The genesis of unified gauge theories

by Tom Kibble

From 8-12 March, at a 'Salamfest' at the International Centre for Theoretical Physics in Trieste, Italy, friends, colleagues, admirers and former students paid tribute to the Institute's founder and director Abdus Salam, and his contributions to science. Presentations centred around the development of today's Standard Model of particle physics and attempts to go beyond it, and the parallels between the physics of condensed matter and elementary particles.

During the week, Salam was awarded the honorary degree of Doctor of Science by the Rector of St. Petersburg University, Academician S. Merkuriev.

While in recent years Salam has mainly been identified with the Centre which he established in 1964, many of his important contributions to physics came when he resided permanently at London's Imperial College. At the Trieste Salamfest , Tom Kibble, formerly Head of Physics at Imperial and a longtime colleague of Salam, described Salam's role at Imperial in the quest for a unification of electromagnetism and the weak force. In 1979 Salam, Sheldon Glashow and Steven Weinberg shared the Nobel Physics Prize for the new synthesis, one of the major achievements of 20th-century physics.

(This is an abridged version of the talk, the complete version being available on request from Tom Kibble at the Blackett Laboratory, Imperial College, London, SW7).

The theoretical physics group at London's Imperial College in 1959 had three permanent faculty: Abdus Salam, his erstwhile thesis supervisor Paul Matthews, and John C.Taylor. I joined as a lecturer the following year.

In those early days we had lots of visitors, both long- and short-term - Murray Gell-Mann, Ken Johnson, John Ward, Lowell Brown, Gordon Feldman and Steven Weinberg.

About a year after I arrived we were transferred from the Mathematics to the Physics Department under the formidable Patrick (P.M.S.) Blackett. Having been brought up in the Cavendish Laboratory tradition under Lord Rutherford, Blackett was rather scornful of theoretical physicists, but he knew a good thing when he saw one and had persuaded Salam to join the rapidly expanding Physics Department.

In 1960 field theory was widely regarded as very *passé*. It had had its triumphs: renormalization theory had made sense of divergences, and quantum electrodynamics had been magnificently vindicated.

But field theory didn't seem to work for anything else, particularly not for the strong interactions, and was definitely out of fashion. There were, however, a few places in the world where field theory was still studied unashamedly. Imperial College was one. Harvard was certainly another; many of our visitors over the next few years were Julian Schwinger's students.

At Imperial there were two dominant theory themes: symmetries and gauge theories. Both had their origins in the concept of isospin.

The isospin symmetry between protons and neutrons had shown how two apparently disparate particles might be regarded as different states of a single fundamental entity, the nucleon. The symmetry was generalized to include Yukawa's mesons in an important paper by Nick Kemmer in 1938, which is incidentally perhaps one of the first papers to suggest the need for a neutral current.

Abdus Salam in Munich, 1947.

Kemmer was very influential in British theoretical physics in the immediate post-war period. He was Paul Matthews' supervisor in Cambridge and when I was a student in Edinburgh he was my Head of Department, having succeeded Max Born in 1953.

In the forties and fifties, as new particles proliferated, it was natural to try to bring some order into this chaos by enlarging the symmetry group beyond the SU(2) of isospin, especially after the discovery of the new quantum number, strangeness.

Salam had students working on every conceivable symmetry group. One of those students was Yuval Ne'eman, who had the good fortune and/or prescience to work on SU(3). From that work, and of course from the independent work of Murray Gell-Mann, stemmed the Eightfold Way, with its triumphant vindication in the discovery of the omega-minus in 1964.

Salam himself made many important contributions to these symmetries, but I believe this was not his first love. His real goal was to find the ultimate theory to describe the weak, electromagnetic and strong interactions, and even gravity - what we would now call a Theory of Everything.

From an early stage, certainly well before I joined Imperial, Salam was convinced that the ultimate theory would be a gauge theory.

The starting point was the epoch-making paper of Yang and Mills in 1954. There may be others who deserve some of the credit - Weyl, Klein, Shaw, Utiyama - but Yang and Mills articulated very clearly the 'gauge principle' - sometimes paraphrased as 'Nature abhors a rigid symmetry'.

