Gauge unification of fundamental forces*

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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 to 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 knew not, but from the Arabic translation then taught in Spain. From Toledo, Michael traveled 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 Eric IV Waldemarssön. Henrick 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, like Syria, Egypt, Iran and Afghanistan, but also from developing lands of the West like Scotland and Scandinavia. Then, as now, there were obstacles to this international scientific concourse, 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 counseled young Michael the Scot to go back to clipping sheep and to the weaving of woolen cloth.

In respect of this cycle of scientific disparity, perhaps I can be more quantitative. George Sarton, in his monumental five-volume *A 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 Hsiian 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. After 1100 appear the first Western names: Gerard of Cremona, Roger Bacon—but the honors are still shared with the names of Ibn-

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Rushd (Averroes), Moses Bin Maimoun, Tusi, and Ibn-Nafis—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 re-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 westward for science. As Al-Kindi wrote 1100 years ago: "It is fitting then for us not to be ashamed to acknowledge truth 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 anniversary of the death 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, flavor, color, 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) × 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–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. By the same token, the so-called weak nuclear force—the force responsible for β radioactivity (and described then by Fermi's nonrenormalizable 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 nonrenormalizable, 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 field-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 (Lee and Yang, 1956) 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 traveling 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 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 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 analog 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 (Salam, 1957a) (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 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 creme. 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 (Wu *et al.*, 1957), Lederman's (Garwin *et al.*, 1957) and Telegdi's (Friedman and Telegdi, 1957) experiments were announced showing that left–right symmetry was indeed violated and ideas similar to mine about chiral symmetry were expressed independently by Landau (1957) and Lee and Yang (1957). 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 (Salam, 1957b)[1] 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

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[1] For reference, see Footnote 7, p. 89, of Marshak, Riazuddin, and Ryan (1969), and W. Pauli's letters (CERN Archives).

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 $B_\mu \to B_\mu - \partial_\mu \Lambda$?" 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 (Yang and Mills, 1954) (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 (1955), 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 (Salam, 1957a,b) of this was limited to neutrinos, electrons, and muons only, while shortly after, that year, Marshak and Sudarshan (Marshak and Sudarshan, 1957 and 1958)[2] Feynman and Gell-Mann (Feynman and Gell-Mann, 1958), and Sakurai (1958) had the courage to postulate γ_5 symmetry for baryons as well as leptons, making this into a universal principle of physics.[3]

Concomitant with the $(V-A)$ theory was the result that if weak interactions are mediated by intermediate mesons, these mesons 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 (Nambu and Jona-Lasinio, 1961) and Goldstone [Nambu (1960) and Goldstone (1961)] 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 such a manner as 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 call these the Klein (1938) (see Klein, 1939) and the Kemmer (1937) alternatives. The Klein suggestion, made in the context of a Kaluza-Klein five-dimensional space-time, was 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 (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 (1959) and Ward and myself (Salam and Ward, 1959) 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 (1958) in a gauge context and this is how matters stood till 1960.

To give you the flavor of, for example, the year 1960,

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[2] The idea of a universal Fermi interaction for (P,N), (ν_e,e), and (ν_μ,μ) doublets goes back to Tiomno and Wheeler (1949a,b) and Yang and Tiomno (1950). Tiomno (1956) considered γ_5 transformations of Fermi fields linked with mass reversal.

[3] Today 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. If the neutrino also turns out to be massive, γ_5-symmetry is spontaneously broken for it, as it is for electrons, muons, and quarks.

there was a paper written that year by Ward and myself (Salam and Ward, 1961) 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 SLAC, 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)× 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 (1961) and independently by Ward and myself (Salam and Ward, 1964), both types of currents and the corresponding gauge particles ($W^{\pm}$, 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 in 1961 which, utilizing a nongauge 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 (Goldstone *et al.*, 1962) with Goldstone, I collaborated with Steve Weinberg, who spent a year at Imperial College in London. I would like to pay here a most sincerely felt tribute to him and to Sheldon Glashow for their warm and personal friendship.

I shall not dwell on the now well-known contributions of Anderson (1963), Higgs (1964a, 1964b, 1966), Brout and Englert (Englert and Brout, 1964; Englert *et al.*, 1966), Guralnik, Hagen, and Kibble (1964; Kibble, 1967) starting from 1963, which showed how spontaneous symmetry breaking using spin-zero fields could generate vector-meson masses, defeating Goldstone at the same time. This is the so-called Higgs mechanism.

