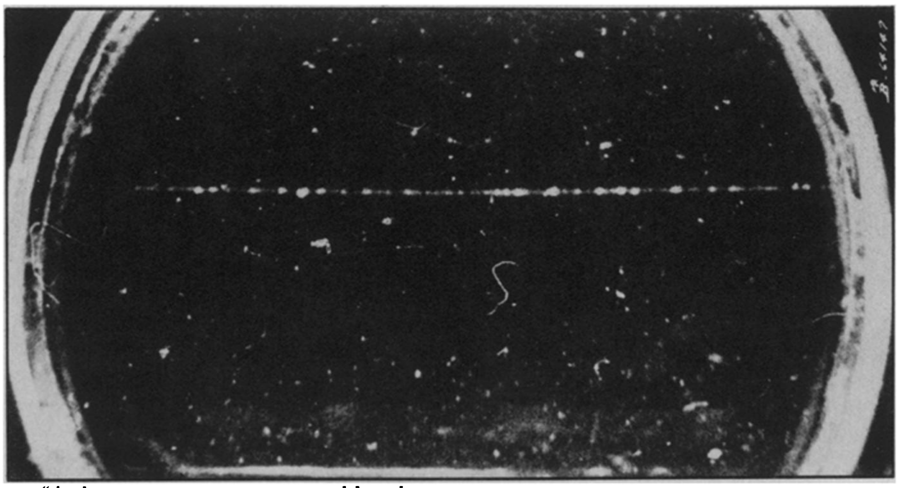
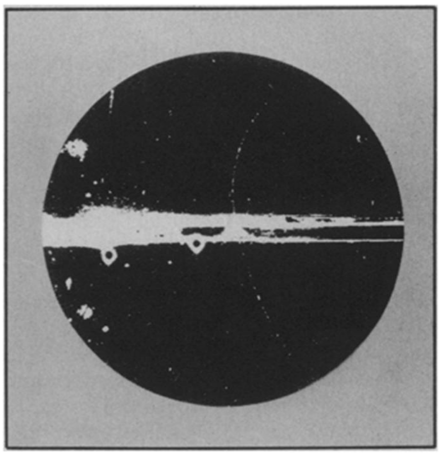
A DOZEN PARTICIPANTS

A prolegomenous drama, grave and stately, but relieved by humor and wit

BY DIETRICK E. THOMSEN





"A slow mesotron, interrupted by electrons" is the contemporary description of the cloud chamber photo (above) taken in 1940 by scientists working with A. H. Compton. The chamber was flown in a plane that posted "a new world's altitude record for transport planes of 28,900 feet." Thin track curving to right in photo at right is a cosmic ray positron observed in 1936 by Carl Anderson.

Luigi Pirandello wasn't there, but a number of other Italians were. These were not poets or playwrights, but physicists who had participated in the foundation of particle physics. Some of them remained juridically Italians. Some, impelled by the political developments of the same years in which particle physics began, had become Americans. They were gathered with other participants, historians and interested observers (not necessarily of Italian origin) to discuss the early history of particle physics in the laboratory named for one of the greatest of the Italians who became American: Enrico Fermi.

Like Pirandello's six characters each of the physicists climbed up on an almost bare stage and said his thing, his account of whatever he had been involved in. Like Pirandello's characters they seemed to be inviting some director to weave their statements together into a collective whole, a dramatic unity. And like Pirandello's characters they showed a strong disposition to want to direct the director.

The history of physics is neither the oldest nor the most practiced subdivision of that discipline. And it has to contend with the opinion that physics has no history. All you need to know about physics is laid out in neat mathematical derivations printed in the archival journals and the textbooks. Anything that can be said about who did it or why or how or whether it would have been different if someone else had done it or if the conditions of the times had been different is irrelevant. The formulas in the textbook are, like Athene, born fully accoutered. All the rest is anecdotes, which may be used to space out the difficult parts of the textbook.

