did witness. As we have seen, Anderson deemed his claims that he was seeing protons in his observations of cosmic rays to be corroborated by the likelihood that those protons had been dislodged from atmospheric nuclei. Similarly, Blackett and Occhialini's interpretations relied on the assumption that the effects of atmospheric collisions should have meant that, if they were seeing protons at all, they should have been seeing both low- and high-energy protons. Later, the establishment of the mesotron particle made stronger use of knowledge about the ability of high-energy particles to penetrate large blocks of matter. Following that establishment it was inevitable that still stronger use of nuclear physics knowledge would help identify the mesotron with Yukawa's meson, given the role the meson was supposed to play within the nucleus.

Also, as important as the use of nuclear knowledge was in interpreting cloud chamber experiments, it was absolutely critical in the interpretation of experiments done using coincidence counters. In a counter experiment, counter tubes were set up in an array. When an ionizing particle passed through the counter, the counter recorded the passage as an electrical signal. A rough path of the particle could be inferred if multiple counters in the array send signals nearly simultaneously. Of course, the path could not be as neatly resolved as it could be in a cloud chamber, and there was also the possibility that coincidences would be counted by particles behaving in ways other than what was expected. A typical coincidence-counter experiment would therefore be designed in such a way that there was a very high probability, or at least a known probability, that a particular pattern of recorded coincidences would have resulted from a particular event, such as the probabilistic stopping of a particle in a block of material usually referred to as an "absorber" (Fig. 3).

Given the centrality of nuclear physics knowledge in both coincidence-counter experimentation and in the identification of the Yukawa meson with the mesotron, it is little surprise that counter experiments would play an important role in attempts to establish that identification.[40] One important contribution for coincidence counters was in determining that mesotrons spontaneously decay. Establishing the existence of such decays would help identify the mesotron with Yukawa's meson, because Yukawa's theory held they were responsible for the well-known nuclear process of β-decay. Rather than

40. The following account of prewar and postwar counter experiments maps generally onto the longer one presented in Monaldi, "Life" and "Indirect" (ref. 3).



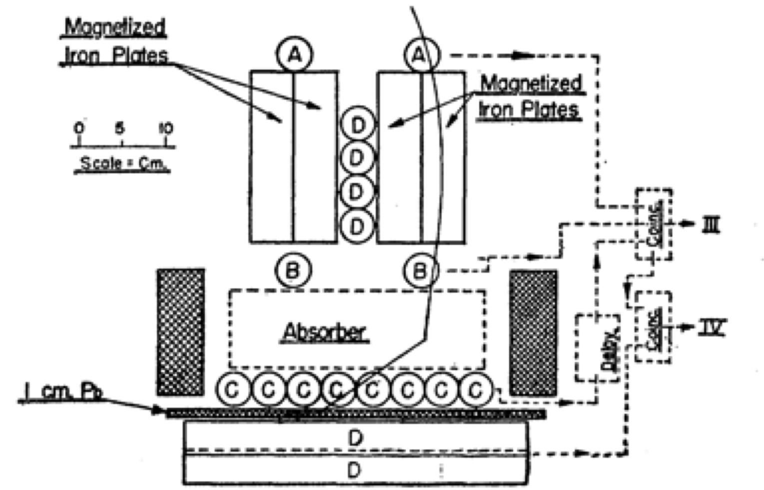


FIG. 1. Disposition of counters, absorber, and magnetized iron plates. All counters "D" are connected in parallel.

Fig. 3 Left: an experiment design used by Bruno Rossi, where S is an iron block to ensure the detection of only penetrating radiation, and Σ represents a removable lead absorber. *Source:* Bruno Rossi and David B. Hall, "Variation of the Rate of Decay of Mesotrons with Momentum," *PR* 59, no. 3 (1941): 223–28, on 224. Figure reprinted with permission. Copyright 1941 by the American Physical Society. Right: the experiment design used by Conversi, Pancini, and Piccioni to distinguish absorption rates of positive and negative mesotrons. *Source:* M. Conversi, F. Pancini, and O. Piccioni, "On the Disintegration of Negative Mesons," *PR* 71, no. 3 (1947): 209–10, on 209. Figure reprinted with permission. Copyright 1947 by the American Physical Society.

attempt to observe these decays directly, Bruno Rossi, an Italian émigré and an important pioneer of the counter method, used counter arrays to amalgamate evidence for the decay.[41] He did so by comparing the rate of mesotrons detected at elevations ranging from Chicago to Mt. Evans in Colorado with the rate of detection of those emerging from solid absorbers with a mass equivalent to the amount of atmosphere between elevations. He supposed that, because the atmosphere and the absorber would on average stop the same number of mesotrons, the discrepancy in the detection rates indicated the spontaneous decay of mesotrons over the time it took them to traverse the vertical difference between the two elevations versus the negligible thickness of the absorber. Sensitive to the prospect that even nucleus size could impact his measurements of the number of mesotrons that had been stopped rather than spontaneously decayed, he specifically chose a carbon absorber, because carbon nuclei have a mass close to that of most of the atoms that constitute atmospheric gas. His experiment succeeded.[42]

These sorts of counter experiments soon became more sophisticated. In 1940, Japanese theorists Sin-Itiro Tomonaga and Gentaro Araki made a series of quantitative predictions of meson lifetimes in matter.[43] Working under difficult conditions during World War II, the Italian physicists Oreste Piccioni and Marcello Conversi set out to test these predictions by attaching a counter apparatus to a timing circuit, which would measure the short delays between the triggering of counters, allowing them to measure these lifetimes.[44] Unexpectedly, though, their sequence of experiments soon led them to results that undermined the identification of the mesotron with Yukawa's meson. Part of Tomonaga and Araki's predictions was that negative mesons should be preferentially absorbed into positively charged nuclei, while positive mesons would be repelled by the Coulomb forces of the nuclei, meaning that most would remain free until they spontaneously decayed into an electron. Joined by fellow

41. On Rossi's early work with counters, see Luisa Bonolis, "Walther Bothe and Bruno Rossi: The Birth and Development of Coincidence Methods in Cosmic-Ray Physics," *American Journal of Physics* 79, no. 11 (2011): 1133–50.

42. Bruno B. Rossi, "The Decay of 'Mesotrons' (1939–1943): Experimental Particle Physics in the Age of Innocence," in *The Birth of Particle Physics*, ed. Laurie M. Brown and Lillian Hoddeson (New York: Cambridge University Press: 1983), 183–205; Bruno Rossi, H. van Norman Hilberry, and J. Barton Hoag, "The Disintegration of Mesons," *PR* 56, no. 8 (1939): 837–38.

43. S. Tomonaga and G. Araki, "Effect of the Nuclear Coulomb Field on the Capture of Slow Mesons," *PR* 58, no. 1 (1940): 90–91.

