FRIEDMAN, KENDALL AND TAYLOR WIN NOBEL PRIZE FOR FIRST QUARK EVIDENCE

The 1990 Nobel Prize in Physics has been awarded to Jerome Friedman and Henry Kendall of MIT and Richard Taylor of SLAC "for their pioneering investigations concerning deep inelastic scattering of electrons on protons and bound neutrons, which have been of essential importance for the development of the quark model in particle physics." The prize of $710 000, which the three recipients shared equally, was awarded in Stockholm on 10 December.

The 1990 Nobel laureates join hands at a SLAC fest following the announcement of the award. From left: Richard Taylor, Henry Kendall and Jerome Friedman.

Friedman, Kendall and Taylor were honored for a series of experiments from 1967 and 1973 that used the then-new two-mile electron linear accelerator at Stanford to study deep inelastic scattering of electrons from protons and neutrons. The SLAC experiments were somewhat analogous to the experiment by Ernest Rutherford that gave evidence for a hard core within the atom: Just as Rutherford's observation of large numbers of alpha particles being scattered at large angles led him to postulate a nucleus within the atom, the SLAC finding of unexpectedly large numbers of electrons being scattered at large angles provided clear evidence for pointlike constituents within nucleons. These constituents are now understood to be quarks.

Quarks had been predicted in 1964 by Murray Gell-Mann and independently by George Zweig at Caltech. Until the SLAC–MIT experiments no one had produced convincing dynamical evidence from experiment for the existence of quarks inside the proton or neutron. In fact during that period many theorists were not sure about the role played by quarks in the theory of hadrons. As Cecilia Jarlskog said at the Nobel ceremony when (on behalf of the Nobel committee for physics) she presented the winners to the king of Sweden, "the quark hypothesis was not alone. There was, for example, a model called 'nuclear democracy' where no particle had the right to call itself elementary. All particles were equally fundamental and consisted of each other."

SLAC and its detectors

In 1962 construction began on the large Stanford linac, which had a proposed energy of 10–20 GeV; eventually it reached 50 GeV over a series of many steps. Two years later SLAC director Wolfgang Panofsky enlisted the help of several young physicists he had worked with when he was director of the Stanford High-Energy Physics Laboratory. Among these was Taylor, who had returned to Stanford in 1962. He agreed to take charge of the beam switchyard, which linked the accelerator proper with the experimental areas. Around the same time the laboratory established a number of experimental teams, one of which was headed by Taylor.

Soon he was joined by Friedman and Kendall, who were by then on the MIT faculty. They had been doing electron scattering experiments at the 5-GeV Cambridge Electron Accelerator, which had limited capacity and "was a circular machine, with all that that limitation meant," Kendall recalls. But at Stanford there was to be a 20-GeV machine going on line with an "absolutely ferocious beam," high current density and external beam, and immediate availability for experimental use. A group from Caltech led by Barry Barish, Jerome Pine and Charles Peck joined the collaboration but concentrated its work on the comparison of electron–proton with positron–proton scattering.

A large team of experimenters from SLAC, MIT and Caltech decided to build two spectrometers. Stanford experimenters included Panofsky, Taylor and their collaborators, who were interested in electron scattering; Burton Richter and his collaborators, who were interested in photoproduction; and David Ritson, who eventually built a third spectrometer at 1.6

GeV. The team, designated as Group A, decided to build a spectrometer at 8 GeV with a large angular acceptance and another at 20 GeV with a smaller angular acceptance.

Taylor managed the construction of the spectrometer facilities; the entire group took part in the design effort. David Coward directed the construction of the end station and then returned to the group to take a major role in the construction of the spectrometer facilities. The MIT group built the hodoscopes and counters to measure electron momentum and scattering angle, and it supervised the procurement and installation of the counting electronics.

Planning the experiment

The spectrometer designs differed from earlier spectrometers in that they used line-to-point focusing in the horizontal direction rather than the older instruments' point-to-point focusing, Taylor explained to us. The new design allowed the scattering angle to be dispersed in the horizontal direction and the momentum to be dispersed in the vertical direction, permitting measurement of the momentum to 0.1% and of the scattering angle to a precision of 0.3 milliradian. The very sophisticated data acquisition system was designed and built by a group headed by Adam Boyarski and Les Cottrell of SLAC.

Friedman told us the Group A collaborators knew they wanted to study elastic scattering and inelastic scattering, especially resonance production. "We also wanted to do a survey of the deep inelastic scattering because we wanted to look at everything," he added. At that time mainstream physics viewed the proton as not having pointlike constituents. The experimenters expected to see the cross section decrease rapidly with increasing q^2, where q is the momentum transfer to the nucleon; in other words, they expected very little large-angle electron scattering. Experiment proved otherwise.

