Particles of History

They are ill discoverers that think there is no land when they can see nothing but sea.
— Francis Bacon

Chronicling the emergence of the standard model of particle physics

By IVARS PETERSON

Like any tribal group, physicists have a way of preserving their collective experience in a fund of telling anecdotes about one another.

The late George Gamow was a master of this particular form of storytelling. In his classic book *Thirty Years That Shook Physics,* he illuminated the peculiar relationship between theorists and experimenters with the following story about Wolfgang Pauli and the widely held notion that theoretical physicists can't safely handle experimental equipment:

"Pauli was such a good theoretical physicist that something usually broke in the lab whenever he merely stepped across the threshold. A mysterious event that did not seem at first to be connected with Pauli's presence once occurred in Professor [James] Franck's laboratory in Göttingen. Early one afternoon, without apparent cause, a complicated apparatus for the study of atomic phenomena collapsed. Franck wrote humorously about this to Pauli at his Zurich address and, after some delay, received an answer in an envelope with a Danish stamp. Pauli wrote that he had gone to visit [Niels] Bohr and at the time of the mishap in Franck's laboratory his train was stopped for a few minutes at the Göttingen railroad station."

Gamow added: "You may believe this anecdote or not, but there are many other observations concerning the reality of the Pauli Effect!"

Last June, particle physicists — both theorists and experimenters — had a chance to tell their own stories. The occasion was the Third International Symposium on the History of Particle Physics, held at the Stanford Linear Accelerator Center (SLAC) in Palo Alto, Calif.

More than 100 physicists, historians, philosophers, and others had gathered to hear, first hand, tales of the great intellectual adventure that had produced the so-called standard model of particle physics, which describes the elementary particles of matter and the forces by which they interact.

Starting in the 1960s with the formulation of the quark hypothesis, and followed in the 1970s by an explosion of crucial experimental and theoretical results, the standard model developed out of notably murky, confused ideas to emerge in the 1980s as the dominant, unchallenged theory of particle physics (SN: 9/29/90, p.204).

The symposium offered the pioneers, who muddled through this compelling quest to understand the basic structure of matter, a chance to look back at the false starts, wrong turns, dead ends, and general confusion — interspersed with strokes of incredible luck — that typify research at the frontiers. Old timers could reminisce about the long-gone days when no fax machines or electronic mail instantly conveyed results anywhere in the world, when collaborations included fewer than 100 physicists, and when the need for new experimental equipment didn't necessitate a congressional debate.

This exercise in the writing of history was not without its perils. Some participants eschewed the human drama to present dry, uninspired, expurgated accounts of their work, sliding past the controversies and rivalries of the times. Others, forced to look again at laboratory notebooks long stored away, were struck by how sloppy and poorly documented much of the material was. And inevitably faulty, incomplete memories of events 20 or more years old took their toll.

As Leon M. Lederman, now at the Illinois Institute of Technology in Chicago, remarked, it wasn't entirely clear whether the conference's purpose was "to set the record straight or to make it as crooked as it could be."

"When I started [in the 1950s], theorists were totally discredited," recalls Murray Gell-Mann of the California Institute of Technology in Pasadena, who shares credit for proposing that such particles as the proton and neutron are themselves composed of more fundamental, fractionally charged entities called quarks. At that time, physicists had no clear notion of how to rationalize the bewildering menagerie of particles they had discovered in their accelerators.

Regarded with suspicion by most physicists for a long time, the idea of quarks took hold only very slowly. "This business of defending quarks over all those years was not an easy one," Gell-Mann notes.

He quarrels with various accounts of how he came up with the designation "quark" — stories insisting that he borrowed the name from a phrase in James Joyce's *Finnegan's Wake.* Gell-Mann says he had simply decided he wanted a straightforward, somewhat playful tag for these particles, and he came up with something that sounded like "kwork." Only later did he come across the reference to "quarks" in Joyce's book.

It also bothers Gell-Mann that a phrase he used in an early paper stating that quarks are "mathematical" rather than "real" objects has so often been misinterpreted. "I did not mean that they weren't there," he contends. "I had always assumed their reality. [But] I wanted to avoid painful arguments with philosophers . . . asking how you would see such a thing."

Whatever Gell-Mann really meant, the idea that quarks were "fictitious, mathematical devices" nonetheless became an integral part of the way most physicists initially looked at the theory. "One might 'abstract' properties of quarks from some model, but one was not allowed to believe in their reality or to take the models too seriously," says David J. Gross of Princeton University.

Gross also remembers his response to an important 1964 paper by Gell-Mann describing a method for extracting useful relations from field theory. In Gell-Mann's words, "We may compare this process to a method sometimes employed in French cuisine: a piece of pheasant meat is cooked between two slices of veal, which are then discarded."

Says Gross, "This paper made quite an impression, especially on impoverished graduate students like me, who could only dream of eating such a meal." Gross went on to play a major role in the 1973 discovery of "asymptotic freedom" by demonstrating theoretically that the force between quarks could become quite weak at short distances.

The existence of this behavior explained why high-energy electrons and neutrinos in high-speed collisions with neutrons and protons appeared to be hitting loosely bound quarks. It also led to quantum chromodynamics as the appropriate mathematical framework for describing the so-called strong force between quarks.