44. M. Conversi and O. Piccioni, "On the Mean Life of Slow Mesons," *PR* 70, nos. 11–12 (1946): 859–73.

experimentalist Ettore Pancini, Piccioni and Conversi fitted their counter apparatus with magnetic "lenses" that could filter out positively or negatively charged particles, allowing direct comparisons of decay rates between positively and negatively charged mesotrons to be made (Fig. 3, right). Mesotrons that stopped in an iron absorber and decayed spontaneously into an electron would be detected as a delayed coincidence, while the capture of mesotrons by nuclei in the absorber was not expected to result in the subsequent emission of particles that could escape the absorber.[45]

Using this experimental arrangement, Piccioni, Conversi, and Pancini were able to confirm a discrepancy between positive and negative mesotrons. They then turned to try to detect the photons they thought would be emitted by nuclei following the capture of mesotrons by nuclei in the absorber. To do so, they replaced the iron absorber with carbon, which they supposed would permit photons to escape. However, before actually attempting to detect the photons, they first decided to repeat their previous experiment to do a direct comparison of the capture rates of mesotrons in iron and carbon.[46] Unexpectedly, with the new absorber they found that *neither* positively nor negatively charged mesotrons were captured, contrary to Tomonaga and Araki's predictions. Because carbon nuclei have positive charge that is less than a quarter of that of iron nuclei, the nuclei turned out to be incapable of compensating for what appeared to be a fundamentally weak interaction of the mesotrons with the light carbon nuclei. The Italians concluded that, contrary to theory, the interactivity of mesons with nuclear particles might vary with nuclear charge for some unclear reason.[47] However, in other quarters the results suggested that mesotrons were simply altogether less interactive with nucleons of nuclei of *any* charge than required by their role as a carrier of the nuclear binding force.[48] This problem represented the most serious doubts to that point that the me-

45. Oreste Piccioni, "The Observation of the Leptonic Nature of the 'Mesotron' by Conversi, Pancini, and Piccioni," in Brown and Hoddeson, *Birth* (ref. 42), 222–41; Marcello Conversi, "The Period that Led to the 1946 Discovery of the Leptonic Nature of the 'Mesotron,'" in Brown and Hoddeson, *Birth* (ref. 42), 242–50; and Oreste Piccioni, "The Discovery of the Muon," in *History of Original Ideas and Basic Discoveries in Particle Physics*, ed. Harvey B. Newman and Thomas Ypsilantis (New York: Plenum Press, 1996), 143–59.

46. On the rationale behind this decision, see Piccioni, "Observation" and "Discovery" (ref. 45).

47. M. Conversi, E. Pancini, and O. Piccioni, "On the Disintegration of Negative Mesons," *PR* 71, no. 3 (1947): 209–10.

48. Most notably, E. Fermi, E. Teller, and V. Weisskopf, "The Decay of Negative Mesotrons in Matter," *PR* 71, no. 5 (1947): 314–15.

sotron was the same as Yukawa's meson, leading Robert Marshak to hypothesize that there must be two distinct, long-lived mesons.

NUCLEAR PHYSICS KNOWLEDGE AND THE PION

One important reason to pay close attention to the strategy of using nuclear physics knowledge to help interpret experiments is that it provided common ground between experimenters using coincidence counters, cloud chambers, and a rising detection technology, nuclear emulsions. One reason nuclear physics could play this role was that processes invoked in the interpretation of particle detection experiments always refer to phenomena that are, at some level, invisible. In many cases, they are invisible because they take place outside the experimental apparatus, or within an opaque block of material. When these invisible processes are of central interest in an experiment, sauce for the goose becomes sauce for the gander: the visual resolution of cloud chambers ceases to be a major advantage. Indeed, as we have seen, counters using timing circuits can even have better temporal resolution. A second reason is that because nuclear physics had to be invoked in the successful interpretation of experiments using all kinds of detectors, it provided an intellectual framework where results from very different kinds of detectors became relevant to each other. This point becomes very clear as we return to the experimental establishment of the pion using nuclear emulsions.

Nuclear emulsions made more intensive use of knowledge of nuclear interactions than any other particle detector.[49] A nuclear emulsion plate chemically records the passage of an ionizing particle in much the same way photographic

49. The following reviews provide helpful prior history and background on emulsions: Maurice M. Shapiro, "Tracks of Nuclear Particles in Photographic Emulsions," *Reviews of Modern Physics* 13, no. 1 (1941): 58–71; C. F. Powell, P. H. Fowler, and D. H. Perkins, *The Study of Elementary Particles by the Photographic Method* (New York: Pergamon Press, 1959); M. Blau, "Photographic Emulsions," in *Methods of Experimental Physics*, vol. 5, pt. A, *Nuclear Physics*, ed. Luke C. L. Yuan and Chien-Shiung Wu (New York: Academic Press, 1961), 208–64. Aside from Galison, *Image and Logic* (ref. 1), ch. 3, more recent historical treatments include Donald H. Perkins, "Cosmic-Ray Work with Emulsions in the 1940s and 1950s," in *Pions to Quarks: Particle Physics in the 1950s*, ed. Laurie M. Brown, Max Dresden, and Lillian Hoddeson (New York: Cambridge University Press, 1989), 89–108; Mario Grilli and Fabio Sebastiani, "Collaborations among Nuclear Emulsion Groups in Europe in the 1950s," *Rivista di Storia della Scienza* 4, no. 1 (2002): 181–206; Milla Baldo Ceolin, "The Discreet Charm of the Nuclear Emulsion Era," *Annual Reviews of Nuclear and Particle Science* 52 (2002): 1–21; and D. H. Perkins, "From Pions to Proton Decay: Tales of the Unexpected," *Annual Reviews of Nuclear and Particle Science* 55 (2005): 1–26.

film records the incidence of light. However, nuclear emulsion plates are thicker, and sometimes stacked in groups, so the passage of particles could be tracked in three dimensions, much as they could in a cloud chamber photographed using stereoscopic cameras. The emulsions could also be exposed over a period of days, allowing many particle tracks to accumulate, removing the necessity of having to time detection to coincide with a particle's passage, but also eliminating any possibility that the temporal resolution of events could be recovered, as they could with counters. Particle ranges were also much shorter than in a cloud chamber, and so tracks could not be effectively curved using magnetic fields. Thus, ionization and range are the only features of particle tracks that can be directly measured from emulsions. Early emulsions were also insensitive to the passage of electrons, positrons, or particles of very high energy, eliminating the possibility of detecting certain kinds of events—but also helpfully eliminating some possible interpretations of the events that *were* observed.

The key advantage in using nuclear emulsions was in their ability to produce nuclear interactions and to make them individually visible. While nuclear disintegrations, in which an incident particle resulted in the ejection of multiple particles from a nucleus, were often observed in cloud chambers (Fig. 4), these kinds of interactions were much more common in denser emulsions, where they produced what were called "stars." Further, with improvements in the composition of emulsions, the paths of ejected particles could be accurately measured, making possible accurate measurements of the masses of incident lightweight particles, which were often more difficult to ascertain. However, it must be emphasized that, in spite of their direct visibility, the proper interpretation of nuclear disintegrations was still not straightforward. The invisibility of ejected neutrons, neutrinos, photons, and high-energy particles meant that the existence, number, and energy of those particles had to be inferred. Further, while the known chemical composition of nuclear emulsions made guessing possible, it was rarely obvious what sort of nucleus had disintegrated. It was, though, a common practice to "load" emulsions with certain elements in the hope of observing certain kinds of expected nuclear interactions, or improving the images of certain kinds of processes. For all these reasons, knowledge of possible nuclear processes remained crucial to the interpretation of particle tracks in emulsion plates.