The foregoing paragraph is a caricature. It drives to extremes an attitude that in milder forms has a sizable currency. The dozen or so people who mounted the platform in the Fermilab auditorium during the 1980 International Symposium on the History of Particle Physics were cooperating in an attempt to develop a history of particle physics. In some of them an observer could sense a certain question: Would the enterprise succeed? Or was it all anecdote?

The authors of *1066 and All That* have assured us that "history is what you remember." Bruno Rossi, describing the first experiment in which he participated after coming to the United States, relates how he and his collaborators mounted cosmic ray observing equipment in a bus belonging to the University of Chicago and drove off to Colorado. (It took three days; this was 1939.) When they got to the high mountains the bus began to act recalcitrant. At one lake they filled every available container with water for the radiator. The water lasted just till they reached the next lake. Mrs. Rossi remembers that she sat on the hood the whole way, constantly pouring water into the radiator. Mr. Rossi suggests that her memory may have been "a bit romanticized" by the passage of time.

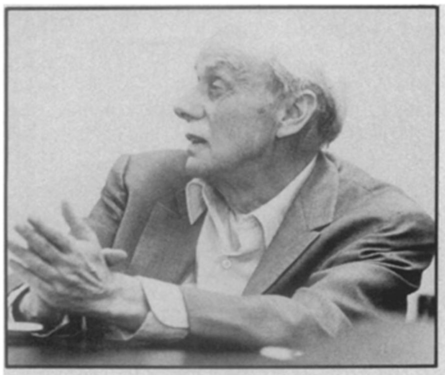
That tale may never have found its way into a textbook, but one that has concerns the discovery of the muon. The muon first appeared in the early 1930s in cosmic rays recorded at the surface of the earth (the "secondary cosmic rays"), such as those the Rossis were going out to observe. According to the story as popularly told, the muon, which then was called "meson," mesotron or even yukon, was believed to be the particle predicted by Hideki Yukawa as the bearer of the strong nuclear force and the binding element of atomic nuclei. According to folklore this belief persisted until the pion was discovered and identified as the Yukawa particle in 1947.

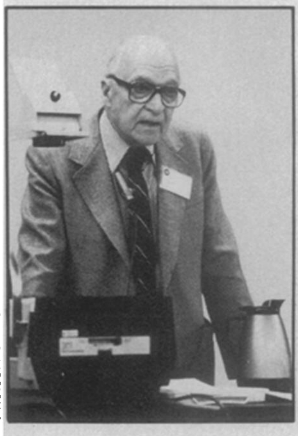
Not so, according to several participants. According to Gilberto Bernardini, Enrico Fermi suggested that the "mesotron" did not have the right properties to be the Yukawa particle not long after the mesotron was found. Carl Anderson, who could not attend the symposium but sent a paper to be read, points out that the Yukawa theory might have been helpful to him and his co-workers had they known of it. For two years they had seen growing evidence of the "meson's" existence, but since they had no reason to suspect the existence of any particles but protons and electrons, they didn't know how to interpret it. Nevertheless, "the 'meson' could not have been the yukon," Anderson says. "In very early experiments cosmic ray 'mesons' ignored the nuclear force." Paying attention to the nuclear force was the reason the Yukawa particle was supposed to exist.

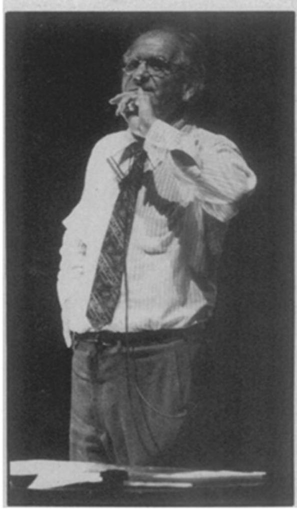
Anderson, who is formally credited with the discovery of the positron, tells us also, "The positron was discovered by accident." Folklore has it that P.A.M. Dirac's elegant theory of negative and positive energy states for electrons led to the prediction of the positron's existence, and then experimenters went and looked for it. Not so, says Anderson. They found the positron in the same series of cosmic ray observations, and for a while they couldn't figure out what it was — again because of the prejudice against particles that play no known role in the atom. In this case, he says, they knew of Dirac's theory, but it didn't influence them. So much for that.

