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GENESIS OF UNIFIED GAUGE THEORIES — PERSONAL RECOLLECTIONS FROM IMPERIAL *

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ABSTRACT

I review the highlights of the historical development of unified gauge theories during the sixties from the perspective of my own recollections as a member of the Imperial College group led by Professor Abdus Salam.

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1. Introduction

I had the great good fortune to join the Imperial College Theoretical Physics Group in 1959, just two years after it was set up by Professor Abdus Salam, accompanied by his erstwhile PhD supervisor, Paul Matthews. It was a time of tremendous excitement and rapid progress, and Imperial was I believe one of the best places to be.

What I would like to do today is to recall some of the flavour of those times, to try to tell you what it was like. I want to review some of the highlights of the sixties from the perspective of my personal recollections, and to explain how the problems seemed at the time. You will not learn any new physics, but perhaps the story of our various misconceptions may serve as a cautionary tale.

Let me emphasize that this is not intended as a definitive historical account of the development of gauge theories. I am not equipped for such a task; I am not a historian of science and I have not tried to conduct a comprehensive survey of the literature. What I am aiming for is a much more personal story. I want to try to convey how it felt to be a member of that community and to participate in one of the most exciting developments in theoretical physics in our lifetimes. If in doing so I concentrate on those parts of the story with which I was most closely involved, I hope I may be forgiven for that degree of bias.

2. Imperial College in 1960

What was Imperial like around 1960? When I arrived, as Bob Delbourgo mentioned, we were still part of the Mathematics Department, occupying one corridor of the old Huxley Building which had once been part of a museum and is now again a wing of the Victoria and Albert Museum — a purpose to which it is much better suited! Our rooms were of very strange dimensions, often high and narrow, because they had been carved out of much larger areas. My office had only the top half of a window. The acoustics were such that we could hear every word of the lectures in the adjacent lecture theatre, and no doubt the students could hear everything we said.

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All the PhD students were crowded into one big office, which was also where we met for tea and coffee. There were three permanent faculty: Salam and Matthews and John C. Taylor. I joined as a lecturer the following year.

Let me say at the start that it is a great sorrow that Paul Matthews could not have been here today, because of his tragic death five years ago. For many years, Imperial *was* Salam and Matthews. They made a superb team, exactly complementing each other's strengths and abilities. Paul would have loved to have been here — and if he had been alive I am sure it would have been he rather than I who would be giving this talk.

One of the great things about our group in those early days was that we always had lots of visitors, both long- and short-term; our guests included Murray Gell-Mann, Ken Johnson, John Ward, Lowell Brown, Gordon Feldman and Steven Weinberg. Overseeing us all was Professor Salam's formidable secretary Bridget, of whom we were all terrified — all, that is, except Professor Salam! Personally, I always typed my own papers rather than face having to ask her to do it — and remember, there were no word processors then!

About a year after I arrived we moved into the Physics Department's new building. The then Head of Department was another formidable and colourful character, Patrick (or P.M.S.) Blackett. Generally speaking, Blackett, having been brought up in the Cavendish Laboratory tradition under Lord Rutherford, was rather scornful of the value of theoretical physicists, but he knew a good thing when he saw one and persuaded Salam to join the rapidly expanding Physics Department, in what is now the Blackett Laboratory. There we had much better rooms and facilities.

3. Physics at Imperial

What about Physics?

Many people have forgotten — and perhaps the younger among you never knew — that in 1960 field theory was widely regarded as very passé. Of course it had had its triumphs: renormalization theory had made sense of the old problem of ultraviolet divergences, and QED had been magnificently vindicated, with the predictions of the Lamb shift and the magnetic moment of the electron — it is still among the most impressively verified of all physical theories.

But field theory didn't seem to work for anything else, particularly not for the strong interactions. By 1960 the dominant ideology, especially in California from where I had just returned, was S-matrix theory. It was a very attractive philosophy. People always like to get something for nothing, even theoretical physicists — and it was amazing in fact how far one could go on the basis of a few very simple assumptions: relativistic invariance, unitarity, crossing symmetry and causality. The most radical version of the ideology, the self-consistent bootstrap, had as its aim to get all of strong-interaction physics from just these principles and nothing else. There were to be no elementary particles; everything was a bound state of everything else.