When in 1947 the group working at Cecil Powell's laboratory in Bristol found evidence for the production of secondary "mesons" in nuclear emulsions exposed at high altitudes, they could confidently distinguish them from protons or ions by their long range and small ionization, as well as by their propensity to be deflected from a straight path by surrounding nuclei. (Here they used the term

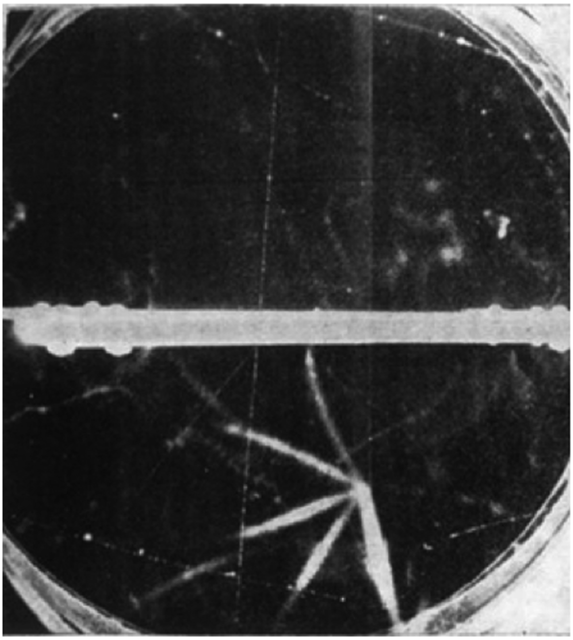


Fig. 4 A nuclear disintegration recorded in a cloud chamber. *Source:* Robert B. Brode and Merle A. Starr, "Nuclear Disintegrations Produced by Cosmic Rays," *PR* 53, no. 1 (1938): 3–5, on 4. Figure reprinted with permission. Copyright 1938 by the American Physical Society.

"meson" to refer not to a theoretical particle, but to *any* intermediate-mass particle.) While noting that "an experienced observer quickly learned to recognize the track of a meson by inspection, provided that its range in the emulsion exceeds 100μ," they ruled that unless ionization and deflection patterns definitively corresponded "to the values characteristic of a particle of small mass," the group would not "regard it as established" that the track was, in fact, a meson. Beyond such determinations, they deemed it "not possible to place serious reliance" on individual mass measurements made by measuring ionization.[50]

Mindful of Hans Bethe's then-recent admonition not to ascribe anomalous masses to observed particles lightly, the Bristol group was determined to use their knowledge of nuclear interactions to go beyond their unreliable mass measurements in order to offer strong evidence for multiple meson masses. In two similar events (for one of the two, see Fig. 5), a meson apparently reached

50. Lattes et al., "Processes" (ref. 14).

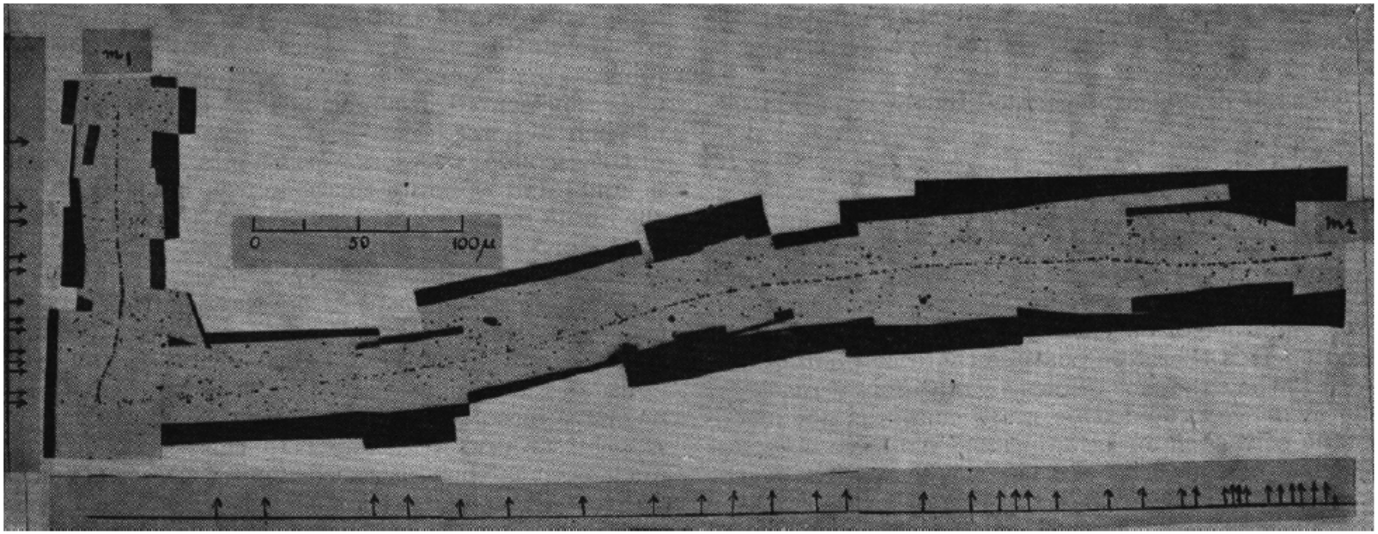


FIG. 1. OBSERVATION BY MRS. I. ROBERTS. PHOTOMICROGRAPH WITH COOKE × 45 'FLUORITE' OBJECTIVE. ILFORD 'NUCLEAR RESEARCH', BORON-LOADED C2 EMULSION. m_1 IS THE PRIMARY AND m_2 THE SECONDARY MESON. THE ARROWS, IN THIS AND THE FOLLOWING PHOTOGRAPHS, INDICATE POINTS WHERE CHANGES IN DIRECTION GREATER THAN 2° OCCUR, AS OBSERVED UNDER THE MICROSCOPE. ALL THE PHOTOGRAPHS ARE COMPLETELY UNRETOUCHED

Fig. 5 Photograph collage of a primary (m_1) and a secondary meson (m_2). The Bristol group analyzed the event considering the possibility that the mesons had identical or differing mass, and that the vertex of their paths represented either a nuclear disintegration or a spontaneous decay. Only through a full accounting of the energy budget of each process could they confidently conclude that the mesons were of differing mass, though they remained unable to determine with certainty what sort of process was exhibited. *Source:* C. M. G. Lattes, H. Muirhead, G. P. S. Occhialini, and C. F. Powell, "Processes Involving Charged Mesons," *Nature* 159, no. 4047 (1947): 694–97, on 695. Reprinted by permission from Macmillan Publishers Ltd, copyright 1947.

the end of its range and produced a secondary meson with a path having similar ionization and deflection characteristics, but traveling with a suddenly higher energy and in a new direction. They "attempted to interpret these two events in terms of an [invisible] interaction of the primary meson with a nucleus in the emulsion which leads to the ejection of a second meson of the same mass as the first." In this scenario, the captured meson would induce a nuclear process akin to a β-decay, wherein the nucleus was transmuted to either a higher or lower element, and the meson simply changed its charge according to whether two protons had been created or destroyed in the process. Yet, such a process could not produce a meson of higher energy. Another possibility involved a fission-like process where perhaps a silver nucleus was split into two undetected low-energy nuclei and another meson. The Powell group considered that interaction energetically unlikely.[51]