According to Kendall, "the specific discovery was a discovery. We did not know it was there, and neither did anybody else in the world—the people who had invented quarks and the entire theoretical community. Nobody had said specifically and unequivocally: 'You folks go look for quarks. I expect them in the nucleon.' "

In planning the experiment the collaborators estimated the expected counting rates. Their projections used a variety of theoretical assumptions, none of which included constituent particles. One of these assumptions was a ballpark estimate extending photoproduction by "real photons" to "off the mass shell" virtual photons involved in electron scattering. This extension used the structure functions observed in elastic scattering. "In retrospect," says Kendall, "it was wrong by one or two orders of magnitude" as a consequence.

Bjorken scaling ideas

But SLAC theorist James Bjorken had other ideas. He remembers hearing Leonard Schiff say in 1961 at a Stanford colloquium that inelastic scattering was the way to see the instantaneous charge distribution inside the proton. This idea built on theoretical work at Stanford by Sidney Drell and Charles Schwartz that showed how the inelastic scattering of electrons from a nucleus gave the momentum distributions of protons and neutrons within that nucleus.

As a Stanford graduate student Bjorken, together with Louis Hand, had already worked out the kinematics of inelastic scattering. When Bjorken returned to Stanford in 1965 it was natural for him to return to the subject "with electrons in mind because of my environmental situation." In that period, Gell-Mann recently told us, he had introduced current algebra, abstracting it from an earlier field theory known to be wrong. Then Gell-Mann threw away the wrong field theory and kept the commutation relations of current algebra. He has compared the process to a gourmet recipe in which a slice of pheasant is cooked between two slices of veal, which are then discarded.

Stephen Adler, among others, had been using local current algebra to derive sum rules for neutrino interactions. Bjorken spent the next two years obsessed with trying to understand high-energy electron and neutrino scattering using current algebra. "It forced me to try to figure out not only what the integrals over the sum rules for structure functions would turn out to be, but also what the shape and size of the structure functions would be."

The elastic scattering showed no surprises. The form factors describing the proton charge distribution continued the sharply decreasing trend of earlier data and did not contradict smoothness of the charge distribution. Particle physicists at that time did not realize that elastic scattering just sampled the entire nucleon simultaneously so that you saw "a smear of charge distribution even when you had multiple constituents," Friedman says.

By the time Group A started to run its deep inelastic scattering measurements, Bjorken had his picture worked out. "I was trying to figure out what was the most reasonable answer for the size and shape of the structure function. I was using a lot of parallel approaches. The most speculative of them was pointlike constituents." Current-algebra sum rules were suggestive of pointlike constituents, but did not require them. In addition to the notion of constituents, other strong-interaction concepts, such as Regge poles, were useful; these concepts helped to determine how the sum rules converged. "Putting these and other ideas together," Bjorken said, "it was very natural but not imperative that the structure function would scale."

Analyzing the data

In the latter part of 1967 and early in 1968 the experimental data on deep inelastic scattering began to accumulate. As Friedman and Kendall tell it, Bjorken presented his ideas of scaling and pointlike behavior very tentatively. Friedman says: "The ideas were all there, yet we were not really totally appreciative. He was a young man and we felt the ideas were very speculative. So when we planned the experiment we didn't expect to see pointlike structure. We expected a big mush."

As soon as the radiative corrections had been calculated, Kendall told us, two immediate features showed up: Bjorken scaling and the independence of the cross section as momentum transfer was varied. "Those were grade A, solid-gold surprises."

The structure functions W_1 and W_2 describing the process, Bjorken told us, are in general functions of two variables taken to be the virtual photon energy ν and q^2. Bjorken had suggested that the most important structure function, W_2, would essentially depend only on the ratio of these variables, $\omega = 2M\nu/q^2$, where M is the proton mass:

$$\nu W_2 = F(\omega)$$

When Kendall showed Bjorken the freshly analyzed data, Bjorken suggested that they be analyzed in terms of the scaling variable ω. Plotted the old way, Kendall says, "the data were strewn all over. It looked like chicken tracks all over the graph. When it was treated according to Bjorken's prescription [νW_2 vs ω] it all nested in quite a moving and powerful way. I recall speculating as to how Balmer felt when he discovered his empirical relation—when the wavelengths of the hydrogen spectrum fitted with absolutely stunning precision."

Eventually the data turned out to nest for two limiting values of a then-unknown parameter, R. Later measurements of R refined a single universal curve for νW_2.

Because SLAC was and still is a unique accelerator, "we weren't under any competitive presure," Kendall said. Data analysis was done independently on the West and East coasts. Theory, radiative correction formulas, computer programs and analysis were all done independently. The first results were reported by Friedman at the 14th International Conference on High-Energy Physics in August 1968. Panofsky, as a rapporteur at the meeting, diffidently raised the possibility of pointlike structure in the nucleon.