Of course, people did study field theory, often rather apologetically; they looked to field theory to provide a concrete model of how scattering amplitudes should behave, particularly to understand their complex analytic structure.

There were, however, a few places in the world where field theory was still studied unashamedly *qua* field theory. Imperial College was one of them. Harvard was certainly another; it is very noticeable how many of our visitors over the next few years were Julian Schwinger's students from Harvard. No doubt there were a few other places too!

Even we were not immune from the charms of S-matrix theory. Much of my early work was on dispersion relations, as indeed was much of Salam's. Salam and Matthews wrote an influential series of papers on dispersion relations for K-meson–nucleon scattering. But at Imperial it was never a major theme. There were two dominant themes: symmetries and gauge theories. Both in a way had their origins in the tremendously successful concept of isospin (then still usually called isotopic spin, or isobaric spin by purists).

In the case of symmetries that origin is fairly obvious. 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 [1] — which is incidentally perhaps one of the first papers to suggest the need for a neutral current. Kemmer was a very influential figure in British theoretical physics in the immediate post-war period. He was Paul Matthews' supervisor in Cambridge and he was my Head of Department when I was a student in Edinburgh, succeeding Max Born in the Tait Chair of Mathematical Physics 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 SU(2), especially after the discovery of the new quantum number, strangeness.

Salam had students working on every conceivable group — on SO(4), on G_2, and so on. One of those students was Yuval Ne'emann, who had the good fortune — or the 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 Ω^-, completing the spin-$\frac{3}{2}$ decuplet.

Salam himself made many important contributions then and later to unravelling these symmetries, but this was not I believe his first love. His real goal was always to find the ultimate theory that would describe the weak, electromagnetic and strong interactions, and even gravity — what we would now call a theory of everything. Discovering the symmetries provided important clues to this theory, and was therefore more of a means than an end in itself.

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

I myself caught that enthusiasm when I arrived at Imperial. I began working on gravity as a gauge theory, following in the pioneering steps of Utiyama, whose work in the fifties [2] was mentioned the other day by Lochlainn O'Raifeartaigh [3]. I showed [4] that in that approach one rather naturally arrives at a generalization of the Einstein theory, involving non-zero torsion (as suggested long before by Cartan), interacting with the spin density.

4. Yang-Mills theory

So I come to the start of my real story: the development of unified gauge theory as I saw it.

O'Raifeartaigh gave us a fascinating account of the early history, but from my point of view the first chapter in the story is where his ended, with the epoch-making paper of Yang and Mills [5], published in 1954.

In retrospect there may be others who deserve some of the credit — Weyl, Klein, Shaw, Utiyama — but it was undeniably the paper of Yang and Mills that caught people's imagination. They articulated very clearly what has come to be known as the "gauge principle" — sometimes paraphrased as "Nature abhors a rigid symmetry".

I remarked earlier that both our major themes — symmetries and gauge theories — grew from the success of isospin symmetry. The work of Yang and Mills was specifically in that context. They argued that a rigid, global isospin symmetry is incompatible with the notion of locality inherent in relativistic field theory. Their point was that once the concept of isospin symmetry has been accepted (and neglecting for the moment the small electromagnetic symmetry-breaking effect), it becomes a matter of arbitrary convention which component is identified with the proton and which with the neutron; it is a question of how one chooses one's axes in isospin space. But it then seems odd that making the choice here and now should automatically fix it throughout all of space and at all times. So they asked the question: can we make isospin into a *local* symmetry?

Of course, we all know the answer now, but it may still be instructive to recall the argument, very briefly.