Stemming from the difficulty of interpreting the disintegration in terms of the emission of a meson of the same mass, they suggested that primary and secondary mesons could well differ in mass. On the basis of ionization measurements, they were willing to entertain a possible mass difference between the primary and secondary mesons of up to a hundred electron-masses.[52] They reasoned that if the primary meson were captured by a carbon nucleus, transmuting it into beryllium and releasing an oppositely charged meson, that meson would have to be about 60 electron-masses lighter than the primary. However, for lack of a sufficient number of energy and momentum measurements, they were unwilling to adjudicate whether the observed creation of a secondary meson was merely "one involving particular nuclei," or whether it might actually be "a fundamental type of process," meaning a spontaneous decay of one meson into another with no intermediary nuclear interaction (which would definitely demand a mass difference to account for the increased kinetic energy of the secondary). In fact, Powell's group had no overwhelming stake in detecting such a fundamental process. Much of their paper was devoted to the analysis of mesons emerging from nuclear disintegration stars where complex nuclei were very clearly involved (Fig. 6). (Star-producing mesons soon came to be called "σ mesons," later identified as the π^-, which tended to be captured by nuclei.) Determining meson interaction with nuclei was very important, because determining what sorts of interactions might take place

51. Ibid.

52. The difference in mass between the pion and the muon is now measured to be about 70 electron-masses.

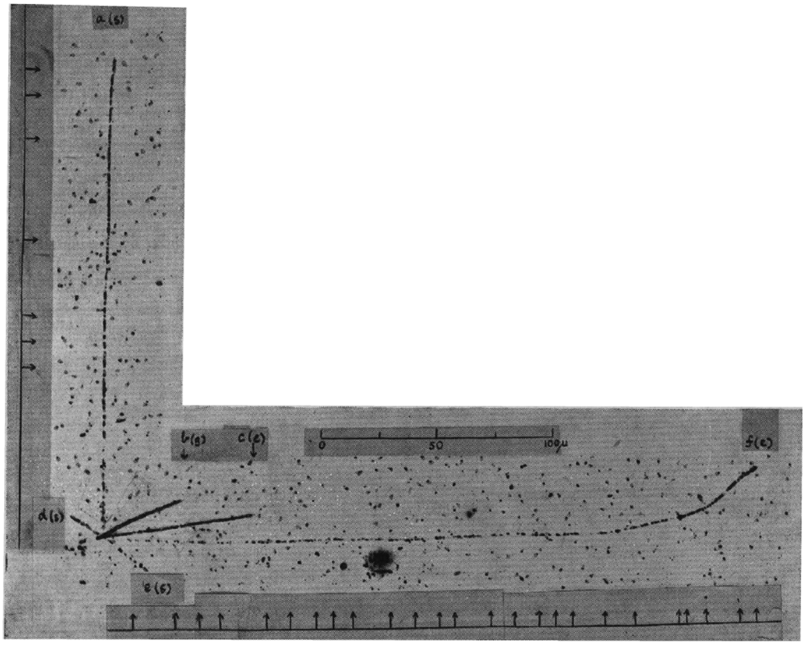


Fig. 3. Observation by Mrs. I. Roberts. Photomicrograph with Cooke × 45 'Fluorite' objective. Ilford 'Nuclear Research', boron-loaded C2 emulsion. The track (*b*) dips steeply and its apparent grain density is greater than the true value through foreshortening. Both (*b*) and (*c*) were probably produced by α-particles

Fig. 6 Photograph collage showing a nuclear disintegration star; particle "f" was estimated from grain counts to have a mass of 375 ± 70 electron-masses. It was estimated that the energy of the primary particle was "at least 200 MeV, and if its mass was equal to or less than that of a proton, it would not have been recorded by the emulsion." *Source:* C. M. G. Lattes, H. Muirhead, G. P. S. Occhialini, and C. F. Powell, "Processes Involving Charged Mesons," *Nature* 159, no. 4047 (1947): 694–97, on 696. Reprinted by permission from Macmillan Publishers Ltd, copyright 1947.

had potential—but still unclear—implications for reinterpreting what the Powell group called the "very radical conclusions" drawn by Piccioni, Conversi, and Pancini from their coincidence counter observations of interactions between mesons and carbon nuclei.[53]

When Powell's group published their paper in May 1947, they were not at that point able to offer any firm conclusions as to what phenomena they had witnessed in their emulsions. Their measurements of meson mass did not directly support the interpretation that they were seeing two mesons, rather than many mesons, or, for that matter, a single meson of variable mass. Further, they were unable to say whether secondary mesons were produced only through nuclear interactions, or whether they could result from spontaneous decay.

53. Lattes et al., "Processes" (ref. 14).

Based on arguments from nuclear physics, they were only able to offer a convincing argument that mesons could produce other mesons, and that these secondary mesons seemed to have a mass lighter than the first meson. Although Marshak and Bethe warned that "many more experiments must be performed before the existence of the heavy meson and, in particular, the proposed identification [with the Yukawa meson] can be accepted," they were sufficiently convinced to regard the results as evidence in favor of the two-meson hypothesis.[54] Within the two-meson framework, the new emulsion results could evidently be reconciled with the Italian counter experiments. If one assumed that the heavy, primary meson had a short lifetime and coupled strongly to nucleons, it could be assumed to decay at high altitudes into a lighter meson that did not couple strongly with nucleons, which was detected by the Italian experimenters near sea level.

NEW STRATEGIES FOR A NEW ERA

It is a commonplace that particle physics came into its own during the second half of the twentieth century. Suddenly, physicists began to seek new particles systematically in an experimental environment that came to be dominated by expensive, high-energy particle accelerators and newly industrialized styles of work. What has been less well recognized was the degree to which this new era was accompanied by the need for new means of interpreting experimental results. By paying careful attention to experimenters' strategies of detection, the discovery of the pion can be seen to represent a remarkably clear turning point. As we have seen, the identification of primary and secondary mesons was accomplished through an unusually deep use of nuclear physics knowledge. However, it was also the first time that a new particle had been identified by its *decay mode*, that is, the way in which a particle spontaneously transforms into other particles.

At the time, it was not obvious that the analysis of decay modes would become an important new strategy of detection. Prior to the pion discovery, observing the spontaneous decay of free particles had been uncommon. Experimenters had often set out to observe decaying nuclei, high-energy particles disintegrating nuclei, and events where a high-energy particle could initiate a "shower" of particles, but these processes were all mediated. The first attempt

54. Marshak and Bethe, "Two Meson" (ref. 16).

to observe a spontaneous decay was a set of cloud chamber experiments that E. J. Williams and G. E. Roberts conducted in 1939 at University College of Wales, Aberystwyth, in which they photographed the decay of a mesotron into an electron.[55] Then, in 1946, "V-particles"—particle decays wherein an unseen zero-charge particle is involved, resulting in a V-shaped track—were seen in a cloud chamber in Manchester, but they were not seen again until 1949 when Carl Anderson's group detected thirty-four of them.[56] With this very thin record, it is not surprising that when the Bristol group captured the decay of one meson into another early in 1947, they did not take it for granted that it was unmediated by a nucleus.

