NOBEL PHYSICS

Quark-hunters are rewarded

Washington

THE Nobel physics prize committee this year took what may be the final step in its curiously unchronological recognition of the experimental discoveries that have established, over the past few decades, the generally accepted standard model of elementary particle physics. The winners, Jerome Friedman and Henry Kendall of the Massachusetts Institute of Technology and Richard Taylor of the Stanford Linear Accelerator Center (SLAC), collaborated in 1967 on electron–proton scattering studies at SLAC that demonstrated the existence of smaller particles inside the proton. Murray Gell-Mann, a co-inventor with George Zweig of these subparticles, named quarks, won the physics prize in 1969, but, for reasons the secretive Swedes are unlikely to reveal, it has taken another 21 years for the experimental work that substantiated Gell-Mann's hypothesis to be similarly rewarded.

Although the results obtained by Friedman, Kendall and Taylor turned out to be of fundamental importance, their experiment was originally seen as a long-shot at best, a waste of accelerator time at worst. When it came on-line in 1966, the Stanford Linear Accelerator was mostly devoted to studies of elastic electron scattering, in which the aim was to record the deflections and energy losses of electrons bounced off protons. Inelastic scattering experiments, which Friedman, Kendall and Taylor decided to pursue, were referred to by some as "beam surveys": an electron was slammed into the protons at high energy to create a shower of new particles, and the debris was examined to see if any useful secondary beams (of muons, for example) could be extracted.

The standard wisdom at the time was that the mess of debris created by inelastic scattering was too complex to be understood in a way that would shed light on the inner structure, if there were any, of the proton. Elastic scattering, the modern version of Rutherford's famous experiment in which he scattered alpha particles off atoms to show the presence of small, dense, electrically charged atomic nuclei, was instead thought to be the way to probe the proton. But the first round of results from elastic scattering experiments showed nothing of interest, indicating only that the proton seemed to be a smooth, structureless distribution of charge. In retrospect, this was inevitable. Elastic scattering could have picked up the existence of a hard core to the proton, as in the Rutherford experiment, but charged quarks moving rapidly about inside the proton would, to the passing electron, be indistinguishable from a smooth charge distribution.

Inelastic scattering produced, as expected, a complex mess of new particles, but Friedman, Kendall and Taylor found that the statistical properties of this mess behaved in a relatively simple way at higher energies. This 'scaling' of the results of deep inelastic scattering (deep because the more energetic electrons were able to penetrate further into the proton) did not have any immediate interpretation, but it struck some theoretical physicists, in particular James Bjorken and Richard Feynmann, as a clue to some underlying simplicity in proton structure.

Feynmann realized that scaling made sense if, at high energies, the electrons were interacting with individual subunits of the proton rather than with its constituents as a whole. He called the subunits partons, and argued that the results of deep inelastic scattering showed that the partons moved rather freely inside the protons. This seemed to be at odds with Gell-Mann's theory, because his quarks had to be strongly interacting. At the same time, according to Bjorken, there were doubts about the reality of the scaling laws, and also alternative explanations for them that involved not proton substructure but new dynamical effects in the electron–proton interaction.

The programme of deep inelastic scattering studies was to continue for many years, accumulating results that allowed different explanations to be distinguished. On the theoretical front it was eventually realized that, at sufficiently high energies, quarks could theoretically behave at close range as if they were free particles, but still be inextricably bound up inside the proton.

By the mid-1970s, quarks had become real particles, not abstractions. And in the long run the success of the quark model put physics on the road towards unification mania, the goal of which is to assemble all the forces of nature into some single, all-encompassing model.

This year's physics award, in conjunction with that of two years ago, when Leon Lederman, Melvin Schwartz and Jack Steinberger were made Nobel laureates for their discovery of neutrinos in the early 1960s, corrects an omission that elementary particle physicists had long noted. In contrast, the 1983 discovery of the W and Z particles, crucial to unification of the weak and electromagnetic forces, was promptly recognized when Carlo Rubbia and Simon van der Meer won the physics prize the following year.