Once the data were taken at 6° and 10° angles with the 20-GeV spectrometer, Group A turned to the 8-GeV spectrometer to cover 18°, 26° and 34°. The resulting data allowed them to determine the second structure function, W_1, which also was found to behave as a function of the single variable ω—that is, to obey Bjorken scaling.

The experimenters waited for formal publication[1] until all the cross-checks were completed. The results stand to this day, according to Kendall. Even when better radiative corrections were applied, the results changed by less than 1%.

Starting in 1970 the experimenters did similar scattering experiments with neutrons, interlacing an hour's run with hydrogen (protons) and an hour's run with deuterium (neutrons) to reduce systematic error.

Feynman, partons and quarks

In 1968 Richard Feynman of Caltech had been thinking about hadrons as being made of smaller pieces he called "partons." When he visited SLAC in August of that year he was shown the inelastic scattering data, along with fits to the Bjorken scaling law. (See the article by Bjorken, "Feynman and Partons," in PHYSICS TODAY, February 1989, page 56.) Friedman reminisces: "Feynman came up with a simple dynamical model which experimenters could really understand. It was another way of saying what Bjorken was saying except it gave it a sort of physical structure." Feynman identified the scaling variable $x = 1/\omega$ with the fraction of momentum the parton carried in a highly relativistic nucleon. If the partons were pointlike you'd get precisely this scaling. He also showed that the structure function was related to the momentum distribution of the partons.

Friedman cites three reasons for the unpopularity of quarks at that time: They had not been seen. The fractional charge assignments appeared unreasonable. And since quarks had not been observed in cosmic rays, they were expected to be very heavy and accordingly to have very strong binding energy, suggesting a great difficulty in thinking of them as independent constituents.

Feynman's work greatly stimulated the theoretical community and a variety of theories emerged. After Curtis Callan and David Gross showed that a particular ratio R of W_1 and W_2 would be sensitive to parton spin, the SLAC–MIT group found that this ratio was consistent with partons with spin $\frac{1}{2}$, just as Gell-Mann had required for the quarks. That eliminated certain competing possibilities. Once the neutron data were analyzed[2] it became clear that the neutron yields differed from the proton yields, eliminating some other competing theories.

Quarks are accepted

Within a year or so complementary measurements of neutrino inelastic scattering at CERN's Gargamelle heavy-liquid bubble chamber provided a powerful extension of the SLAC–MIT results. If one divided the SLAC deuterium results by $\frac{5}{18}$ (the value obtained by taking the average of the squares of the quark charges, or $\frac{1}{2}[(-\frac{1}{3})^2 + (\frac{2}{3})^2]$) to account for the difference between the electromagnetic interaction between quark charges and the weak currents in neutrino interactions, "the neutrino data lay right smack on the SLAC results," according to Kendall. The neutrino and electron data taken as a whole gave very strong evidence that the constituents were quarks.

There followed deep inelastic muon scattering, electron–positron collisions and proton–antiproton collisions showing quark–quark interactions. Then hadron jets showed up.

It took several years for the community of physicists to accept quarks, largely because of the contradiction between their appearance as pointlike constituents and their very strong binding within hadrons. Even then, as Jarlskog said at the Nobel ceremony, "the results could not be entirely explained by quarks alone. The Nobel Prize-winning experiment indicated that the proton also contained electrically neutral constituents. These were soon found to be 'gluons,' particles gluing the quarks together in protons and other particles."

JOAN CARTIER

ALRIGHT RUTH, I ABOUT GOT THIS ONE RENORMALIZED.

In 1973 the asymptotic freedom of non-Abelian gauge theories was discovered by Gross and Frank Wilczek and independently by H. David Politzer. Asymptotic freedom says that if the interaction of quarks is mediated by colored gauge gluons, then the coupling between quarks vanishes logarithmically at short distances. The asymptotically free gauge theory of quarks, later dubbed quantum chromodynamics, could easily explain all of the SLAC results (albeit with logarithmic corrections to scaling). In addition, the flip side of asymptotic freedom, the growth of the coupling at large distances (called infrared slavery) provided a mechanism for quark confinement.

The father of quarks, Gell-Mann, said in 1972 at the 16th International Conference on High-Energy Physics: "Real quarks detectable in the laboratory are not required by theory. In this respect they're like the magnetic monopoles. . . . They may conceivably exist, but they do not fill any obvious theoretical need." He told us recently that earlier he had defined "real quarks" as those that could be isolated in the laboratory and "mathematical quarks" as ones that cannot—for example, because they are trapped in an infinitely high potential (as is now indeed believed to be the case). According to Gell-Mann this terminology "proved to be a terrible choice of words because many people misunderstood what I meant." Although he was not surprised when quarks showed pointlike behavior in the SLAC electron scattering experiments, he never expected that we would eventually get "such a beautiful glimpse of quarks as hadron jets provide."