What you remember can be delivered as

IN SEARCH OF A HISTORY







An experimentalist between two theorists is a way of illustrating one recurrent theme in the history of physics. These men are P. A. M. Dirac (top), Bruno Rossi (center), and Victor F. Weisskopf (bottom).

Photos: Fermilab

Rossi's "Some Personal Reminiscences of an Experimental Physicist" or as Robert Serber's memoir of a place, a time and a person (Berkeley in the 1930s with J. Robert Oppenheimer) or as Julian Schwinger's advocate-like brief fortified by documentary "exhibits" in which he sought to pin down the temporal sequence and the reasons for the steps in the development of the theory of relativistic quantum electrodynamics. One of the things that helps him is that the manner of writing the mathematical formulations changed about 1950, and by inspecting the mathematics in the documents he can date them before or after.

Relativistic quantum electrodynamics began in the 1940s. Work was pursued simultaneously in the West and in Japan, in Japan particularly by Sin-Ichiro Tomonaga. The Japanese physicists published in their own country in their own language, and because of the war, their work was unavailable in the West in any form. It is frequently said that if the Japanese work had been known in the West at the time it was done, development of the subject would have gone faster. "I will be bold enough to disagree," says Schwinger. "I know Oppenheimer told me about Tomonaga." The two groups were aiming at somewhat different goals.

In physics, history can also be whom you remember. Madeleine Barnothy pointed out that in any discussion of cosmic rays mention should be made of Victor Hess, whose reputation, she says, has been overshadowed by an acrimonious controversy with Robert A. Millikan. She had, she says, a long letter from Hess detailing these things, but "it had to be burned when we had to escape from Hungary." One of the historians assures her that independent documentation exists.

Millikan was what in the old slang of German physics students was called a *Bonze*. (The word comes from southeast Asia, where it refers to a senior and commanding Buddhist monk, one well on toward detachment.) Wolfgang Pauli was certainly another. During one of the discussions it was related that H.A. Kramers went to Pauli with a paper on electron spin. The idea that the serious problems encountered by the first formulation of quantum theory could be solved if the electrons had spin — whatever that is — had been kicking around for some time. Kramers was prepared to publish a paper alleging electron spin, according to the story, but Pauli "said something that crushed him." To this Victor Weisskopf, who had been Pauli's assistant for years, responded: "Anything Pauli said crushed somebody."

Conversely the two who are credited with the discovery of electron spin, Samuel A. Goudsmit and George E. Uhlenbeck, published, as it is recounted, because of the positive attitude of a Bonze. They took their paper to their professor, Paul Ehrenfest. Ehrenfest was a chance taker and sent the paper off to a journal quickly. When Goudsmit and Uhlenbeck came back to say they wanted to reconsider, it was too late. The paper was already in press.

History can also be where you were. Robert Serber speaks of "the isolation of our California group. We were too poor to travel. Mostly we relied on the journals. There were no preprints." Some things were done over because Berkeley physicists were unaware that others were doing them. Some things, however, were local. In the early thirties Oppenheimer became convinced that the proton must—like the electron—be a Dirac particle and have its own antiparticle. The antiproton was discovered at Berkeley two decades later.

Gilberto Bernardini remembers that in the same years Florence was a fortunate place to be doing physics research. There was a man named Garbasso, mayor of Florence, senator in the Italian parliament and, if Bernardini remembers correctly, sometime cabinet minister—and also director of the physics institute of the University of Florence. Garbasso used his political influence to get funds for his physicist colleagues to do their research. Someone else remembered a similar *padrone* in Rome, who had enabled Enrico Fermi to do his early research.

