

# Weinberg, Salam and Glashow on physics

**\* In our next issue we will cover the award of the 1980 Nobel Physics Prize to J.W. Cronin and V.L. Fitch.**

Last year the Nobel Prize for physics was awarded to Steven Weinberg, Abdus Salam and Sheldon Glashow for the development of the theory which unifies electromagnetic and weak interactions\*. The three recipients each presented a lecture on the occasion of the presentation of the prize, and these lectures have been published in full (together with footnotes, appendices and references) in the July edition of *Reviews of Modern Physics* (Vol. 52 No. 3). The lectures provide an illuminating insight into the challenges and rewards of modern physical theory and as such are well worth reading in their entirety. In addition, extracts illustrate several aspects of today's physics and physicists.

## *The objectives of physics and physicists*

Weinberg:

'Our job in physics is to see things simply, to understand a great many complicated phenomena in a unified way, in terms of a few simple principles. At times, our efforts are illuminated by a brilliant experiment, such as the 1973 discovery of neutral current neutrino reactions. But even in the dark times between experimental breakthroughs, there always continues a steady evolution of theoretical ideas, leading almost imperceptibly to changes in previous beliefs.'

Salam quotes Feynman from an interview in the magazine 'Omni':

'As long as it looks like the way things are built [is] with wheels within wheels, then you are looking for the innermost wheel — but it might not be that way, in which case you are looking for whatever the hell it is you find!' In the same interview he remarks 'a few years ago I was very sceptical about the gauge theories... I was expecting mist, and now

it looks like ridges and valleys after all.'

Salam also cites Einstein:

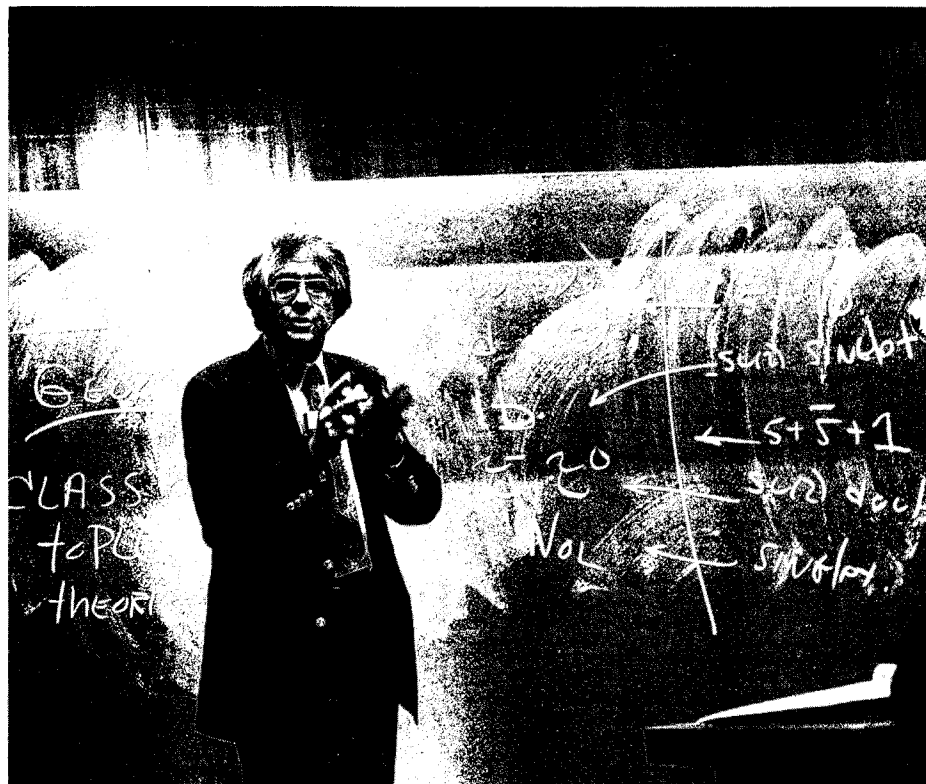
'There is the apocryphal story about Einstein, who was asked what he would have thought if experiment had not confirmed the light deflection predicted by him. Einstein is supposed to have said, "Madam, I would have thought the Lord has missed a most marvellous opportunity." I believe, however, that the following quote from Einstein's Herbert Spencer lecture of 1933 expresses his, my colleagues', and my own views more accurately. "Pure logical thinking cannot yield us any knowledge of the empirical world; all knowledge of reality starts from experience and ends in it."