However, with the two-meson hypothesis it made sense to presume that tracks resulting in secondary mesons, but which did not create a disintegration star, did, in fact, represent a spontaneous decay. Thus, by the autumn of 1947, the Bristol group (having received a copy of Marshak and Bethe's paper prior to its publication, and having obtained new high-elevation photographs from Bolivia) were already referring to "π" and "μ" mesons, the former transforming into the latter through "μ-decay."[57] With this process identified, they quickly began to classify meson behaviors in their emulsions according to whether they caused disintegration stars, originated in disintegration stars, underwent μ-decay, or apparently stopped in emulsions (meaning they might be μ mesons decaying into undetectable electrons). They then combined these classifications with knowledge of possible nuclear interactions occurring both inside and outside their emulsions, in order to establish a unified, if tentative, scheme of particles (Fig. 7). Later in 1948, using new electron-sensitive emulsions, the Bristol group began to observe other kinds of spontaneous decays. First, they captured a two-stage decay of a π into a μ into an electron.[58] They also discovered another new

55. E. J. Williams and G. E. Roberts, "Evidence for Transformation of Mesotrons into Electrons," *Nature* 145, no. 3664 (1940): 102–03.

56. See George Rochester, "Cosmic-Ray Cloud Chamber Contributions to the Discovery of the Strange Particles in the Decade 1947–1957," in Brown et al., *Pions* (ref. 49), 57–88.

57. C. M. G. Lattes, G. P. S. Occhialini, and C. F. Powell, "Observations on the Tracks of Slow Mesons in Photographic Emulsions, Part 1," *Nature* 160, no. 4066 (1947): 453–56; and C. M. G. Lattes, G. P. S. Occhialini, and C. F. Powell, "Observations on the Tracks of Slow Mesons in Photographic Emulsions, Part 2," *Nature* 160, no. 4067 (1947): 486–92. These papers also made preliminary attempts to determine the masses of the two particles; for more on their methods, see C. M. G. Lattes, G. P. S. Occhialini, and C. F. Powell, "A Determination of the Ratio of the Masses of π- and μ-Mesons by the Method of Grain-counting," *Proceedings of the Physical Society* 61, no. 2 (1948): 173–83.

58. R. Brown, U. Camerini, P. H. Fowler, H. Muirhead, C. F. Powell, and D. M. Ritson, "Observations with Electron-Sensitive Plates Exposed to Cosmic Radiation, Part 1," *Nature* 163,

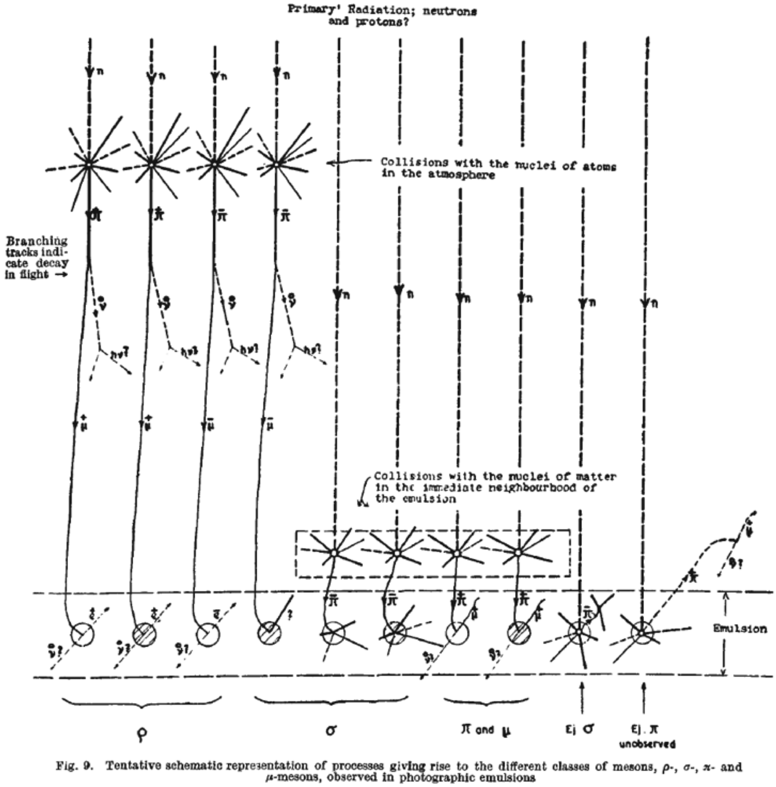


Fig. 7 A schematic produced by Giuseppe Occhialini and Cecil Powell representing the decays and nuclear disintegrations that likely created different kinds of tracks seen in their nuclear emulsions. *Source:* G. P. S. Occhialini and C. F. Powell, "Observations on the Production of Mesons by Cosmic Radiation," *Nature* 162, no. 4109 (1948): 168–73, on 173. Reprinted by permission from Macmillan Publishers Ltd, copyright 1948.

particle, later called the τ (labeled "k" in Fig. 8), which decayed into three particles, one of which then triggered a nuclear disintegration.

Identifying the characteristic decay modes of elementary particles quickly became a way of routinely identifying new particles. As a consequence, knowledge of nuclear physics became less central to particle physics experimentation. (Nuclear interactions would later be virtually eliminated by the ascendancy of particle colliders.) However, successful decay mode analysis also necessitated the refinement of another strategy of detection: precision measurement. To

no. 4132 (1949): 47–51. The group presumed the final particle was an electron, but were unwilling to say so definitively.