The final steps towards the electroweak theory were taken by Weinberg (1967) and by myself (Salam, 1968) (with Kibble at Imperial College tutoring me about the Higgs phenomena). We were able to complete the present formulation of the spontaneously broken SU(2)× U(1) theory so far as leptonic weak interactions were concerned—with one parameter $\sin^2\theta$ describing all weak and electromagnetic phenomena and with one isodoublet Higgs multiplet. An account of this development was given during the contribution (Salam, 1968) to the Nobel Symposium (organized by Nils Svartholm and chaired by Lamek Hulthén held at Gothenburg after some postponements, in early 1968). As is well known, we did not have then, and still do not have, a prediction for the scalar Higgs mass.

Both Weinberg and I suspected that this theory was likely to be renormalizable.[4] Regarding spontaneously broken Yang–Mills–Shaw theories in general this had earlier been suggested by Englert, Brout, and Thiry (1966). But this subject was not pursued seriously except at Veltman's school at Utrecht, where the proof of renormalizability was given by 't Hooft (1971a, b) in 1971. This was elaborated further by that remarkable physicist, the late Benjamin Lee (Lee, 1972; Lee and Zinn-Justin, 1972, 1973), working with Zinn-Justin, and by 't Hooft and Veltman (1972a, 1972b).[5] This followed on the earlier basic advances in Yang–Mills calculational technology by Feynman (1963), DeWitt (1967a, b), Faddeev and Popov (1967), Mandelstam (1968a, b), Fradkin and Tyutin (1970), Boulware (1970), Taylor (1971), Slavnov (1972), Strathdee and Salam (Salam and Strathdee, 1970). In Coleman's eloquent phrase "'t Hooft's work turned the Weinberg–Salam frog into an enchanted prince." Just before had come the GIM (Glashow, Iliopoulos, and Maiani) mechanism (Glashow *et al.*, 1970), emphasizing that the existence of the fourth charmed quark (postulated earlier by several authors) was essential to the natural resolution of the dilemma posed by the absence of strangeness-violating currents. This tied in naturally with the understanding of the Steinberger–Schwinger–Rosenberg–Bell–Jackiw–Adler anomaly (see Jackiw, 1972) and its removal for SU(2) × U(1) by the parallelism of four quarks and four leptons, pointed out by Bouchiat, Iliopoulos, and Meyer (1972) and independently by Gross and Jackiw (1972).

If one has kept a count, I have so far mentioned around fifty theoreticians. As a failed experimenter, I have always felt envious of the ambience of large experimental teams and it gives me the greatest pleasure to acknowledge the direct or the indirect contributions of the "series of minds" to the spontaneously broken SU(2)× U(1) gauge theory. My profoundest personal appreciation goes to my collaborators at Imperial College, and Cambridge and the Trieste Centre, John Ward, Paul Matthews, Jogesh Pati, John Strathdee, Tom Kibble, and to Nicholas Kemmer.

In retrospect, what strikes me most about the early part of this story is how uninformed all of us were, not

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[4] When I was discussing the final version of the SU(2)×U(1) theory and its possible renormalizability in Autumn 1967 during a postdoctoral course of lectures at Imperial College, Nino Zichichi from CERN happened to be present. I was delighted because Zichichi had been badgering me since 1958 with persistent questioning as to of what theoretical avail his precise measurements on (g-2) for the muon as well as those of the muon lifetime were, when not only the magnitude of the electromagnetic corrections to weak decays was uncertain, but also conversely the effect of nonrenormalizable weak interactions on "renormalized" electromagnetism was so unclear.

[5] An important development in this context was the invention of the dimensional regularization technique by Bollini and Giambiagi (1972), Ashmore (1972), and 't Hooft and Veltman.

only of each other's work, but also of work done earlier. For example, only in 1972 did I learn of Kemmer's paper written at Imperial College in 1937. Kemmer's argument essentially was that Fermi's weak theory was not globally SU(2) invariant and should be made so—though not for its own sake but as a prototype for strong interactions. Then this year I learnt that earlier, in 1936, Kemmer's Ph.D. supervisor, Gregor Wentzel (1937), had introduced (the yet undiscovered) analogs of lepto-quarks, whose mediation could give rise to neutral currents after a Fierz reshuffle. And only this summer, Cecilia Jarlskog at Bergen rescued Oscar Klein's paper from the anonymity of the Proceedings of the International Institute of Intellectual Cooperation of Paris, and we learnt of his anticipation of a theory similar to Yang–Mills–Shaw long before these authors. As I indicated before, the interesting point is that Klein was using his triplet, of two charged mesons plus the photon, not to describe weak interaction but for strong nuclear force unification with the electromagnetic—something our generation started on only in 1972—and not yet experimentally verified. Even in this recitation I am sure I have inadvertently left off some names of those who have in some way contributed to SU(2)× U(1). Perhaps the moral is that not unless there is the prospect of quantitative verification, does a qualitative idea make its impress in physics.