Yang and Mills argued that a rigid, global isospin symmetry is incompatible with relativistic field theory. Their point was that once isospin symmetry has been accepted, it is arbitrary which component is identified with the proton and which with the neutron. But it then seems odd that making this choice should automatically fix the convention throughout all space for ever. So they looked at what needed to be done to make isospin a local symmetry.

The gauge principle provided a natural basis for electromagnetic interactions, and after the work of Yang and Mills people began to look for gauge theories of the strong and weak interactions.

The first goal was strong interactions; that is what Yang and Mills themselves were after. But it was hard to make progress because calculations were difficult. With such a strong coupling, perturbation theory would not work, and the asymptotic freedom of quarks was unknown.

The International Centre for Theoretical Physics, Trieste, Italy was founded by Abdus Salam in 1964.

So the weak interactions emerged as a better bet. There were certainly tantalizing hints of a structure very similar to electrodynamics. While Fermi's classic recipe with four particles interacting at a point was obviously non-renormalizable, it was probably a shorthand way of writing an effective interaction due to the exchange of a heavy boson.

Progress was held up while people searched for the correct space-time symmetry of the weak interaction. The breakthrough came with another suggestion of Yang's, working this time with T.D. Lee, that mirror symmetry (parity) is not conserved in weak interactions. After the fall of parity in 1957, Salam was one of the first to point out the connection between left-handedness and a zero mass neutrino.

Meanwhile Marshak and Sudarshan and Feynman and Gell-Mann showed how the weak interaction should be written down. This suggested that weak interactions could be mediated by a charged vector boson, the W.

The seemingly insuperable difficulty was the large W mass. If the interaction were of the same strength as electromagnetism, the W mass would have to be about 40 GeV. But putting a mass term in the Lagrangian would destroy the gauge invariance, and the heavy vector particle would make the formalism blow up and become unrenormalizable.

As early as 1958, Salam and John Ward proposed a unified gauge theory of weak and electromagnetic interactions, involving a charge triplet of vector mesons, with the neutral component identified with the photon. They placed the electron, neutrino and positron too in a triplet. This was ingenious, but of course they could only obtain the parity-conserving part of the weak interaction. Parity violation was artificially imposed, and the W mass put in by hand.

Two years later they proposed a unified theory of weak, electromagnetic and strong interactions, based on the gauge group SO(8), a paper well ahead of its time, foreshadowing later ideas of grand unification.

But these theories did not really work; nor did similar ones proposed by Glashow and others. The major obstacle remained the vector meson mass. This was essential to make the interaction weak and short-range, but apparently incompatible with both gauge invariance and renormalizability. The only way anyone knew to make a vector-meson theory renormalizable was to use zero-mass gauge bosons.

As often happens, progress was delayed by a 'folk theorem'. Theoretical physicists sometimes quote 'theorems' that everyone believes but eventually turn out not to be true.

One such folk theorem was that the photon is massless because of gauge invariance, considered one of the predictive successes of the gauge principle. In 1961 Julian Schwinger said this theorem might be false, although he was thinking more about strong interactions at the time.

Another folk theorem came in when people began edging towards spontaneous symmetry breaking to explain the heavy gauge mesons. Here the Goldstone theorem apparently predicted unobserved massless spin-zero particles.

When Steven Weinberg came to Imperial College in 1961-62, he and Salam, collaborating at long range with Jeffrey Goldstone, spent a lot of time confirming this theorem. In condensed-matter physics, counterexamples to the Goldstone theorem were known for long-range forces. But the theorem seemed to rule out this mechanism for relativistic theories.

Abdus Salam at Stockholm in 1979 - the first Pakistani to receive the Nobel Award.

An important 1963 paper by Phil Anderson showed how Schwinger's suggestion of a heavy gauge field could work. One example was the plasmon: in a high-density plasma the photon acquires a non-zero 'mass' - the plasma frequency. But Anderson also pointed out, using the example of superconductivity, how Goldstone bosons could 'become tangled up with Yang-Mills gauge bosons and, thus, do not in any true sense have zero mass'. He concluded 'the Goldstone zero-mass difficulty is not a serious one, because we can probably cancel it off against an equal Yang-Mills zero-mass problem'. This is exactly what is now known as the Higgs mechanism.