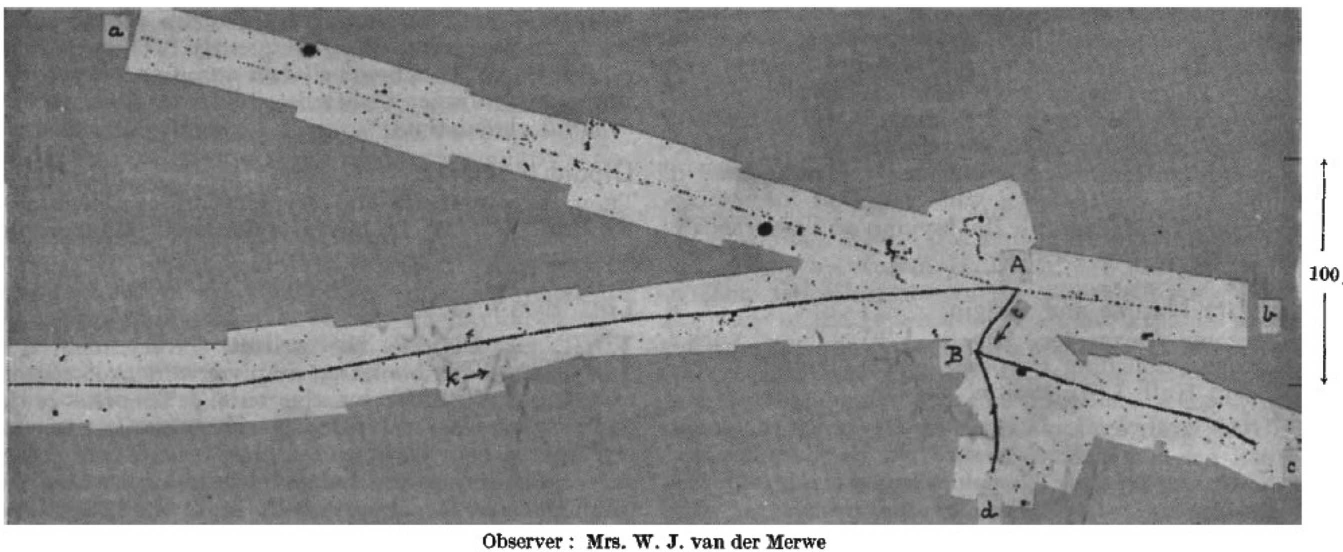


Fig. 8 Photograph collage of an emulsion showing the apparent decay (A) of a previously unknown particle, labeled "k," and the subsequent disintegration of a nucleus (B) caused by the absorption of a π-. *Source:* R. Brown, U. Camerini, P. H. Fowler, H. Muirhead, C. F. Powell, and D. M. Ritson, "Observations with Electron-Sensitive Plates Exposed to Cosmic Radiation," *Nature* 163, no. 4133 (1949): 82–87, on 82. Reprinted by permission from Macmillan Publishers Ltd, copyright 1949.

that point, new particles were typically identified through a painstaking process of eliminating the possibility that they could be existing particles, and then establishing characteristics such as mass through the aggregation of measurements. Even the discovery of the τ did not rely on precision measurements. Relatively imprecise measurements of range and ionization made it apparent that it was heavier than a π or μ, but lighter than a proton. However, the paper announcing the discovery also took pains to discuss experimental means of measuring mass, not only because it was a desideratum of experiment, but because the group was unable to identify the τ's decay products definitively. The particle "t" (in Fig. 8) was identified as a π^- because that was the only kind of particle that could create the disintegration pattern at B (giving rise to two protons, "c" and "d"). However, measurements were unable to determine whether "a" and "b" were π or μ particles.[59]

In the new exploratory era of particle physics, it would be necessary to make these sorts of determinations quickly so that the decays exhibited in individual images could be swiftly identified, and the events' connection to an existing body of physical knowledge debated. In the case of the τ, once its decay products were all established to be π particles, it soon led to one of the most famous dilemmas in early particle physics. Measurements of the τ's characteristics were essentially identical to those of another new particle called the θ, which decayed into only two π particles. The τ and the θ could not, however, be presumed to be the same because their differing decay modes suggested the particles had different "parities," which was a quantity that supposedly remained constant through decay. The so-called "τ-θ puzzle" was only resolved in the mid-1950s when it was determined that parity could indeed be violated in what came to be known as "weak" interactions.[60] Only then could the particles be verified as one and the same.

The new premium on decay mode analysis and precision measurement also left counter-type experiments at a decided disadvantage in the new era of particle physics, which would only increase with the introduction of bubble chambers later in the 1950s. However, as observations of particle interactions turned

59. R. Brown, U. Camerini, P. H. Fowler, H. Muirhead, C. F. Powell, and D. M. Ritson, "Observations with Electron-Sensitive Plates Exposed to Cosmic Radiation, Part 2," *Nature* 163, no. 4133 (1949): 82–87.

60. See Allan Franklin, "The Discovery and Nondiscovery of Parity Nonconservation," *Studies in History and Philosophy of Science* 10, no. 3 (1979): 201–57; Richard H. Dalitz, "*K*-meson Decays and Parity Violation," in Brown et al., *Pions* (ref. 49), 434–57; and Val F. Fitch, "The τ-θ Puzzle: An Experimentalist's Perspective," in Brown et al., *Pions* (ref. 49), 458–63.

from the study of cosmic rays to the collision products from new high-energy accelerators, experimenters gained a new level of control over their work. First, it was possible to know the energies of accelerated particles before they collided with material targets. It was also possible, for the first time since prewar studies of the mesotron, to isolate particles of interest, such as by "analyzing" them by applying magnetic fields to known collision products, thus separating the particles according to their momentums. As early as 1950 such methods were being used to isolate beams of pions for study at the new Nevis Cyclotron of Columbia University.[61] Along with new quantities of interest such as particle scattering angles, new control over experimental conditions meant that nonvisual detectors would continue to play a role in pushing back the frontiers of particle knowledge, even though they could not aspire to the resolution of visual detectors. Sophisticated experiment design would be crucial to progress in high-energy physics after 1950 (Fig. 9), but it is beyond the scope of this paper to discuss these new strategies in further detail.

CONCLUSION: EXPERIMENTATION BEYOND DISCOVERY

In Peter Galison's conception, experimenters strive to produce results that conform to their epistemological ideals. Accordingly, these ideals also function as a standard by which experimenters judge whether a phenomenon can legitimately be said to exist. It is important to realize, though, that while this conception was originally built as part of a critique of the idea of a *moment* of discovery, it continued to place the *act* of discovery at the center of history. By contrast, this article's revised history of particle detection practices takes discovery to be only one task among many possible tasks of experimentation. Alternative tasks, like measuring critical quantities, helped to build a robust and deeply intertwined *body* of physical knowledge, rather than suffice with the collection of discrete *units* of knowledge. Because different tasks required different strategies, it makes sense to trace the history of various strategies and patterns of their use. I would argue that this approach is particularly necessary for studying the period under scrutiny here, because, prior to the experimental discovery of the pion, new discoveries were simply not widely anticipated, and

61. L. Lederman, J. Tinlot, and E. T. Booth, "On the Decay of the π^- Meson," *PR* 81, no. 2 (1951): 281–82. See also Jack Steinberger, "A Particular View of Particle Physics in the Fifties," in Brown et al., *Pions* (ref. 49), 307–30, esp. 311–13; or Jack Steinberger, *Learning about Particles: 50 Privileged Years* (New York: Springer, 2005), esp. 41–42.