The inelastic electron scattering experiments were "the basement for the construction of quantum chromodynamics," says Kendall. "Then you got the standard model. And that is such an astonishing success that it's depressing. There's nothing left to do."

The laureates themselves

The day Taylor learned he had won the Nobel Prize, he put a note on the mirror he was using while shaving. It read: "Murray is smart. Pief is smart. Dick Garwin is smart. You are lucky." Taylor received his BSc in 1950 and MSc in 1952 from the University of Alberta. He then went to Stanford and from 1954 to 1958 did his thesis research, for which he was awarded his PhD in 1962. From 1958 to 1961 he was at the Ecole Normale Supérieure and helped build the Orsay linac. After a year at Lawrence Berkeley Lab, he went to SLAC. He became an associate professor at SLAC in 1968 and a professor in 1970. Taylor is still trying to do electron scattering from nucleons, but now he is working at HERA in Germany, where the momentum transfer squared is 100 times higher than in the SLAC experiments. This time, he says, he is not trying to take a leading role.

Friedman earned an AB in 1950, MS in 1953 and PhD in 1956, all from the University of Chicago. After a year more at Chicago, he spent three years at Stanford as a research associate. He joined MIT as an assistant professor in 1960, was promoted to professor in 1967 and was head of the physics department there from 1983 to 1988.

Kendall earned a BA from Amherst College in 1950 and a PhD in physics from MIT in 1954. After two years as a postdoc at Brookhaven and MIT, he went to Stanford as an assistant professor. In 1961 he joined the MIT faculty, and he became a professor in 1967. Kendall is chairman and a founding member of the Union of Concerned Scientists.

—Gloria B. Lubkin

References

1. E. D. Bloom, D. H. Coward, H. DeStaebler, J. Drees, G. Miller, L. Mo, R. E. Taylor, M. Breidenbach, J. I. Friedman, G. C. Hartmann, H. W. Kendall, Phys. Rev. Lett. **23**, 930 (1969). M. Breidenbach, J. I. Friedman, H. W. Kendall, E. D. Bloom, D. H. Coward, H. DeStaebler, J. Drees, L. W. Mo, R. E. Taylor, Phys. Rev. Lett. **23**, 935 (1969).
2. A. Bodek, M. Breidenbach, D. L. Dubin, J. E. Elias, J. I. Friedman, H. W. Kendall, J. S. Poucher, E. M. Riordan, M. R. Sogard, D. H. Coward, Phys. Rev. Lett. **30**, 1087 (1973). J. S. Poucher, M. Breidenbach, W. R. Ditzler, J. I. Friedman, H. W. Kendall, E. D. Bloom, R. L. Cottrell, D. H. Coward, H. C. DeStaebler, C. L. Jordan, H. Piel, R. E. Taylor, publication 1309, SLAC, December 1973.

EARLY RADAR PICTURES FROM MAGELLAN YIELD SOME SURPRISES

As the Magellan spacecraft orbits Venus its radar instruments are penetrating the thick clouds that prevent optical cameras from photographing the planet's surface. A portion of the Venusian surface was unveiled at a meeting of the American Geophysical Union in San Francisco in early December when mission scientists presented radar images of the planet and discussed their first tentative interpretations of what those images reveal. The images on these pages represent only a few of the strikingly diverse types of terrain that have been seen so far, with the mission having mapped only 15% of the surface.

Launched from the shuttle Atlantis on 4 May 1989, the unmanned craft settled into orbit around our nearest neighbor some 15 months later, and began its 243-day primary mission to image the surface with radar, to gather altimetry and radiometry data and to map the planet's internal density distribution. A few initial spacecraft and communications problems, which have since been surmounted, resulted in a loss of slightly less than 3% of the data.

For radar imaging Magellan uses side-looking, synthetic-aperture radar that tracks 20-km-wide swaths of the surface at roughly 120-m resolution as the satellite orbits the planet once every 3 hours and 15 minutes. In addition to the side-looking radar, Magellan has a radar altimeter that looks directly down at the planet's surface to measure its topography with an accuracy of a few tens of meters vertically. Between radar pulses, Magellan's main antenna passively receives radiation from the surface to measure its thermal emission.

The only nonradar study in the mission is the gravity field experiment, which monitors the slight variations in the spacecraft's speed that may result from topographical features or from density gradients within the planet. Curiously, data from earlier Venus missions show a strong correlation between gravity field variations and topography at long wavelengths—a correlation not observed on Earth.

Pristine features

Even in these early stages of the mission the images reveal a rich variety of geologic features, which project scientist R. Stephen Saunders of the Jet Propulsion Laboratory