Glashow takes a more personal viewpoint:

'In 1956, when I began doing theoretical physics, the study of elementary particles was like a patch-

*Sheldon Glashow: making a patchwork quilt of theory into a tapestry. Here he is seen explaining how the long-awaited top quark may not really be necessary.*

*(Photo CERN 310.12.79)*



work quilt. Electrodynamics, weak interactions, and strong interactions were clearly separate disciplines, separately taught and separately studied. There was no coherent theory that described them all. Developments such as the observation of parity violation, the successes of quantum electrodynamics, the discovery of hadron resonances and the appearance of strangeness were well-defined parts of the picture, but they could not be easily fitted together.

Things have changed. Today we have what has been called a "standard theory" of elementary particle physics in which strong, weak, and electromagnetic interactions all arise from a local symmetry principle. It is, in a sense, a complete and apparently correct theory, offering a qualitative description of all particle phenomena and precise quantitative predictions in many instances. There

*Abdus Salam: after a brief foray into experimental physics as a student, he soon turned his attention to theory. A wise move, it would appear.*

*(Photo CERN 371.10.79)*



are no experimental data that contradict the theory. In principle, if not yet in practice, all experimental data can be expressed in terms of a small number of "fundamental" masses and coupling constants. The theory we now have is an integral work of art: the patchwork quilt has become a tapestry.

Tapestries are made by many artisans working together. The contributions of separate workers cannot be discerned in the completed work, and the loose and false threads have been covered over. So it is in our picture of particle physics.'

Salam, the senior of the three prizewinners, describes his early years in physics research:

'I started physics research thirty years ago as an experimental physicist in the Cavendish, experimenting with tritium-deuterium scattering. Soon I knew the craft of experimental physics was beyond me — it was

the sublime quality of patience — patience in accumulating data, patience with recalcitrant equipment — which I sadly lacked. Reluctantly I turned my papers in, and started instead on quantum field theory with Nicholas Kemmer in the exciting department of P.A.M. Dirac.'

#### *The search for electroweak unification*

Glashow toyed with the idea of electroweak unification at an early stage in his physics career:

'Schwinger, as early as 1956, believed that the weak and electromagnetic interactions should be combined into a gauge theory. The charged massive vector intermediary and the massless photon were to be the gauge mesons. As his student, I accepted this faith. In my 1958 Harvard thesis, I wrote: "It is of little value to have a potentially

renormalizable theory of beta processes without the possibility of a renormalizable electrodynamics. We should care to suggest that a fully acceptable theory of these interactions may only be achieved if they are treated together..." We used the original SU(2) gauge interaction of Yang and Mills. Things had to be arranged so that the charged current, but not the neutral (electromagnetic) current, would violate parity and strangeness. Such a theory is technically possible to construct, but it is both ugly and experimentally false. We know now that neutral currents do exist and that the electroweak gauge group must be larger than SU(2).'

Weinberg talks of his 'love affair' with broken symmetry:

'Sometime in 1960 or early 1961, I learned of an idea which had originated earlier in solid state physics and had been brought into particle physics by those like Heisenberg, Nambu, and Goldstone, who had worked in both areas. It was the idea of "broken symmetry," that a quantum theory could possess an exact symmetry, and that the physical states might nevertheless not provide neat representations of the symmetry. In particular, a symmetry of the theory might turn out to be not a symmetry of the vacuum.'

As theorists sometimes do, I fell in love with this idea. But as often happens with love affairs, at first I was rather confused about its implications. I thought (as it turned out, wrongly) that the approximate symmetries — parity, isospin, strangeness, the eightfold way — might really be exact symmetry principles, and that the observed violations of these symmetries might somehow be brought about by spontaneous symmetry breaking. It was therefore rather disturbing for me to hear of a result of Goldstone, that in at least

Steven Weinberg: a 'love affair' with broken symmetry.

(Photo CERN 349.12.79)



one simple case the spontaneous breakdown of a continuous symmetry like isospin would necessarily entail the existence of a massless spin zero particle — what would today be called a "Goldstone boson." It seemed obvious that there could not exist any new type of massless particle of this sort which would not already have been discovered.