Later on in Italy things were not so rosy. Marcello Conversi told how he, Oreste Piccioni and others managed to do an important cosmic ray experiment at the height of the war. This was after the surrender of Italy and the consequent occupation of the country by the Germans. The physicists managed by using the services of a friend who was a black market expert to obtain components for the simplest circuitry they could design to do the job. When the Germans became suspicious about a bunch of Italians assembling electronic components, the experimenters moved their work "under the wings of the pope" into Vatican territory where the German authorities dared not set foot.

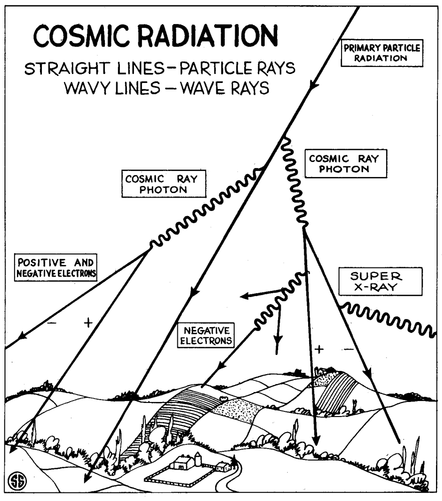
Where you were could be even more sinister. Rossi relates almost impassively — it is, after all, more than forty years ago — the increasing anti-Semitic actions of the Italian government. "In 1938 I learned that I was no longer an Italian citizen." Then it was Copenhagen, Manchester, Chicago and finally a stopping place at Cornell.

History can also be a case of attitude. An old adage says "the doctor's mistakes get buried." Statesmen's mistakes become countries and take hostages. Physicists' mistakes come somewhere in between. Those that are detected in the course of a piece of research are expunged from the published record. Those that happen to get published never die, but after their error is pointed out, their citations fade away. Most physicists would like, formally at least, to put mistakes into a limbo of forgetfulness. When historians probe them, physicists bristle, anticipating that someone wants to make fun of them.

Yet it would seem sometimes that physics proceeds by mistakes and confusion. Weisskopf speaks of "our complete disarray" in the early years of quantum mechanics. In Dirac's talk dealing with the same years, the phrase "that was my mistake" runs like a Wagnerian leitmotiv.

But "mistake" in the popular sense is perhaps too strong a word. One of Dirac's mistakes was a strong fascination with the symmetry of Niels Bohr's model of the atom. Dirac's work — he has said it on numerous occasions — has been a search for mathematical beauty, and he was slow to realize that the beauty of the Bohr model had to be supplanted, that "a new mathematics was necessary." Werner Heisenberg provided this in matrix mechanics, and Dirac now thinks that that was the great turning point of physics in that time: Heisenberg supplanted the commutative algebra of classical physics with the noncommutative algebra of the new physics. There was thus continuity and analogy to the past (all formally algebra) and revolution (non-commutative algebras had been a curiosity, not a piece of practical activity).

The counter to Dirac's ultimate mathematicism is provided somewhat surprisingly by Schwinger. The relativistic quantum electrodynamics that he is famous for is often presented as a beautiful mathematical enterprise unfortunately marred by the necessity of accommodating reality. If one writes the theory in its most mathematical form, the masses and electric



Schematic roughly contemporary with the photos on p. 10 illustrates cosmic-ray production of positrons but not "mesotrons."

charges of electrons come out either zero or infinite. Not only is this difficult for calculation, it is physically absurd: Experiment shows finite values for both those quantities. Schwinger says the theorists were continually inspired by experiment. Much of their effort was devoted to making the theory yield predictions that corresponded to actuality. They weren't forced to; they wanted to.

The historians probe and question. They look for turning points: The most dramatic change of direction due to unexpected discovery? Dirac says Heisenberg's substitution of noncommutative for commutative algebra. Weisskopf agrees: quantum mechanics. The others are not sure that alone stands out among the others. Considering the many instances of the opposition between holding on to old ideas and embracing the new, is there some rule of thumb about when you should be bold and when conservative? The physicists hem and haw. Finally Dirac sums it up with a forthright equivocation: "There are no rules about whether one should be bold or not."