With most notable particle discoveries now rewarded, it may be that physicists will have to find the top quark or the electroweak Higgs boson before the Nobel will come their way again.

David Lindley

NOBEL CHEMISTRY

Corey the logical choice

London

THIS year's Nobel prize in chemistry has been awarded to Elias Corey of Harvard University, Massachusetts, for his contributions to synthetic organic chemistry. Corey is widely credited by chemists as having played a key part in transforming organic synthesis from something resembling a 'black art' to a discipline founded firmly on logic. That he has won the prize alone rather than sharing it is itself a testament to his position in the field and the pervasive influence of his ideas.

Over the past three decades, Corey has devised synthetic routes to more than 100 natural products, many of which have found wide use in medicine and industry. He is perhaps best known for leading the group that in 1969 made the first synthetic prostaglandins, molecules involved in the regulation of among other things blood pressure and the heart. But it was not so much his prolific output that attracted the Nobel Committee as his strictly logical approach to complex syntheses.

In the 1950s, at the start of his career, organic synthesis was largely a trial-and-error pursuit. Progress was being made in understanding the mechanisms of organic reactions and in developing useful reagents, but synthetic chemists still analysed target molecules case by case.

In the absence of general rules, the prevailing approach was to identify a possible starting subunit (a readily available reagent, for example) within the structure of the target molecule. The problem then became how to manipulate the subunit so as to generate the full structure.

This approach led to some notable successes, such as Robert Woodward's heroic synthesis of chlorophyll, one of the achievements that earned him the Nobel prize in chemistry in 1965. Yet by and large, chemical manipulations were still deployed in an *ad hoc* fashion, a practice that remained the norm until the 1960s when Corey began working on sesquiterpenes, a family of natural products whose structures were void of obvious starting subunits.

Corey reasoned that to devise logical synthetic routes to such molecules, it was necessary to work backwards from the end product, disconnecting chemical structure until one obtained a set of simple precursors. In seminal papers published in 1967 and 1968, Corey wove his ideas into a general synthetic strategy, coining the term 'retrosynthetic' analysis to describe it.

Put crudely, the analysis involves subjecting the target molecule of the synthesis to a series of stepwise dissections, according to a set of simple rules bearing on the character and structural context of the chemical bonds being broken. Ultimately the analysis generates a 'tree' of possible synthetic intermediates.

The obvious advantage of the approach was that it made synthesis systematic. Nevertheless, it was only after Corey had successfully used it to synthesize a host of natural products that it became widely accepted. Now it is standard laboratory practice. "No one embarks on a synthesis these days without applying the retrosynthetic method", says John Mann, reader in organic chemistry at the University of Reading.

Corey was also quick to realize the potential power of the computer as an aid to analysing the hundreds of intermediates that can result from a retrosynthesis. In recent years, programmes have emerged that not only generate intermediates but offer guidance on picking the simplest route through them to the target molecule.

Born in 1928, Corey received his bachelor's degree from the Massachusetts Institute of Technology in 1948 and doctorate there two years' later. From 1951 to 1959 he rose through the ranks at the University of Illinois before moving to Harvard where he has been ever since. He is the sixth Harvard professor to win the prize in Chemistry, and his award brings the total number of Nobel laureates on the Harvard faculty to 18. It is also the latest in a run of awards to Americans.

Finally, for scientists who consider teaching to be the bugbear of academic life, this year's chemistry prize holds a moral: it was Corey's quest for a more logical way of teaching organic synthesis that originally shaped his thinking about the subject. He has been much quoted since for maintaining that teaching and fundamental research are two sides of the same coin. **David Concar**

AUSTRALIAN UNIVERSITIES

First strike over pay and conditions

Melbourne

Australian university and college staff last week held their first one-day strike in a campaign to protest against poor career structure and to demand wage rises. The 4,500 academic staff who went on strike warned that unless their wage claims were met by 1 November, they would take further action including a refusal to release examination results or enrol students.

The three unions involved, the Federation of Australian University Staff Associations (FAUSA), the Australian Teachers Union and the Union of Australian College Academics, representing 35 colleges and universities, want average wage increases of 17 per cent in order to makes salaries competitive with those in private industry and at universities elsewhere. The increase would also bring academic salaries into line with the increased rates paid to scientists at the Commonwealth Scientific and Industrial Research Organization (CSIRO).