We start with a Lagrangian $\mathcal{L}$ invariant under the rigid isospin transformation

$$\delta\psi(x) = -i\delta\vec{\omega}\cdot\vec{T}\psi(x), \tag{1}$$

where $\vec{T} = \{T_i\}$ are the isospin generators, satisfying

$$[T_i, T_j] = i\epsilon_{ijk}T_k. \tag{2}$$

The question is: can it be made invariant also under the corresponding local transformation

$$\delta\psi(x) = -i\delta\vec{\omega}(x)\cdot\vec{T}\psi(x)? \tag{3}$$

The original Lagrangian is not invariant, because the derivative of ψ transforms inhomogeneously. It can be made invariant by replacing $\partial_\mu\psi$ by a *covariant* derivative,

$$D_\mu\psi = \partial_\mu\psi - ig\vec{A}_\mu\cdot\vec{T}\psi, \tag{4}$$

where $\vec{A}_\mu = \{A_{\mu i}\}$ is a triplet of gauge fields transforming according to

$$\delta\vec{A}_\mu = \delta\vec{\omega}\times\vec{A}_\mu - \frac{1}{g}\partial_\mu\delta\vec{\omega}, \tag{5}$$

so that

$$\delta(D_\mu\psi) = -i\delta\vec{\omega}\cdot\vec{T}D_\mu\psi. \tag{6}$$

The Lagrangian for the gauge field must also be invariant, and may be taken to be

$$\mathcal{L}_g = -\tfrac{1}{4}\vec{F}_{\mu\nu}\cdot\vec{F}^{\mu\nu}, \tag{7}$$

where

$$\vec{F}_{\mu\nu} = \partial_\mu\vec{A}_\nu - \partial_\nu\vec{A}_\mu + g\vec{A}_\mu\times\vec{A}_\nu. \tag{8}$$

The great point of all this is that it provides a reason for interactions to exist, and also prescribes a very specific form for the interaction. Of course the argument is not in the nature of a deductive proof — no such proof exists — but it is a very strong heuristic argument from symmetry to interaction.

The gauge principle provided a beautiful description of the electromagnetic interactions. So it was very natural after the work of Yang and Mills that people began to look for gauge theories of the strong and weak interactions.

The emphasis in the early papers was on finding a gauge theory of strong interactions; that is what Yang and Mills themselves were after. But it was hard to make progress because no one knew how to calculate anything. Given the large value of the coupling constant, one would not expect perturbation theory to work; of course no one knew then about asymptotic freedom.

So gradually the weak interactions began to seem a better bet. There were certainly tantalizing hints of a structure very similar to QED.

5. The $V-A$ interaction

The phenomenologically successful theory of beta decay was Fermi's theory involving a direct four-fermion interaction. This was obviously non-renormalizable. It was natural to suppose that it was really an effective interaction due to the exchange of a massive boson, so that in place of the diagram of Figure 1 one should really have that of Figure 2.

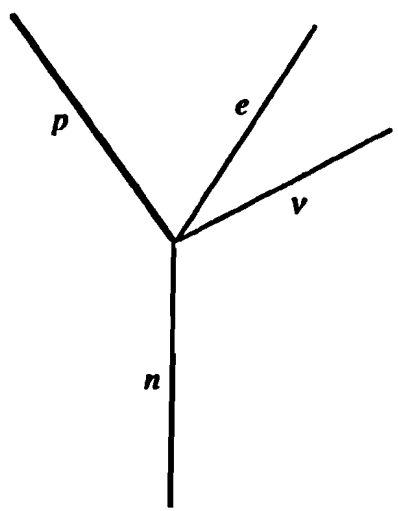

Figure 1: The Fermi interaction

Progress was held up for quite a while because of experimental results that seemed to show that the interaction was predominantly scalar and tensor rather than vector and axialvector. The real breakthrough came with another contribution of Yang's — the suggestion by Lee and Yang [6] that parity is not conserved in weak interactions. As you all know, that led directly to the $V-A$ theory of Marshak and Sudarshan [7] and Feynman and Gell-Mann [8].

As O'Raifeartaigh mentioned, Salam was one of the first people [9] to point out the connection between γ_5 symmetry and the masslessness of the neutrino. This is a very interesting and significant paper.

