

GAUGE UNIFICATION OF FUNDAMENTAL FORCES

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by

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Introduction: In June 1938, Sir George Thomson, then Professor of Physics at Imperial College, London, delivered his 1937 Nobel Lecture. Speaking of Alfred Nobel, he said: "The idealism which permeated his character led him to . . . (being) as much concerned with helping science as a whole, as individual scientists. . . . The Swedish people under the leadership of the Royal Family and through the medium of the Royal Academy of Sciences have made Nobel Prizes one of the chief causes of the growth of the prestige of science in the eyes of the world . . . As a recipient of Nobel's generosity, I owe sincerest thanks to them as well as to him."

I am sure I am echoing my colleagues' feelings as well as my own, in reinforcing what Sir George Thomson said - in respect of Nobel's generosity and its influence on the growth of the prestige of science. Nowhere is this more true than in the developing world. And it is in this context that I have been encouraged by the Permanent Secretary of the Academy - Professor Carl Gustaf Bernhard - to say a few words before I turn to the scientific part of my lecture.

Scientific thought and its creation is the common and shared heritage of mankind. In this respect, the history of science, like the history of all civilization, has gone through cycles. Perhaps I can illustrate this with an actual example.

Seven hundred and sixty years ago, a young Scotsman left his native glens to travel south to Toledo in Spain. His name was Michael, his goal to live and work at the Arab Universities of Toledo and Cordova, where the greatest of Jewish scholars, Moses bin Maimoun, had taught a generation before.

Michael reached Toledo in 1217 AD. Once in Toledo, Michael formed the ambitious project of introducing Aristotle to Latin Europe, translating not from the original Greek, which he did not know, but from the Arabic translation then taught in Spain. From Toledo, Michael travelled to Sicily, to the Court of Emperor Frederick II.

Visiting the medical school at Salerno, chartered by Frederick in 1231, Michael met the Danish physician, Henrik Harpestraeng - later to become Court Physician of King Erik Plovpenning. Henrik had come to Salerno to compose his treatise on blood-letting and surgery. Henrik's

sources were the medical canons of the great clinicians of Islam, Al-Razi and Avicenna, which only Michael the Scot could translate for him.

Toledo's and Salerno's schools, representing as they did the finest synthesis of Arabic, Greek, Latin and Hebrew scholarship, were some of the most memorable of international assays in scientific collaboration. To Toledo and Salerno came scholars not only from the rich countries of the East and the South, like Syria, Egypt, Iran and Afghanistan, but also from developing lands of the West and the North like Scotland and Scandinavia. Then, as now, there were obstacles to this international scientific course, with an economic and intellectual disparity between different parts of the world. Men like Michael the Scot or Henrik Harpestraeng were singularities. They did not represent any flourishing schools of research in their own countries. With all the best will in the world their teachers at Toledo and Salerno doubted the wisdom and value of training them for advanced scientific research. At least one of his masters counselled young Michael the Scot to go back to clipping sheep and to the weaving of woollen cloth.

In respect of this cycle of scientific disparity, perhaps I can be more quantitative. George Sarton, in his monumental five-volume *History of Science* chose to divide his story of achievement in sciences into ages, each age lasting half a century. With each half century he associated one central figure. Thus 450 BC - 400 BC Sarton calls the Age of Plato; this is followed by half centuries of Aristotle, of Euclid, of Archimedes and so on. From 600 AD to 650 AD is the Chinese half century of Hsian Tsang, from 650 to 700 AD that of I-Ching, and then from 750 AD to 1100 AD - 350 years continuously - it is the unbroken succession of the Ages of Jabir, Khwarizmi, Razi, Masudi, Wafa, Biruni and Avicenna, and then Omar Khayam - Arabs, Turks, Afghans and Persians - men belonging to the culture of Islam. After 1100 appear the first Western names; Gerard of Cremona, Roger Bacon - but the honours are still shared with the names of Ibn-Rushd (Averroes), Moses Bin Maimoun, Tusi and Ibn-Nafi-the man who anticipated Harvey's theory of circulation of blood. No Sarton has yet chronicled the history of scientific creativity among the pre-Spanish Mayas and Aztecs, with their invention of the zero, of the calendars of the 'moon and Venus and of their diverse pharmacological discoveries, including quinine, but the outline of the story is the same - one of undoubted superiority to the Western contemporary correlates.