I had long discussions of this problem with Goldstone at Madison in the summer of 1961, and then with Salam while I was his guest at Imperial College in 1961–62. The three of us soon were able to show that Goldstone bosons must in fact occur whenever a symmetry like isospin or strangeness is spontaneously broken, and that their masses then remain zero to all orders of perturbation theory. I remember being so discouraged by these zero masses that when we

wrote our joint paper on the subject, I added an epigraph to the paper to underscore the futility of supposing that anything could be explained in terms of a noninvariant vacuum state: it was Lear's retort to Cordelia, "Nothing will come of nothing: speak again." Of course, *The Physical Review* protected the purity of the physics literature, and removed the quote. Considering the future of the noninvariant vacuum in theoretical physics it was just as well.

Later, Weinberg describes how he fell on the idea of electroweak unification:

'At some point in the fall of 1967, I think while driving to my office at MIT, it occurred to me that I had been applying the right ideas to the wrong problem. It is not the rho meson that is massless: it is the photon. And its partner is not the A1, but the massive intermediate bosons, which since the time of Yukawa

had been suspected to be the mediators of the weak interactions. The weak and electromagnetic interactions could then be described in a unified way in terms of an exact but spontaneously broken gauge symmetry. And this theory would be renormalizable like quantum electrodynamics because it is gauge invariant like quantum electrodynamics. It was not difficult to develop a concrete model which embodied these ideas. I had little confidence then in my understanding of strong interactions, so I decided to concentrate on leptons.'

Salam's account is more like a saga:

'For me, personally, the trek to gauge theories as candidates for fundamental physical theories started in earnest in September 1956 — the year I heard, at the Seattle Conference, Yang expound his and Lee's ideas on the possibility of the hitherto sacred principle of left-right symmetry being violated in the realm of the weak nuclear force. Lee and Yang had been led to consider abandoning left-right symmetry for weak nuclear interactions as a possible resolution of the kaon decay puzzle.

I remember travelling back to London on an American Air Force transport flight. Although I had been granted, for that night, the status of a Brigadier or a Field Marshal — I don't quite remember which — the plane was very uncomfortable, full of crying servicemen's children — that is, the children were crying, not the servicemen. I could not sleep. I kept reflecting on why Nature should violate left-right symmetry in weak interactions.

Now the hallmark of most weak interactions was Pauli's neutrino. While crossing over the Atlantic, a deeply perceptive question about the neutrino came back to me which

*Wolfgang Pauli, the father of the neutrino. His correspondence with Salam during the gestation of the new ideas on weak interactions makes interesting reading. One typical remark: if a theoretician says universal, it just means pure nonsense.*

Rudolf Peierls had asked when he was examining me for a Ph.D. a few years before. Peierls' question was: "The photon mass is zero because of Maxwell's principle of a gauge symmetry for electromagnetism; tell me, why is the neutrino mass zero?" I had then felt somewhat uncomfortable at Peierls, asking for a Ph.D. viva,

question of which he himself said he did not know the answer. But during that comfortless night the answer came. The analogue for the neutrino of the gauge symmetry for the photon existed: it had to do with the masslessness of the neutrino, with symmetry under a particular transformation later christened "chiral symmetry". The existence of this symmetry for the massless neutrino must imply one of two possibilities for the neutrino interactions. Nature had the choice of an aesthetically satisfying but a left-right symmetry-violating theory, with a neutrino which travels exactly with the velocity of light; or alternatively a theory where left-right symmetry is preserved, but the neutrino has a tiny mass — some ten thousand times smaller than the mass of the electron.

It appeared at that time clear to me what choice Nature must have made. Surely, left-right symmetry must be sacrificed in all neutrino interactions. I got off the plane the next morning, naturally very elated. I rushed to the Cavendish, worked out the Michel parameter and a few other consequences of the symmetry, rushed out again, got onto a train to Birmingham where Peierls lived. To Peierls I presented my idea: he had asked the original question; could he approve of the answer? Peierls' reply was kind but firm. He said "I do not believe left-right symmetry is violated in weak nuclear forces at all."