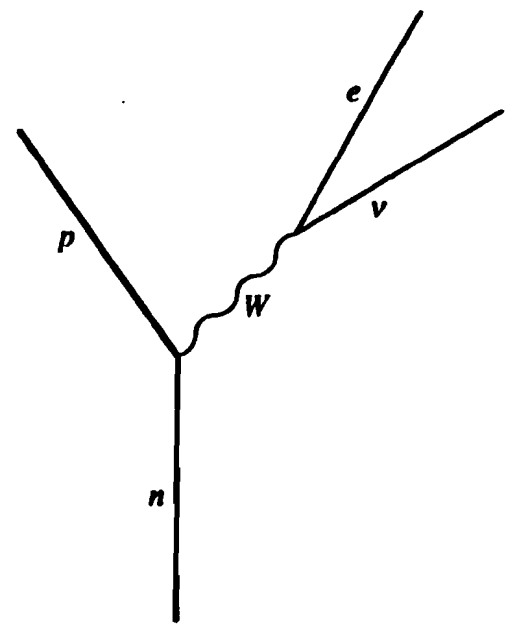

Figure 2: The intermediate vector boson interaction

The discovery of the $V-A$ structure of weak interactions in 1957–58 implied that they could be seen as mediated by vector bosons, $W^{\pm}$, thus naturally encouraging the idea that they were described by a gauge theory.

But there still seemed to be an insuperable difficulty: the large mass of the W bosons. If the interaction were of the same strength as electromagnetism, the W would have a mass of about 40 GeV. But of course putting in a mass term in the Lagrangian would destroy the gauge invaiance. Moreover, the propagator of a massive vector field,

$$\left(g_{\mu\nu} - \frac{k_\mu k_\nu}{m^2}\right) \frac{-i}{k^2 - m^2 + i0} \tag{9}$$

is very badly behaved at infinity and leads to a non-renormalizable theory.

As early as 1958, Salam and Ward [10] proposed a unified gauge theory of weak and electromagnetic interactions, involving a triplet of vector mesons with W^0 identified with the photon. They placed the electron, neutrino and positron too in a triplet, and assumed the interaction

$$\mathcal{L}_{\rm int} = g\vec{\psi} \times \vec{\psi}\cdot\vec{W}, \tag{10}$$

where

$$\vec{\psi} = \begin{pmatrix} e^+ \\ \nu \\ e^- \end{pmatrix}, \qquad \vec{W} = \begin{pmatrix} W^+ \\ \gamma \\ W^- \end{pmatrix}. \tag{11}$$

This was a very ingenious theory, but of course they could only obtain directly the parity-conserving part of the weak interaction. They generated the parity violation by imposing on the resulting theory Salam's γ_5 invariance. And they had to put in the W mass by hand.

Two years later [11] they actually 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 make it a gauge theory with zero-mass gauge bosons.

6. Folk theorems

As often happens, progress was delayed by a "folk theorem". Theoretical physicists have a tendency to develop such things — "theorems" that everyone believes and quotes but that turn out not to be true, or at least not applicable to the cases of interest.

One such folk theorem was the assertion that the photon mass is zero *because* of gauge invariance. The theorem is highly plausible. The bare mass is obviously zero, and within perturbation theory the zero mass is apparently protected by gauge invariance. Indeed the zero photon mass was seen as one of the predictive successes of the gauge principle.

It was Julian Schwinger who pointed out in 1961 that this theorem might be false [12]. This was another of the key papers in the development of the subject — though in a way it propagated a myth of its own. For Schwinger said that "gauge invariance of a vector field does not necessarily imply zero mass for an associated particle *if the current vector coupling is sufficiently strong*" (my emphasis). He was of course thinking primarily of a theory of strong interactions. But at first sight his (perfectly correct) statement seemed to imply that the idea would not work for weak interactions. Of course we now understand that non-perturbative symmetry breaking can occur even in weak-coupling theories, but that was not so obvious at the time.

There was another, related problem too — and another folk theorem to contend with. People began edging towards the idea of spontaneous symmetry breaking as the origin of the vector meson masses. But then there was the Goldstone theorem. In one form, this states that in a relativistic theory, if the Lagrangian but not the vacuum state is invariant under a continuous symmetry transformation, there must exist massless spin-zero particles. But of course such particles were ruled out phenomenologically.