After 1350, however, the developing world loses out except for the occasional flash of scientific work, like that of Ulugh Beg - the grandson of Timurlane, in Samarkand in 1400 AD; or of Maharaja Jai Singh of Jaipur in 1720 - who corrected the serious errors of the then Western tables of eclipses of the sun and the moon by as much as six minutes of arc. As it was, Jai Singh's techniques were surpassed soon after with the development of the telescope in Europe. As a contemporary Indian chronicler wrote: "With him on the funeral pyre, expired also all science in the East." And this brings us to this century when the cycle begun by Michael the Scot

turns full circle, and it is we in the developing world who turn to the Westwards for science. As Al-Kindi wrote 1100 years ago: "It is fitting then for us not to be ashamed to acknowledge and to assimilate it from whatever source it comes to us. For him who scales the truth there is nothing of higher value than truth itself; it never cheapens nor abases him."

Ladies and Gentlemen,

It is in the spirit of Al-Kindi that I start my lecture with a sincere expression of gratitude to the modern equivalents of the Universities of Toledo and Cordova, which I have been privileged to be associated with - Cambridge, Imperial College, and the Centre at Trieste.

I. FUNDAMENTAL PARTICLES, FUNDAMENTAL FORCES AND GAUGE UNIFICATION

The Nobel lectures this year are concerned with a set of ideas relevant to the gauge unification of the electromagnetic force with the weak nuclear force. These lectures coincide nearly with the 100th death-anniversary of Maxwell, with whom the first unification of forces (electric with the magnetic) matured and with whom gauge theories originated. They also nearly coincide with the 100th anniversary of the birth of Einstein - the man who gave us the vision of an ultimate unification of *all* forces.

The ideas of today started more than twenty years ago, as gleams in several theoretical eyes. They were brought to predictive maturity over a decade back. And they started to receive experimental confirmation some six years ago.

In some senses then, our story has a fairly long background in the past. In this lecture I wish to examine some of the theoretical gleams of today and ask the question if these may be the ideas to watch for maturity twenty years from now.

From time immemorial, man has desired to comprehend the complexity of nature in terms of as few elementary concepts as possible. Among his quests - in Feynman's words - has been the one for "wheels within wheels" - the task of natural philosophy being to discover the innermost wheels if any such exist. A second quest has concerned itself with the fundamental forces which make the wheels go round and enmesh with one another. The greatness of gauge ideas - of gauge field theories - is that they reduce these two quests to just one; elementary particles (described by relativistic quantum fields) are representations of certain charge operators, corresponding to gravitational mass, spin, flavour, colour, electric charge and the like, while the fundamental forces are the forces of attraction or repulsion between these same charges. A third quest seeks for a *unification* between the charges (and thus of the forces) by searching for a single entity, of which the various charges are components in the sense that they can be transformed one into the other.

But are all fundamental forces gauge forces? Can they be understood as such, in terms of charges - and their corresponding currents - only? And if they are, how many charges? What unified entity are the charges components of?

What is the nature of charge? Just as Einstein comprehended the nature of gravitational charge in terms of space-time curvature, can we comprehend the nature of the other charges - the nature of the entire unified set, *as a set*, in terms of something equally profound? This briefly is the dream, much reinforced by the verification of gauge theory predictions. But before I examine the new theoretical ideas on offer for the future in this particular context, I would like your indulgence to range over a one-man, purely subjective, perspective in respect of the developments of the last twenty years themselves. The point I wish to emphasize during this part of my talk was well made by G. P. Thomson in his 1937 Nobel Lecture. G. P. said ". . . The goddess of learning is fabled to have sprung full grown from the brain of Zeus, but it is seldom that a scientific conception is born in its final form, or owns a single parent. More often it is the product of a series of minds, each in turn modifying the ideas of those that came before, and providing material for those that come after."