Thus rebuffed in Birmingham, like



Zuleika Dobson, I wondered where I could go next and the obvious place was CERN in Geneva, with Pauli — the father of the neutrino — nearby in Zurich. At that time CERN lived in a wooden hut just outside Geneva airport. Besides my friends, Prentki and d'Espagnat, the hut contained a gas ring on which was cooked the staple diet of CERN — Entrecôte à la crème. The hut also contained Villars from MIT, who was visiting Pauli the same day in Zurich. I gave him my paper. He returned the next day with a message from the Oracle: "Give my regards to my friend Salam and tell him to think of something better."

This was discouraging, but I was compensated by Pauli's excessive kindness a few months later, when Mrs. Wu's, Lederman's and Telegdi's experiments were announced showing that left-right symmetry was indeed violated, and

ideas similar to mine about chiral symmetry were expressed independently by Landau and by Lee and Yang.

I received Pauli's first, somewhat apologetic letter on 24 January 1957. Thinking that Pauli's spirit should by now be suitably crushed, I sent him two short notes I had written in the meantime. These contained suggestions to extend chiral symmetry to electrons and muons, assuming that their masses were a consequence of what has come to be known as dynamical spontaneous symmetry breaking. With chiral symmetry for electrons, muons, and neutrinos, the only mesons that could mediate weak decays of the muons would have to carry spin one. Reviving thus the notion of charged intermediate spin-one bosons, one could then postulate for these a type of gauge invariance which I called the "neutrino gauge."

Pauli's reaction was swift and terrible. He wrote on 30 January 1957, then on 18 February and later on 11, 12, and 13 March: "I am reading (along the shores of Lake Zurich) in bright sunshine quietly your paper... I am very much startled on the title of your paper 'Universal Fermi Interaction'... For quite a while I have for myself the rule if a theoretician says universal it just means pure nonsense. This holds particularly in connection with the Fermi interaction, but otherwise too, and now you too, Brutus, my son, come with this word..." Earlier, on 30 January, he had written "There is a similarity between this type of gauge invariance and that which was published by Yang and Mills..." I quote from his letter: "However, there are dark points in your paper regarding the vector field. If the rest mass is infinite (or very large), how can this be compatible with the gauge transformation?" and he concluded his letter with the remark: "Every reader will realize that you deliberately conceal here something and will ask you the same questions." Although he signed himself "With friendly regards," Pauli had forgotten his earlier penitence. He was clearly and rightly on the warpath. Now the fact that I was using gauge ideas similar to the Yang-Mills gauge theory was no news to me. This was because the Yang-Mills theory (which married gauge ideas of Maxwell with the internal symmetry SU(2) of which the proton-neutron system constituted a doublet) had been independently invented by a Ph.D. pupil of mine, Ronald Shaw, at Cambridge at the same time as Yang and Mills had written. Shaw's work is relatively unknown; it remains buried in his Cambridge thesis.

I must admit I was taken aback by Pauli's fierce prejudice against universalism — against what we would

today call unification of basic forces — but I did not take this too seriously. I felt this was a legacy of the exasperation which Pauli had always felt at Einstein's somewhat formalistic attempts at unifying gravity with electromagnetism — forces which in Pauli's phrase "cannot be joined — for God hath rent them asunder." But Pauli was absolutely right in accusing me of darkness about the problem of the masses of the Yang-Mills fields; one could not obtain a mass without wantonly destroying the gauge symmetry one had started with. The problem was to be solved only seven years later with the understanding of what is now known as the Higgs mechanism.'

#### *Renormalization*

With the first formulations of the electroweak theory, the spanner in the works was their renormalization — there appeared to be no neat way of avoiding troublesome infinities in the calculations.

Weinberg:

'The next question now was renormalizability. The Feynman rules for Yang-Mills theories with unbroken gauge symmetries had been worked out by deWitt, Faddeev, and Popov and others, and it was known that such theories are renormalizable. But in 1967 I did not know how to prove that this renormalizability was not spoiled by the spontaneous symmetry breaking. I worked on the problem on and off for several years, partly in collaboration with students, but I made little progress. With hindsight, my main difficulty was that I adopted a gauge now known as the unitarity gauge: this gauge has several wonderful advantages, it exhibits the true particle spectrum of the theory, but it has the disadvantage of making renormalizability totally obscure.

Finally, in 1971 't Hooft showed in a beautiful paper how the problem could be solved: The proof was subsequently completed by Lee and Zinn-Justin and by 't Hooft and Veltman.