The same period also saw a number of remarkable experimental results, including the discovery of particles that required the existence of a fourth, "charmed" quark to supplement the original "up," "down," and "strange" quarks, which made up the other known particles affected by the strong force. Researchers also found the tau lepton, a charged elementary particle that, like the electron and muon, is immune to the strong force.

In describing these experiments, some presenters proceeded along a straight and narrow path from hypothesis to discovery. In contrast, Lederman was among those who took particular delight in revealing how convoluted and fortuitous that path really was.

Lederman portrayed the steps leading to the discovery at Fermilab in 1977 of a particle containing the fifth, "bottom" (or "beauty") quark as a story of "missed opportunities, stupid mistakes, inoperative equipment, dismal judgments – and incredible luck." Indeed, he notes, luck "is an essential part of a career in physics."

When discussing the experiments that led to the discovery of the tau particle, SLAC's Martin L. Perl highlighted the fact that in the original proposal for building the detector in which the particle was identified, "there was nothing . . . hinting at what was actually discovered. No one was sure what they were looking for."

In this and other cases, theorists provided little guidance, not because they didn't have any ideas but because there were too many competing theories. Out of the many "flavors" of models available, experimenters often had no obvious criteria for choosing the one on which to stake their experiments.

"Experiment and theory don't always go hand in hand," comments Gösta Ekspong of Stockholm University in Sweden. "Maybe such decoupling is necessary for setting the stage for discoveries."

Symposium presentations furnished evidence that physics does not evolve in an orderly, rational fashion. What order there is appears more out of hindsight than foresight – the gradual emergence of a coherent picture out of conflicting currents and irrelevant side branches.

It's not so much a process of induction and deduction as of natural selection, says Steven Weinberg of the University of Texas at Austin.

But the symposium, even with videotaped and printed proceedings, couldn't do justice to the rich texture of physics research. There was too little time to explore the nuances and unravel the contrary perceptions of the intricate communal process that produced the standard model.

"Science progresses in a much more muddled fashion than is often pictured in history books," Gross contends. "This is especially true of theoretical physics, partly because history is written by the victorious. Consequently, historians of science often ignore the many alternate paths that people wandered down, the many false clues they followed, the many misconceptions they had."

As one example, Gross notes that the development of particle physics suffered in its early years from the arrogance of many of its practitioners in ignoring such fields as solid state physics. "Particle physicists thought they had little to learn from 'dirt physics' (or 'squalid state physics')," Gross says. "This attitude was unfortunate. We could have profited much from a deeper study of superconductivity – the preëminent advance in condensed matter physics in this period."

Historian John Krige of the European University Institute in Florence, Italy, echoes this sentiment in his plea to physicists to collect and save documents so that historians will have the raw material necessary for their studies.

"We want to look at the way scientific knowledge is produced," Krige says.

Instead of beginning from the present to reconstruct the past, historians like Krige try to recreate the situation as the participating figures saw it – back when they didn't know who was going to win or lose.

Many of the physicists speaking at the conference felt uncomfortable, however, with the notion of turning over their reminiscences to historians and philosophers of science they weren't sure they could trust. Much of this discomfort results from their perceptions of the attitudes of a vocal group of historians, philosophers, and sociologists who have in the last few years mounted a concerted attack on science.

These critics argue that science has an undeserved reputation for objectivity. They maintain that social interests determine not only how science is done but also its content. Science itself is a matter of opinion – decided on the basis of personal or political beliefs, they insist. It is nothing but a social construction, perhaps even a group self-delusion.

"I think this is really all quite wrong," Weinberg replies.

Most physicists contend there is an objective reality. The search for understanding resembles climbing a peak, Weinberg says. There are many paths to the top, and one can argue over what paths to take, but eventually one gets there.

Nonetheless, the making of science is a messy process. Studies of contemporary science have revealed that there really is no universal "scientific method." Moreover, observations depend to some degree on the way the observer construes the situation and on the theoretical notions he or she brings to the experiment.

At the same time, many historians and philosophers don't take the extreme view that science is merely a product of human whim. Science itself – aimed at the production of general, reliable knowledge about the world – is constrained by the way the world is, they readily admit.

"We can always conjecture, but there is some control. The world kicks back," says Michael Redhead, professor of the history and philosophy of science at the University of Cambridge in England.

"There is an objective reality," he emphasizes. "But we're nowhere near to grasping it in its totality, . . . and if we did get to the truth, would we ever know it?"

The product of a communal effort, the establishment of the standard model of particle physics required the inventiveness of engineers, who built the accelerators, detectors, and computers; the perseverance of experimenters, who designed the apparatus and experiments and analyzed the data; the insights of theorists; and the effectiveness of those individuals who made high-energy physics a national priority, says Silvan S. Schweber of Brandeis University in Waltham, Mass.

"The establishment of the standard model is one of the great achievements of the human intellect – one that rivals the establishment of quantum mechanics," Schweber contends.

"We should be very happy with what we have accomplished and to look to the future with confidence," Gell-Mann adds.

But there are question marks everywhere. Despite its great success, physicists realize that the standard model of particle physics is incomplete, perhaps only a low-energy approximation of the "true" theory of matter. Yet they have few clues to tell them in which direction to proceed to repair the model (SN: 7/11/92, p.30).

"The standard model is too good," says SLAC Director Burton Richter. "It predicts too well. It has made life . . . difficult for experimenters."

When will the next history symposium – devoted to topics "beyond the standard model" – be held? Will it take place in a few years or a few decades? That is the question now hanging over particle physics. □