II. THE EMERGENCE OF SPONTANEOUSLY BROKEN $SU(2) \times U(1)$ GAUGE THEORY

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 year 1949 was the culminating year of the Tomonaga-Schwinger-Feynman-Dyson reformulation of renormalized Maxwell-Dirac gauge theory, and its triumphant experimental vindication. A field theory must be renormalizable and be capable of being made free of infinities - first discussed by Waller - if perturbative calculations with it are to make any sense. More - a renormalizable theory, with no dimensional parameter in its interaction term, connotes *somehow* that the fields represent "structureless" elementary entities. With Paul Matthews, we started on an exploration of renormalizability of meson theories. Finding that renormalizability held only for spin-zero mesons and that these were the only mesons that empirically existed then, (pseudoscalar pions, invented by Kemmer, following Yukawa) one felt thrillingly euphoric that with the triplet of pions (considered as the carriers of the strong nuclear force between the proton-neutron doublet) one might resolve the dilemma of the origin of this particular force which is responsible for fusion and fission. By the same token, the so-called weak nuclear force - the force responsible for β -radioactivity (and described then by Fermi's non-renormalizable theory) had to be mediated by some unknown spin-zero mesons if it was to be renormalizable. If massive charged spin-one mesons were to mediate this interaction, the theory would be non-renormalizable, according to the ideas then.

Now this agreeably renormalizable spin-zero theory for the pion was a field theory, but not a gauge field theory. There was no conserved charge

which determined the pionic interaction. As is well known, shortly after the theory was elaborated, it was found wanting. The $(\frac{3}{2}, \frac{3}{2})$ resonance Δ effectively killed it off as a fundamental theory; we were dealing with a complex dynamical system, not “structureless” in the held-theoretic sense.

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 Professor Yang expound his and Professor Lee's ideas[1] 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 (τ, θ) puzzle. I remember travelling back to London on an American Air Force (MATS) 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 service-men'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 the involvement in radioactivity phenomena of Pauli's neutrino. While crossing over the Atlantic, came back to me a deeply perceptive question about the neutrino which Professor 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, a 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 the γ_5 transformation [2] (later christened “chiral symmetry”). The existence of this symmetry for the massless neutrino must imply a combination $(1 + \gamma_5)$ or $(1 - \gamma_5)$ 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 γ_5 symmetry, rushed out again, got into 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. I would not touch such ideas with a pair of tongs.” 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 Professor Villars of 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[3], Lederman's[4] and Telegdi's[5] experiments were announced showing that left-right symmetry was indeed violated and ideas similar to mine about chiral symmetry were expressed independently by Landau[6] and Lee and Yang[7]. 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[8] 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 30th 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 . . . In the latter, of course, no γ_5 was used in the exponent." and he gave me the full reference of Yang and Mills' paper; (Phys. Rev. 96, 191 (1954)). I quote from his letter: "However, there are dark points in your paper regarding the vector field B_μ . If the rest mass is infinite (or very large), how can this be compatible with the gauge transformation $B_\mu \rightarrow B_\mu - \partial_\mu \Lambda$?" and he concludes 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 (non-Abelian SU(2)-invariant) gauge theory was no news to me. This was because the Yang - Mills theory [9] (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,[10] 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. And this was particularly serious in this context, because Yang and Mills had conjectured the desirable renormalizability of their theory with a proof which relied heavily and exceptionally on the masslessness of their spin-one intermediate mesons. The problem was to be solved only seven years later with the understanding of what is now known as the Higgs mechanism, but I will come back to this later.

Be that as it may, the point I wish to make from this exchange with Pauli is that already in early 1957, just after the first set of parity experiments, many ideas coming to fruition now, had started to become clear. These are:

1. First was the idea of chiral symmetry leading to a V-A theory. In those early days my humble suggestion [2], [8] of this was limited to neutrinos, electrons and muons only, while shortly after, that year, Sudarshan and Marshak,[11] Feynman and Gell-Mann,[12] and Sakurai[13] had the courage to postulate γ_5 symmetry for baryons as well as leptons, making this into a universal principle of physics.'

Concomitant with the (V-A) theory was the result that if weak interactions are mediated by intermediate mesons, these must carry spin one.