I have to admit that when I first saw 't Hooft's paper in 1971, I was not convinced that he had found the way to prove renormalizability. The trouble was not with 't Hooft, but with me: I was simply not familiar enough with the formalism on which 't Hooft's work was based.'

Salam:

'Both Weinberg and I suspected that this theory was likely to be renormalizable. Regarding spontaneously broken Yang-Mills-Shaw theories in general this had earlier been suggested by Englert, Brout, and Thiry. But this subject was not pursued seriously except at Veltman's school at Utrecht, where the proof of renormalizability was given by 't Hooft in 1971. This was elaborated further by that remarkable physicist, the late Benjamin Lee, working with Zinn-Justin, and by 't Hooft and Veltman. In Coleman's eloquent phrase " 't Hooft's work turned the Weinberg-Salam frog into an enchanted prince."

Glashow:

'Our labours were in vain. In the spring of 1971, Veltman informed us that his student Gerhart 't Hooft had established the renormalizability of spontaneously broken gauge theory. In pursuit of renormalizability, I had worked diligently but I completely missed the boat.'

#### *The neutral current*

Electroweak unification implied the existence of a neutral current, enabling weak interactions to happen without altering the electric charges of the participating particles. However the neutral current

*The heavy liquid bubble chamber Gargamelle in position at the CERN 28 GeV proton synchrotron where in 1973 it was the scene of the discovery of the neutral current (below). According to Weinberg, the electroweak theory predicted rates for the neutral current which were low enough to have escaped earlier detection, so there was every reason to look harder.*

*(Photo CERN 143.4.71)*

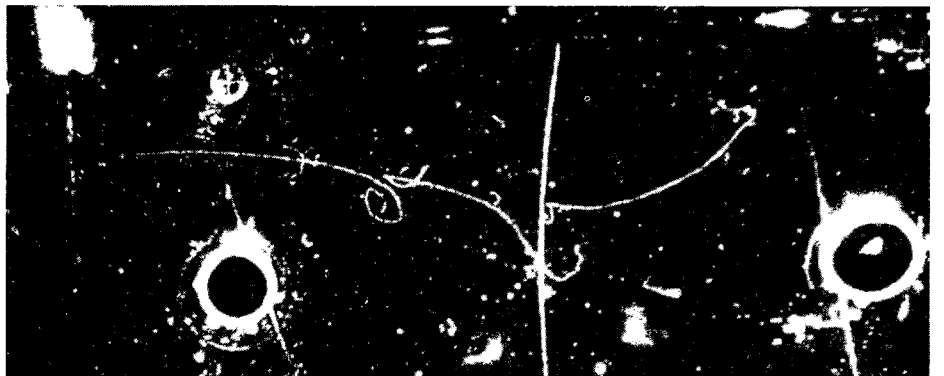
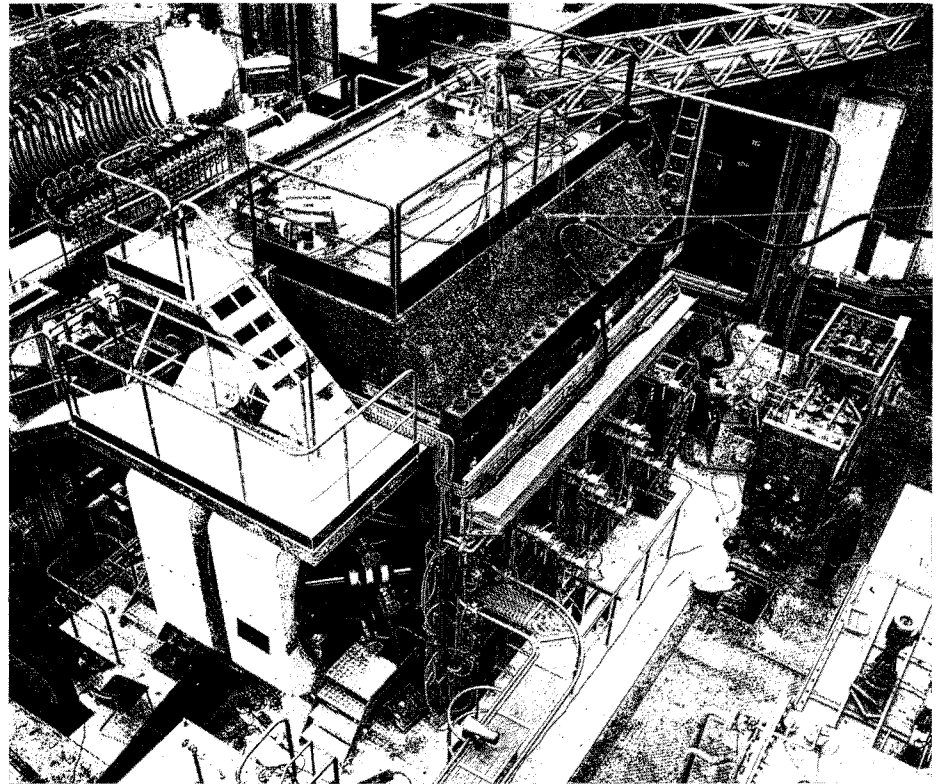
was not discovered until the experiments at CERN with the Gargamelle bubble chamber in 1973, six years after the publication of the electroweak ideas.