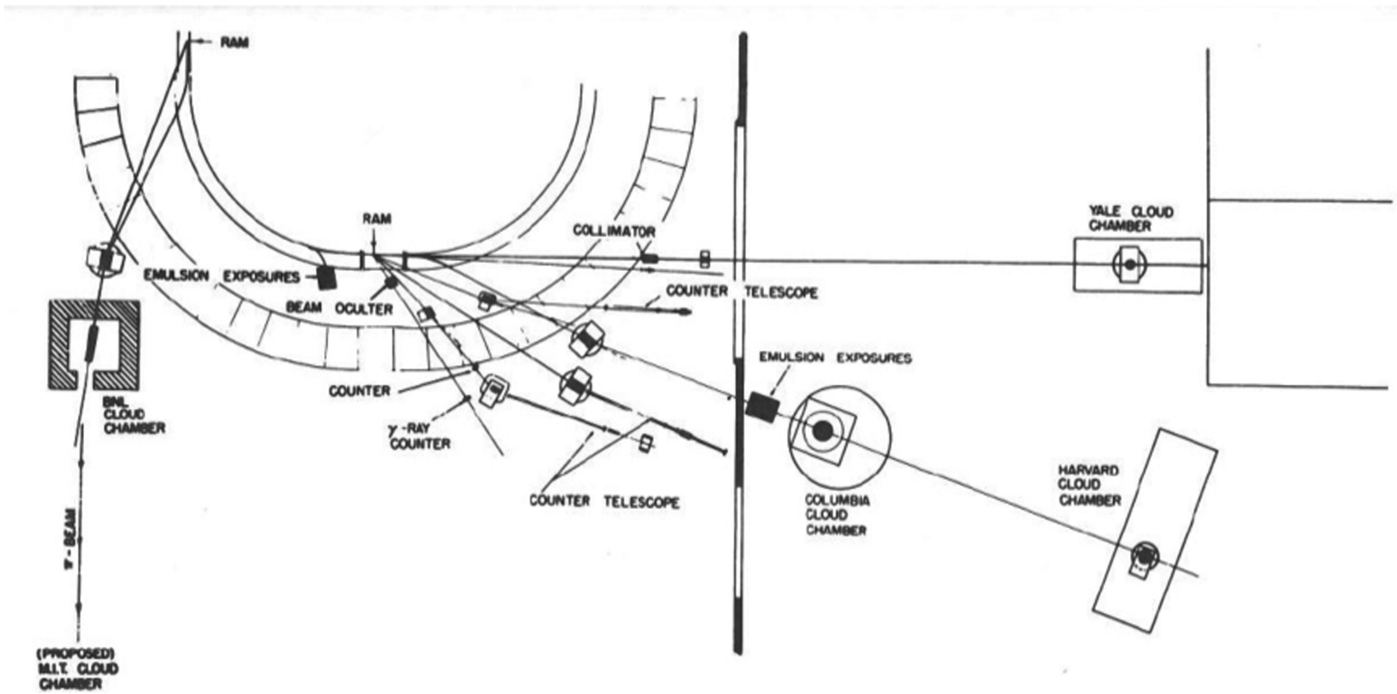


Fig. 9 Multiple cloud chamber, emulsion, and counter experiments designed around the known products of the Cosmotron accelerator at the Brookhaven National Laboratory in 1954. *Source*: William Chinowsky, "Strange Particles," in *Pions to Quarks: Particle Physics in the 1950s*, ed. Laurie M. Brown, Max Dresden, and Lillian Hoddeson (New York: Cambridge University Press, 1989), 331–42, on 333. Courtesy of Brookhaven National Laboratory.

were, therefore, generally resisted. Thus, discovery was a decidedly secondary task for experimenters, even though new discoveries were certainly widely celebrated once they had been firmly established.

The relations this article traces between experimental and interpretive practices, the varying tasks of experimentation, and their periodization build on certain criticisms of Galison's history already advanced by the historian of science Richard Staley and the philosopher of science Kent Staley (no relation). Richard Staley has criticized a portrait that Galison developed with Alexi Assmus, which held that, in its very early history, cloud chamber experimentation *shifted* from a "mimetic" meteorological tradition to a laboratory tradition of "analytic" ion physics.[62] According to Staley, though, the chamber was *interchangeably* used to mimic atmospheric phenomena and to analyze the physical behavior of air samples, which might shed light on conditions higher in the atmosphere.[63] This emphasis on interchangeability of uses demonstrates the importance of attending to the variety of tasks to which a single instrument could be put in the advancement of a single scientific program, in this case meteorology.

Picking up the history of cloud chamber experimentation circa 1930, a similar variety of tasks and interchangeability of instrument uses can be seen. Notably, experiments using cloud chambers could produce compelling images, as in the case of Anderson's positron discovery. However, they could also be used to aggregate images, as in Blackett and Occhialini's argument that the cosmic radiation comprised mainly electrons and positrons. This observation accords with Kent Staley's criticism of Galison's division of experimental practices between image-tradition experimenters, who sought to establish discoveries by producing single, compelling images, and logic-tradition experimenters, who sought rigorous statistical proofs. While Galison acknowledges that image-tradition experiments regularly aggregated evidence, he claims that such uses were epistemologically subordinated to the search for golden events.[64]

62. Peter Galison and Alexi Assmus, "Artificial Clouds, Real Particles," in *The Uses of Experiment*, ed. David Gooding, Trevor Pinch, and Simon Schaffer (New York: Cambridge University Press, 1989), 225–74; see also Galison, *Image and Logic* (ref. 1), ch. 2.

63. Richard Staley, "Fog, Dust and Rising Air: Understanding Cloud Formation, Cloud Chambers, and the Role of Meteorology in Cambridge Physics in the Late 19th Century," in *Intimate Universality: Local and Global Themes in the History of Weather and Climate*, ed. James R. Fleming, Vladimir Jankovic, and Deborah Coen (Sagamore Beach, MA: Science History Publications, 2006), 93–113.

64. Galison, *Image and Logic* (ref. 1), 453; and Peter Galison, "Reflections on *Image and Logic: A Material Culture of Microphysics*," *Perspectives on Science* 7, no. 2 (1999), 255–284, 277–278.

According to Staley, statistics-based argumentation was as integral to the image tradition as to the logic tradition.[65] By attending to the different tasks of experimentation, it becomes clear that Staley at least has a point: aggregation was not only desirable for establishing new discoveries and verifying mass measurements, it was absolutely essential to many tasks not oriented around discovery.

A crucial support for Galison's argument concerning the image tradition's concentration on the production of compelling images is his consistent depiction of cloud chambers as yielding high-integrity images. Of course, experimenters required extensive training to learn how to interpret cloud chamber photographs properly, but, once they had mastered the necessary skills, identifying novel phenomena in images was, in his view, fairly unproblematic. Through this depiction, Galison is able to present a long continuity in experimental practice, wherein the development of the bubble chamber in the 1950s represented the "culmination" of a "homomorphic" ideal that had its roots in cloud chamber experiments conducted "half a century before."[66] By contrast, I emphasize the limitations of cloud chamber images, and the existence of an important discontinuity in practice circa 1930 and again in the late 1940s. As we have seen, in the 1930s experimenters coped with the cloud chamber's limitations by employing particular inferential strategies. Heavy reliance on these strategies only began to be obviated with the rise of precision measurement. Of course, attempts to attain precision were long-standing. At Berkeley in 1938, for instance, Robert Brode and his student Dale Corson published results of experiments done with a counter-controlled cloud chamber rigged so that ions had time to diffuse before the chamber expanded, allowing vapor droplets to be

65. Kent W. Staley, "Golden Events and Statistics: What's Wrong with Galison's Image/Logic Distinction," *Perspectives on Science* 7, no. 2 (1999): 196–230; for recent agreement, see Allan Franklin, "Commentary 02," *Centaurus* 50, nos. 1–2 (2008): 162–65. Staley supposes that the commonality of statistical argumentation between the image and logic traditions suggests an underlying philosophical "unity" of science, which is, of course, a point of view directly at odds with Galison's emphasis on science's disunity. For Galison's reply, which concentrates on debunking the assertion of underlying unity, see Peter Galison, "Reflections" (ref. 64). I take no firm stand on the unity-disunity question. On a related point, although Monaldi, "Indirect" (ref. 3), 382, asserts that "Galison has amply demonstrated the historical force of the 'homomorphic form of evidence' produced by the cloud chamber and other detectors in the image tradition," her elucidation of counter experimenters' distinction between "direct" and "indirect" observation likewise suggests stronger commonalities between the traditions than Galison would allow.