2. Second, was the idea of spontaneous breaking of chiral symmetry to generate electron and muon masses: though the price which those latter-day Shylocks, Nambu and Jona-Lasinio[14] and Goldstone[15] exacted for this (i.e. the appearance of massless scalars), was not yet appreciated.
3. And finally, though the use of a Yang-Mills-Shaw (non-Abelian) gauge theory for describing spin-one intermediate charged mesons was suggested already in 1957, the giving of masses to the intermediate bosons through spontaneous symmetry breaking, in a manner to preserve the renormalizability of the theory, was to be accomplished only during a long period of theoretical development between 1963 and 1971.

Once the Yang-Mills-Shaw ideas were accepted as relevant to the charged weak currents - to which the charged intermediate mesons were coupled in this theory - during 1957 and 1958 was raised the question of what was the third component of the SU(2) triplet, of which the charged weak currents were the two members. There were the two alternatives: the electroweak unification suggestion, where the electromagnetic current was assumed to be this third component; and the rival suggestion that the third component was a neutral current unconnected with electroweak unification. With hindsight, I shall

¹Today we believe protons and neutrons are composites of quarks, so that γ_5 symmetry is now postulated for the elementary entities of today - the quarks.

call these the Klein[16] (1938) and the Kemmer[17] (1937) alternatives. The Klein suggestion, made in the context of a Kaluza-Klein five-dimensional space-time, is a real tour-de-force; it combined two hypothetical spin-one charged mesons with the photon in one multiplet, deducing from the compactification of the fifth dimension, a theory which looks like Yang-Mills-Shaw's. Klein intended his charged mesons for *strong* interactions, but if we read charged *weak* mesons for Klein's *strong* ones, one obtains the theory independently suggested by Schwinger[18] (1957), though Schwinger, unlike Klein, did not build in any non-Abelian gauge aspects. With just these non-Abelian Yang-Mills gauge aspects very much to the fore, the idea of uniting weak interactions with electromagnetism was developed by Glashow[19] and Ward and myself[20] in late 1958. The rival Kemmer suggestion of a global SU(2)-invariant triplet of weak charged and neutral currents was independently suggested by Bludman[21] (1958) in a gauge context and this is how matters stood till 1960.

To give you the flavour of, for example, the year 1960, there is a paper written that year of Ward and myself[22] with the statement: "Our basic postulate is that it should be possible to generate strong, weak and electromagnetic interaction terms with all their correct symmetry properties (as well as with clues regarding their relative strengths) by making local gauge transformations on the kinetic energy terms in the free Lagrangian for all particles. This is the statement of an ideal which, in this paper at least, is only very partially realized". I am not laying a claim that we were the only ones who were saying this, but I just wish to convey to you the temper of the physics of twenty years ago - qualitatively no different today from then. But what a quantitative difference the next twenty years made, first with new and far-reaching developments in theory-and then, thanks to CERN, Fermilab, Brookhaven, Argonne, Serpukhov and SLAG in testing it!

So far as theory itself is concerned, it was the next seven years between 1961-67 which were the crucial years of quantitative comprehension of the phenomenon of spontaneous symmetry breaking and the emergence of the SU(2) \times U(1) theory in a form capable of being tested. The story is well known and Steve Weinberg has already spoken about it. So I will give the barest outline. First there was the realization that the two alternatives mentioned above a pure electromagnetic current versus a pure neutral current-Klein-Schwinger versus Kemmer-Bludman - were not alternatives; they were complementary. As was noted by Glashow^[23] and independently by Ward and myself^[24], both types of currents and the corresponding gauge particles (W^+ , Z^0 and γ) were needed in order to build a theory that could simultaneously accommodate parity violation for weak and parity conservation for the electromagnetic phenomena. Second, there was the influential paper of Goldstone[25] in 1961 which, utilizing a non-gauge self-interaction between scalar particles, showed that the price of spontaneous breaking of a continuous internal symmetry was the appearance of zero 'mass scalars-a result foreshadowed earlier by Nambu. In giving a proof of this theorem[26] with Goldstone I collaborated with Steve Weinberg, who spent a year at Imperial College in London.

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