Because of the importance of this issue, when Steven Weinberg came to Imperial College on sabbatical in 1961–62, he and Salam spent a great deal of time going over the proof of the theorem, collaborating at long range with Jeffrey Goldstone. This resulted in a paper that gave a very careful and thorough proof of the theorem [13].

To see just how inescapable the difficulty seemed at the time, I think it is worthwhile recalling the argument in its simplest form.

Let j^μ be the relevant current, satisfying

$$\partial_\mu j^\mu = 0, \tag{12}$$

so that the corresponding charge,

$$Q = \int d^3\mathbf{x}\, j^0(t,\mathbf{x}), \tag{13}$$

is conserved. Suppose there is a field $\phi(x)$ whose vacuum expectation value is not invariant under the symmetry transformation, *i.e.*,

$$i\,\langle 0|[Q,\phi(0)]|0\rangle \neq 0. \tag{14}$$

Define

$$f^\mu(k) = i\int d^4x\, e^{ik\cdot x}\,\langle 0|[j^\mu(x),\phi(0)]|0\rangle\,. \tag{15}$$

From relativistic invariance, since f^μ is a function of a single four-vector k^μ, we conclude that it must have the form

$$f^\mu(k) = k^\mu[g(k^2) + \epsilon(k^0)h(k^2)]. \tag{16}$$

Current conservation, Eq. (12), then implies

$$0 = k_\mu f^\mu = k^2 g(k^2) + \epsilon(k^0)k^2 h(k^2). \tag{17}$$

So g and h can only be proportional to the mass-shell delta function, $\delta(k^2)$. On the other hand the symmetry-breaking condition, Eq. (14), implies

$$\int \frac{dk^0}{2\pi} f(k^0,0) \neq 0. \tag{18}$$

Thus h cannot vanish. This implies that there are zero-mass states that couple to the vacuum through the current in question (and through ϕ).

In condensed-matter physics, counter-examples to the Goldstone theorem wereknown, in the case of long-range forces. But relativistic invariance seemed to rule out such a mechanism. For quite a while this seemed a very severe obstacle. Zero-mass particles coupled in the appropriate way could not possibly have existed and escaped detection.

7. Evading the Goldstone theorem

The next step in the story, at least from my perspective, was an important paper by Phil Anderson, published in April 1963 [14]. The declared aim of that paper was to exhibit an example of Schwinger's suggestion that in suitable cases a gauge field can acquire a mass. One example he gave was the plasmon: the fact that in a high-density plasma the photon acquires a non-zero "mass", namely the plasma frequency,

$$m_\gamma = \omega_{\text{plasma}} = \sqrt{\frac{n_e e^2}{m_e}}. \tag{19}$$

But this paper actually did much more than exhibit a special case of Schwinger's mechanism. Anderson also pointed out, using the example of superconductivity, how in some cases Goldstone bosons "become tangled up with Yang-Mills gauge bosons and, thus, do not in any true sense have zero mass". He concluded that "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 just what is now known as the Higgs mechanism.

This should have cleared everything up. Reading Anderson's paper now, it seems fairly straightforward, but that is not how it seemed at the time. I have to admit that one of the

reasons I started working on this problem was my total failure to understand that paper. If my memory serves me correctly, I took it to Gerry Guralnik, and possibly also Dick Hagen, both of whom were on sabbatical at Imperial that year, saying "Can you make any sense of this?"

Basically, what we found hard to understand was just *how* Anderson had managed to evade the Goldstone theorem. It took us quite a long time to appreciate that the theorem actually doesn't apply to gauge symmetries. By the time we reached that conclusion, others had reached it too. The result was published independently by Englert and Brout [15] and by Peter Higgs [16]; our paper followed a little later [17].

Of course the reason the Goldstone theorem does not apply to gauge theories is by now very well known. If one uses a physical gauge such as the Coulomb gauge, then manifest relativistic invariance is lost. On the other hand, in a covariant gauge, the theorem does apply, but the predicted massless states are unphysical, pure gauge modes.

So 1963–64 saw a great step forward. At least in principle the problem of the origin of mass was solved. Further work on more explicit models was done in the next few years, by Peter Higgs [18] and by myself [19], among others.