66. Galison, *Image and Logic* (ref. 1), 426; for further discussion of the homomorphic ideal, see esp. 19 and 67.

more easily counted and velocity better measured.[67] But, precision measurement did not become a *regularized* strategy of detection until it began to complement decay mode analysis in the post-1947 era of rapid discovery. From this perspective, the outstanding integrity of bubble-chamber images can be seen as a more novel and important accomplishment than if one simply regards bubble chambers as the culmination of the decades-long refinement of image-tradition techniques.

Establishing this new periodization also allows us to make more sense of the interpretive practices used in nuclear-emulsion experiments. Recognizing that emulsion experimenters had difficulties stabilizing discovery claims, Galison regards them as having had a "deeply ambivalent attitude toward the status of individual or golden events," which, in his view, was anomalous for the image tradition. He attributed this anomalous attitude to the fact that emulsions were "infinitely more volatile" than cloud chambers. Experimenters who used them struggled to cope with "problems connected with stabilizing a terrifyingly unstable gelatinous chemical mass in its production, utilization, storage, processing, and interpretation." Because Galison depicts cloud chamber images as comparatively reliable, he supposes emulsion experimenters experienced an acute "anxiety." Thus, he refers to Cecil Powell as acting "as if in self-reproach," when he "worked ceaselessly to piece together the photomicrographic track mosaics in imitation of the crystal-sharp cloud chamber photographs with which [C. T. R.] Wilson had startled the world a half-century earlier."[68] As we have seen, though, emulsions only began to be commonly used at the beginning of the post-1947 era of rapid discovery in particle physics. In earlier cloud chamber experiments, inferential strategies actively suppressed novel interpretations of particle tracks. On occasions when the use of these strategies was relaxed, results became predictably unreliable. Recall that it was cloud chambers that had been responsible for the measurements suggesting multiple mesotron masses, which Powell had in mind when he made his elaborate arguments for the differing mass of secondary mesons. In the new era, *both* cloud chambers and emulsions were ill-suited to the definitive establishment of new discoveries. The rise of decay mode analysis aided both instruments in the task until the arrival of the bubble chamber several years later.

67. Dale R. Corson and Robert B. Brode, "The Specific Ionization and Mass of Cosmic-Ray Particles," *PR* 53, no. 10 (1938): 773–77.

68. On "anxiety" and nuclear emulsions, see Galison, *Image and Logic* (ref. 1), esp. 230–38; "ambivalent" on 234; "stabilizing," "volatile," and "self-reproach" on 237.

If Galison seems to have read the precision of bubble chamber experiments back onto earlier cloud chamber experiments, that quality is joined in this retrograde interpretation by another quality of bubble-chamber experiments: their passivity. According to Galison's conception, because discovery in the image tradition was accomplished by obtaining novel images, passivity served discovery because it allowed for serendipitous observations to be made of unsuspected phenomena. By contrast, because experimenters in the logic tradition could never fully visualize what was occurring in their instruments, interpretations always followed from confirmation or disconfirmation of some result that experimenters expected to follow from an experiment's design, or from the manipulation of experimental conditions.[69] This distinction does, in fact, seem to have often obtained in the bubble chamber era. However, I would urge that in the pre-1947 era, both traditions were characterized by an emphasis on *serial* experimentation. Due to the unreliability of all available instruments, experimental conclusions were tested less through repetition and confirmation than from conclusions successfully informing the design and interpretation of future experiments. Follow-on experiments might have been a variation on an initial experiment, such as one with altered counter arrangements, one with a lead plate placed within a cloud chamber, or one undertaken at a different altitude. They might also have been totally different experiments, possibly even done by a different experimenter using a different instrument.[70]

The historical importance of serial experimentation becomes clear if we compare the identification of Anderson's 1932 discovery with a positively charged electron to the non-identification of the mesotron with Yukawa's meson. Generally, the former identification has not been deeply probed, simply because subsequent experiments tended to validate it. In the latter case, the subsequent experiments are better known because they ultimately ended up undermining the identification. However, there was little besides their ultimate fate to distinguish the two sets of follow-on experiments. Understanding how serial experimentation worked not only prompts us to pay attention to the follow-on experiments to the positron discovery, it also permits better interpretations to be made of experiments following the mesotron discovery. Most

69. Ibid., 25 and ch. 6.

70. Monaldi, "Indirect" (ref. 3), 355, also notes the importance of series of experiments in counter experimentation. Mary Jo Nye, *Blackett: Physics, War, and Politics in the Twentieth Century* (Cambridge, MA: Harvard University Press, 2004), 140, notes the importance that Blackett ascribed to the continual adaptation of his cloud chamber apparatus, as well as to voluminous data collection.

notably, E. J. Williams and G. E. Roberts's aforementioned 1939 cloud chamber photograph of a mesotron decaying into an electron has generally been portrayed as a sort of golden event. It certainly was, in the sense that it demonstrated the existence of the decay. At the time, though, because the decay was anticipated, the real significance of the photograph was actually taken to be that it was provisional evidence in favor of mesotron-meson identification.[71] Patrick Blackett was still describing the result in these terms as late as his 1947 obituary for Williams, who had died prematurely of cancer.[72] It was only in the wake of the Italian experiments indicating the non-identity of the particles that the photograph could be retroactively considered a discovery of the decay mode of the suddenly mysterious μ particle.[73] Such capriciousness in interpretation was typical of this period of physics.

If this article's history of particle detection practices diverges in significant ways from Galison's history, its basic project derives mainly from his work. I believe that the emphasis he has placed on experimental practice, and on the intellectual sources of legitimate experimental interpretation, and his search for new objects of mesoscopic history all provide an excellent basis for developing and debating the history of particle physics, not to mention the history of science more generally. Although I believe that his characterization and periodization of the image and logic traditions is sometimes misleading, I agree with him that historians' efforts to characterize and periodize historical actors' practices are well rewarded by the increased understanding it brings of their ideas and actions. I also agree with Galison that experimenters have often worked according to their own sets of intellectual rules. If my sense of how these rules worked differs from his, I agree that determining what they were is an important task for historians. I myself offer no fully worked out epistemology of experiment here. I can only suggest that any satisfactory epistemology should make room for the decisions scientists make in matching their experimental and interpretive strategies to different experimental tasks. Above all, I would stress that Galison is right to press for more depth in historians' analyses

71. Monaldi, "Life" (ref. 3), 439, places the experiment in its proper context.

72. P. M. S. Blackett, "Evan James Williams, 1903–1945," *Obituary Notices of Fellows of the Royal Society* 5, no. 15 (1947): 386–406, on 395.

73. As we have seen, the Italian experimenters were not themselves persuaded of this point, and instead understood the experiment to challenge the body of theoretical knowledge surrounding the meson. Later, they would reinterpret the experiment within a discovery narrative, taking it to have established the "leptonic" nature of the μ, even though the concept of the lepton had not yet settled into its ultimate form, and their experiments did not suggest any defining similarity between the μ and the electron; see Piccioni, "Discovery" and Conversi, "Period" (ref. 45).

of scientific thought and practice. We can only expect to maintain professional progress if we actively try and articulate what our deepest and most synoptic accounts of history are, and then engage with them constantly and candidly.

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