Weinberg describes how he did not give up hope:

'Of course, the possibility of neutral currents was nothing new. There had been speculations about possible neutral currents as far back as 1937 by Gamow and Teller, Kemmer, and Wentzel, and again in 1958 by Bludman and Leite-Lopes. Attempts at a unified weak and electromagnetic theory had been made by Glashow, and Salam and Ward in the early 1960s, and these had neutral currents with many of the features that Salam and I encountered in developing the 1967-68 theory. But since one of the predictions of our theory was a value for the mass of the intermediate boson, it made a definite prediction of the strength of the neutral currents.

More important, now we had a comprehensive quantum field theory of the weak and electromagnetic interactions that was physically and mathematically satisfactory in the same sense as quantum electrodynamics — a theory that treated photons and intermediate vector bosons on the same footing, that was based on an exact symmetry principle, and that allowed one to carry calculations to any desired degree of accuracy. To test this theory, it had now become urgent to settle the question of the existence of the neutral currents.

Late in 1971, I carried out a study of the experimental possibilities. The results were striking. Previous experiments had set upper bounds on the rates of neutral current processes which were rather low, and many people had received the impression that neutral currents were pretty well ruled out, but I



found that in fact the 1967-68 theory predicted quite low rates, low enough in fact to have escaped clear detection up to that time. So there was every reason to look a little harder.'

Glashow's encounter with the problem of the neutral current was somewhat more lengthy:

'When I came upon the model in 1960, I had speculated on a possible extension to include hadrons. To

construct a model of leptons alone seemed senseless: nuclear beta decay, after all, was the first and foremost problem. One thing seemed clear. The fact that the charged current violated strangeness would force the neutral current to violate strangeness as well. It was already well known that strangeness-changing neutral currents were either strongly suppressed or absent. I concluded that the interme-

*Abdus Salam celebrates the award of his Nobel Prize with members of the Gargamelle team. Salam learnt of the neutral current discovery from Paul Musset (on Salam's right in the photo) in the street in Aix-en-Provence, on his way to the 1973 European Conference on High Energy Physics.*

*(Photo CERN 392.10.79)*



diate boson of neutral currents had to be made very much heavier than its charged current counterparts. This was an arbitrary but permissible act in those days: the symmetry breaking mechanism was unknown. I had "solved" the problem of strangeness-changing neutral currents by suppressing all neutral currents: the baby was lost with the bath water.

I returned briefly to the question of gauge theories of weak interactions in a collaboration with Gell-Mann in 1961. We showed that a gauge theory of weak interactions would inevitably run into the problem of strangeness-changing neutral currents. We concluded that something essential was missing. Indeed it was. Only after quarks were invented could the idea of the fourth quark and the GIM (Glashow-Iliopoulos-Maiani) mechanism arise.

From 1961 to 1964, Sidney Cole-

man and I devoted ourselves to the exploitation of the unitary symmetry scheme. In the spring of 1964, I spent a short leave of absence in Copenhagen. There, Bjorken and I suggested that the Gell-Mann-Zweig system of three quarks should be extended to four. We called the fourth quark the charmed quark. Part of our motivation for introducing a fourth quark was based on our mistaken notions of hadron spectroscopy. But we also wished to enforce an analogy between the weak leptonic current and the weak hadronic current. Because there were two weak doublets of leptons, we believed there had to be two weak doublets of quarks as well.