According to Di Zetlin, general secretary of FAUSA, wage increases are necessary to attract the academics needed to replace the large number who will retire in the next decade. "With an expected shortfall of 10,000 academics by the year 2000, the bottom line is that we must create more attractive salaries and career structures. Most of those retiring will be senior academics."

The unions are seeking a common salary scales and career structures for all academics in Australian tertiary institutions, an increase in tenured employment in universities, and a removal of barriers to promotion within the different salary ranges.

In October last year, the federal cabinet agreed to support an average award increase for academics of 6.7 per cent. But CSIRO scientists, traditionally receiving salaries equivalent to those given to academics, obtained increases of between 5 per cent and 10 per cent above the cabinet allocation.

According to Simon Marginson, research officer for FAUSA, career structures also need to be reviewed. Junior academics are hardest hit, with fewer than one in ten in permanent employment, and many working on a casual basis without benefits. FAUSA is seeking increases in the number of tenured positions open to junior academics.

The Australian Higher Education Industrial Association (AHEIA), representing the employers, admits that there is both a case for a "major upgrading in salaries", and for a "greater proportion of staff having tenure at junior levels", according to Professor David Penington, president of AHEIA.

The biggest obstacle to negotiations between the unions and employers now seems to be the employers' demand that salary rises be linked to performance. In the present system, academics are assessed only when applying for promotion to a higher grade. Movement and salary increase within each grade, however, is automatic. The universities are trying to introduce a "soft bar" which can prevent a person progressing within a group if his or her work is not up to standard. Marginson sees this "soft bar" as "managerial interference and a way to delay the promotion of academics". Penington, however, points out that "all CSIRO's officers are continually assessed at every level. If the unions want us to follow the CSIRO system, then they must be prepared to undergo performance appraisal". Tania Ewing

ASTRONOMY

Another pulsar bites the dust

São Paulo & Washington

The source of periodic brightness variations that led a team of Brazilian astronomers to announce that they had at last found a pulsar in the remnant of supernova 1987A was traced, a few days after the claim was made, to mechanical vibrations in the telescope. This is the second spurious 'pulsar' to have been found in SN1987A; the first, pulsing at a barely credible frequency of 2,000Hz (*Nature* **338**, 234; 1989) was eventually ascribed to stray emissions from nearby electronics. The latest non-pulsar was more short-lived, but the rumours were substantial enough to be noted in these pages (see *Nature* **347**, 511; 1990).

The Brazilian team, led by J. E. Steiner of the University of São Paulo, asked the Center for Astrophysics (CfA) in Cambridge, Massachusetts, to transmit an International Astronomical Union (IAU) circular to alert the astronomical community of their discovery. But according to Daniel Green of the CfA, the details were not wholly convincing, and a request for more information was sent to Steiner; he responded by asking for the IAU circular not to be sent, as unspecified problems had turned up.

Although the Brazilians, who made their observations on a 1.6-metre University of São Paulo telescope, checked their instrumentation by ascertaining that stars near the supernova remnant showed no pulsation, they later realized that their photometric detector was being confused by light from the nebula around the supernova site, and sent into some kind of instability. More negative evidence came from the European Southern Observatory (ESO) in Chile, where H. Ögelmann and C. Gouisses were also looking for a pulsar. On the night of 28 September, when Steiner's team believed it had seen something, they found no optical pulsations.

John Danziger, an astronomer at ESO headquarters in Munich, says there is solid evidence for a source of energy in SN1987A in addition to the still-decaying radioactive debris of the explosion: its luminosity has been falling less rapidly than the simple exponential curve that radioactivity alone would produce. Energy coming from material falling onto a compact central object such as a neutron star can reasonably explain the observations, but the case that this is indeed a pulsar is only inferential. The excess brightness is entirely at infrared wavelengths, suggesting that energy from within is being reprocessed by the surrounding cocoon of dust, concealing the source from direct observation.

David Lindley