8. Dénouement

But there was still another major hurdle to overcome. The key remaining difficulty was to reconcile the requirement of unification with the fact that the weak intereactions are parity-violating, while the electromagnetic interactions are not. In a gauge theory, the $W^{\pm}$ had to have a neutral partner, W^0. At least in the simplest models, $W^{\pm}$ would acquire a mass but W^0 would not. That was fine if W^0 was the photon; but that idea didn't work because if W^0 was partnered with $W^{\pm}$ it too would have parity-violating interactions.

Once again, there seemed no way out — either one had an unobserved massless gauge boson or else a parity-violating photon. Neither was acceptable. It took another three years for people to realise that the resolution of this dilemma was, as often happens, to accept neither horn, but rather a synthesis of the two. The essential key of course was to extend the gauge group from SU(2) to SU(2) × U(1).

Actually the solution, or something very like it, was already there in the literature, in the form of a 1961 paper by Sheldon Glashow [20], which proposed an SU(2) × U(1) model with mixing between the neutral particles, but of course without the key concept of spontaneous symmetry breaking and the Higgs mechanism.

At Imperial, Salam kept plugging away at the problem, especially in collaboration with John Ward. I had quite a few discussions with him about symmetry breaking during that period.

As fate would have it, in a sense I missed the dénouement of this story, but I would still like to tell you about my reaction to it.

In the summer of 1967, I went to Brookhaven and then on to a year's sabbatical at the University of Rochester. In the autumn of that year, Salam gave a series of lectures at Imperial in which he expounded the SU(2) × U(1) theory, though I only heard about them later — remember there was no e-mail then, and we still used transatlantic telephones, at least from the British end, only in dire emergencies! The work he talked about was presented at a Nobel Symposium early the following year [21]. Meanwhile, the same model had been found independently by Steven Weinberg [22].

It is easy with hindsight see the discovery of the spontaneously broken SU(2) × U(1) theory as a crucial turning point. But it is salutory to recall how it seemed at the time.

When Weinberg's paper appeared in Phys. Rev. Letters, Marshak (who of course knew of my interest in such things) asked me to give a talk to his weekly discussion group. The entire high energy group used to meet regularly once a week in his office — fortunately a very big office — for an informal and wide-ranging seminar. Various group members would be asked to talk about exciting recent papers or current problems.

So I gave a talk about Weinberg's paper, mentioning that Salam and Ward had been working on very similar ideas. I talked about the various problems one faced in constructing a unified theory of weak and electromagnetic interactions and pointed out how ingeniously the model avoided them. The most important thing, I felt, was a sort of existence proof: it proved that this *could* be done. I described it, I believe, as a really wonderful toy model — but, of course, I said, one cannot seriously believe this has anything to do with the real world!

In retrospect, I was certainly myopic. But in a sense, my reasons were absolutely 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 adjustable parameters. If one thinks of it as part of the final, fundamental theory, it *is* ugly; surely, one feels, 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 SU(2) × U(1) fits together does have a remarkable beauty.

9. Postscript

That was by no means the end of the story. So far as electroweak unification was concerned, there were several important steps still to come: the GIM mechanism in 1970 [23], 't Hooft's proof of renormalizability in 1971 [24], the discovery of neutral currents in 1973 [25], and of the W and Z bosons in 1983 [26, 27].

And of course Salam went on to even grander unification, for example in his work with Pati on lepton-hadron unification [28] and with Strathdee on supersymmetry and superfields [29]; I could mention many others.

But although it is not the end of the story — indeed, will there be an end to the story? — it is a good place for me to stop.

Before I do so, however, I would like to repeat what I said at the beginning. It was a tremendous privilege to be part of the team led by Professor Abdus Salam, and to participate in this voyage of discovery. I cannot imagine a place I would rather have been during those critical and exciting years. Many of us here, myself included, owe Professor Salam a great debt for the stimulus, support and friendship he has given us over many years. I hope that this meeting has helped in some way to express that gratitude as well as our warm good wishes for the future.

Acknowledgements

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