So it goes. There is material here for a book or several books. Whether they will be history or at best biography depends on the answer to the question: Is physics some kind of hard, multifaceted crystal suspended perhaps in Dirac's infinitely dimensioned Hilbert space, and all we have to do to perceive it is to unwrap it properly? Or does the essence of physics have to do with the essences of the people who have worked on it? Most physicists have tended professionally and formally, if not in their heart of hearts, to state the former. Many who have observed them for years tend to the latter opinion. Perhaps the professional historians should decide — and convince. □

. . . Spinal cord

damaged spinal cords in an ice solution will reduce swelling and the damage it causes. Initial results in a few patients look promising, and the researchers now want to set up a multicenter trial to test the technique as scientifically as possible.

Some studies are attempting to find ways of optimizing patients' rehabilitation. For example, Terry Carle, a physician specializing in physical medicine and rehabilitation at Northwestern Memorial Hospital in Chicago, and colleagues have been measuring spinal cord patients' progress in activities of daily living such as washing their faces, combing their hair, getting out of their wheelchairs and so forth. They have found that paraplegics (spinal cord patients paralyzed from the waist down) make quicker progress while undergoing rehabilitation at the hospital than do quadriplegics (patients paralyzed from the neck down), probably bacause the former patients' injuries are less severe. However, the quadriplegics go on to make more progress in the months after they return home than do the paraplegics because, as Carle explains, "they are refining techniques they have been taught at the hospital."

Yet other investigators are trying to better understand physiologic disturbances resulting from spinal cord injury with an eye to correcting them. E. Eric Naftchi and his colleagues at New York University Medical Center have found that while male sex hormones are temporarily impaired in male paraplegics, they may be permanently disturbed in male quadriplegics. The researchers have also found that spinal cord injury interrupts sensory stimuli to the brain and in turn the release of adrenal hormones. The interrupted release of adrenal hormones may partly explain patients' drastic weight loss following spinal cord injury.

Still other research is aimed at exploiting current technology to make the lives of spinal cord patients easier and more fulfilling. Philip Fine and colleagues at the University of Alabama in Birmingham have developed a technological assist for the handicapped that they call "C2E2." Using a computer, the Alabama group wrote a series of special programs and put a voice link in it. A quadriplegic can talk to the computer, and it will perform various functions — switch appliances on and off, write letters on a typewriter, answer the phone. Says Fine, "The computer is a fun thing, but also a very helpful thing. There's a lot of international interest in it. It represents a significant breakthrough in the daily living of the handicapped."

The ultimate scientific challenge as far as upgrading the treatment of spinal cord patients goes, of course, is to find some way of regenerating central nerves in patients' severely damaged spinal cords so that the nerves will function again, and the patients will no longer be paralyzed. Some authorities in the spinal cord injury field, such as Paul Thomas, medical scientific administrator of the National Institute of Handicapped Research, think that spinal regeneration will probably not become functionally effective for many years. Others, such as Donald Tower, director of the National Institute of Neurological and Communicative Disorders and Stroke, are more sanguine (SN: 10/27/79, p. 277).

Whether regeneration ever becomes possible or not, though, there is no doubt that research efforts to improve the care of spinal cord patients are already leading to beneficial results. The encouraging preliminary data from the 14-model spinal cord care system study have led to the establishment of similar spinal care centers at numerous medical schools and hospitals throughout the United States.

And the greatest benefits from research to upgrade the care of spinal cord patients are probably still to come. As Thomas predicts: "Ongoing clinical research and demonstrations are going to influence the national medical community into better recognizing spinal cord patients' potential and to more fully meet their needs. There will ultimately be a nationwide network of high-quality spinal cord injury treatment centers. In addition, our social and community agencies will be more responsive to the community needs of the spinal cord injured in terms of family adjustment programs, independent living activities, recreational programs and work. So I feel very confident about the future for spinal cord patients." □