This should have cleared everything up, but these new ideas were difficult to understand. By the time Gerry Guralnik and Dick Hagen, both at Imperial that year, and I had also realized the Goldstone theorem doesn't apply to gauge symmetries, others were there too. The result was published independently in 1964 by Englert and Brout and by Peter Higgs.

So by 1963-64 the problem of the origin of mass was solved, at least in principle. But there was still another major hurdle, to unify weak interactions, which are parity-violating, with electromagnetism, which is not. It took another three years to realize that for the photon to coexist with the parity violation of weak interactions, the gauge group had to be extended from SU(2) to SU(2)xU(1), with two neutral particles rather than one.

Actually the solution, or something very like it, was already there in Sheldon Glashow 's 1961 paper which had proposed SU(2)xU(1) with mixing between the neutral particles, but this was before the key concepts of spontaneous symmetry breaking and the Higgs mechanism had been developed.

At Imperial, Salam kept plugging away at the problem, especially in collaboration with John Ward. In autumn 1967, Salam gave a series of lectures at Imperial in which he described the SU(2)xU(1) theory. Meanwhile the same model had been found independently by Steven Weinberg.

When Weinberg's paper appeared I was at Rochester, where Bob Marshak asked me to give a talk to his weekly discussion group.

I mentioned that Salam and Ward had been working on very similar ideas, and focused on the problems in constructing a unified theory of weak and electromagnetic interactions and how ingeniously the new model avoided them. However I described it as a wonderful toy - without any connection to the real world!

While I was myopic, in a sense I was right. The whole thing seemed much too ad hoc and ugly, with its curious built-in asymmetry between the left- and right-handed fermions and its large number of independent parameters. If it is part of the final theory, it is ugly; surely the Creator was having an off day! But that is not the right way to look at it. Seen merely as one step towards a still undiscovered final theory, the intricate way the electroweak picture fits together does have a remarkable beauty.

It is sad that Paul Matthews, who died tragically six years ago, could not have given this tribute. For many years, Imperial was Salam and Matthews. They made a superb team, exactly complementing each other's strengths and abilities.

Paul Matthews (1919-1987). Abdus Salam's first mentor.

First crumb of research

In 1964, Abdus Salam introduced Paul Matthews' inaugural lecture at London's Imperial College. It was a poignant moment. Salam, who had taken his first steps in theoretical physics at Cambridge under Matthews' watchful eye, had become Imperial's first professor in Theoretical Physics. Now he was overseeing the promotion of his former supervisor.

Salam recalled his 1949 research debut at Cambridge, where, because of impressive examination results, he had initially been directed towards the laboratory.

'Soon, I knew the craft of experimental physics was beyond me,' wrote Salam later. 'It was the sublime quality of patience which I lacked.' Looking towards theory, he had gone to Nicholas Kemmer (in the front row at Matthews' inaugural). Kemmer had said he had enough students already and did not want another. Salam had pleaded, and fortunately Kemmer had relented.

'All theoretical problems in quantum electrodynamics have been solved by Schwinger, Feynman and Dyson,' Kemmer had told Salam. 'Paul Matthews has applied their methods to meson theories. He is finishing his PhD. Ask him.'

At Imperial in 1964, Salam recalled that first meeting with Matthews in 1950.

'What are you reading?' Matthews had asked.

'Heitler's Quantum Theory of Radiation,' had come the reply. It was the only standard text at the time.

Matthews quickly recommended instead the new work by Schwinger, Feynman and Dyson, then known only to a privileged few.

Later, his PhD complete, Matthews left a research 'crumb', as Salam put it. The agreement was that Salam would look at a continuing problem in meson field renormalization while Matthews took a few months off before starting work at Princeton in the fall. If Salam had made no progress, Matthews would repossess the problem.

Characteristically, Salam's first act as a research student was to phone Freeman Dyson (his 'hero'), then visiting Birmingham, and ask for an interview. The discussion continued on the train to Southampton, where Dyson was to embark for the US. The seeds of the solution were sown and soon the 'crumb' problem was solved. It was the start of a meteoric career.