The weak current Bjorken and I introduced in 1964 was precisely the GIM current. The associated neutral current, as we noted, conserved strangeness. Had we inserted these currents into the earlier

electroweak theory, we would have solved the problem of strangeness-changing neutral currents. We did not. I had apparently quite forgotten my earlier ideas of electroweak synthesis. The problem which was explicitly posed in 1961 was solved, in principle, in 1964. No one, least of all me, knew it. Perhaps we were all befuddled by the chimera of relativistic  $SU(6)$ , which arose at about this time to cloud the minds of theorists.

Five years later, John Iliopoulos, Luciano Maiani and I returned to the question of strangeness-changing neutral currents. It seems incredible that the problem was totally ignored for so long. We argued that unobserved effects would be expected to arise in any of the known weak interaction models and showed how the unwanted effects would be eliminated with the conjectured existence of a fourth quark. After lan-

guishing for a decade, the problem of the selection rules of the neutral current was finally solved. Of course, not everyone believed in the predicted existence of charmed hadrons.

This work was done fully three years after the epochal work of Weinberg and Salam and was presented in seminars at Harvard and at M.I.T. Neither I, nor my co-workers, nor Weinberg, sensed the connection between the two endeavours. We did not refer, nor were we asked to refer, to the Weinberg-Salam work in our paper.

The relevance became evident only a year later. Due to the work of 't Hooft, Veltman, Benjamin Lee, and Zinn-Justin, it became clear that the Weinberg-Salam ansatz was in fact a renormalizable theory. With GIM, it was trivially extended from a model of leptons to a theory of weak interactions. The ball was now squarely in the hands of the experimenters.'

Especially amusing is Salam's story of how he first came to hear of the discovery of neutral currents:

'I still remember Paul Matthews and I getting off the train at Aix-en-Provence for the 1973 European Conference and foolishly deciding to walk with our rather heavy luggage to the student hostel where we were billeted. A car drove from behind us, stopped, and the driver leaned out. This was Musset whom I did not know well personally then. He said: "Are you Salam?" I said "Yes." He said: "Get into the car. I have news for you. We have found neutral currents." I will not say whether I was more relieved for being given a lift because of our heavy luggage or for the discovery of neutral currents. At the meeting that great and modest man, Lagarrigue, was also present and the atmosphere was that of a carnival — at least this is how it appeared to me.'

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### *The future*

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Weinberg is in poetic mood:

'I suppose that I tend to be optimistic about the future of physics. And nothing makes me more optimistic than the discovery of broken symmetries. In the seventh book of *The Republic*, Plato describes prisoners who are chained in a cave and can see only shadows that things outside cast on the cave wall. When released from the cave at first their eyes hurt, and for a while they think that the shadows they saw in the cave are more real than the objects they now see. But eventually their vision clears, and they can understand how beautiful the real world is. We are in such a cave, imprisoned by the limitations on the sorts of experiments we can do. In particular, we can study matter only at relatively low temperatures, where symmetries are likely to be spontaneously broken, so that nature does not appear very simple or unified. We have not been able to get out of this cave, but by looking long and hard at the shadows on the cave wall, we can at least make out the shapes of symmetries, which though broken, are exact principles governing all phenomena, expressions of the beauty of the world outside.'

Despite its successes, Glashow does not hold out much hope for the present form of the electroweak theory:

'Let me stress that I do not believe that the standard theory will long survive as a correct and complete picture of physics. All interactions may be gauge interactions, but surely they must lie within a unifying group. This would imply the existence of a new and very weak interaction which mediates the decay of protons. All matter is thus inherently unstable, and can be observed to decay. Such a synthesis of weak,

strong, and electromagnetic interactions has been called a "grand unified theory", but a theory is neither grand nor unified unless it includes a description of gravitational phenomena. We are still far from Einstein's truly grand design.'

Salam expresses 'amazement':

'All I can say is that I am forever and continually being amazed at the depth revealed at each successive level we explore. I would like to conclude with a prediction which J. R. Oppenheimer made more than twenty-five years ago and which has been fulfilled today in a manner he did not live to see. More than anything else, it expresses the faith for the future with which this greatest of decades in particle physics ends: "Physics will change even more... If it is radical and unfamiliar... we think that the future will be only more radical and not less, only more strange and not more familiar, and that it will have its own new insights for the inquiring human spirit."