And this brings me to experiment, and the year of the Gargamelle (Hasert *et al.*, 1973). 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 peered out of the window and 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 Aix-en-Provence 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. Steve Weinberg gave the rapporteur's talk with T. D. Lee as the chairman. T. D. was kind enough to ask me to comment after Weinberg finished. That summer Jogesh Pati and I had predicted proton decay within the context of what is now called grand unification, and in the flush of this excitement I am afraid I ignored weak neutral currents as a subject which had already come to a successful conclusion, and concentrated on speaking of the possible decays of the proton. I understand now that proton decay experiments are being planned in the United States by the Brookhaven, Irvine and Michigan and the Wisconsin–Harvard groups and also by a European collaboration to be mounted in the Mont Blanc Tunnel Garage No. 17. The later quantitative work on neutral currents at CERN, Fermilab, Brookhaven, Argonne and Serpukhov is, of course, history, but a special tribute is warranted to the beautiful SLAC–Yale–CERN experiment (Taylor, 1979) of 1978 which exhibited the effective Z^0-photon interference in accordance with the predictions of the theory. This was foreshadowed by Barkov *et al.*'s experiments (Barkov, 1979) at Novosibirsk in the USSR in their exploration of parity-violation in the atomic potential for bismuth. 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 marvelous 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." This is exactly how I feel about the Gargamelle–SLAC experience.

III. THE PRESENT AND ITS PROBLEMS

Thus far we have reviewed the last twenty years and the emergence of SU(2)× U(1), with the twin developments of a gauge theory of basic interactions, linked with internal symmetries, and of the spontaneous breaking of these symmetries. I shall first summarize the situation as we believe it to exist now and the immediate problems. Then we turn to the future.

(1) To the level of energies explored, we believe that the following sets of particles are "structureless" (in a field-theoretic sense) and, at least to the level of energies explored hitherto, constitute the elementary entities of which all other objects are made.

$$
\begin{array}{llll}
 & \mathrm{SU}_c(3)\ \text{triplets} & & \\
\text{Family I} & \text{quarks}\begin{Bmatrix} u_R, u_Y, u_B \\ d_R, d_Y, d_B \end{Bmatrix} & \text{leptons}\begin{bmatrix} \nu_e \\ e \end{bmatrix} & \mathrm{SU(2)\,doublets} \\
\text{Family II} & \text{quarks}\begin{Bmatrix} c_R, c_Y, c_B \\ s_R, s_Y, s_B \end{Bmatrix} & \text{leptons}\begin{bmatrix} \nu_\mu \\ \mu \end{bmatrix} & \mathrm{SU(2)\,doublets} \\
\text{Family III} & \text{quarks}\begin{Bmatrix} t_R, t_Y, t_B \\ b_R, b_Y, b_B \end{Bmatrix} & \text{leptons}\begin{bmatrix} \nu_\tau \\ \tau \end{bmatrix} & \mathrm{SU(2)\,doublets}
\end{array}
$$

Together with their antiparticles each family consists of 15 or 16 two-component fermions (15 or 16 depending on whether the neutrino is four-component or not). The third family is still conjectural, since the top quark (t_R, t_Y, t_B) has not yet been discovered. Does this family really follow the pattern of the other two? Are there more families? Does the fact that the families are replicas of each other imply that Nature has discovered a dynamical stability about a system of 15 (or 16) objects, and that by this token there is a more basic layer of structure underneath? (See Pati and Salam, 1975a; Pati *et al.*, 1975a; Harari, 1979; Schupe, 1979; Curtwright and Fruend, 1979).

(2) Note that quarks come in three colors: Red (R), Yellow (Y), and Blue (B). Parallel with the electroweak SU(2)× U(1), a gauge field[6] theory (SU$_c$(3)) of

strong (quark) interactions (quantum chromodynamics, QCD[7] has emerged which gauges the three colors. The indirect discovery of the (eight) gauge bosons associated with QCD (gluons), has already been surmised by the groups at DESY.[8]

(3) All known baryons and mesons are singlets of color $SU_c(3)$. This has led to a hypothesis that color is always confined. One of the major unsolved problems of field theory is to determine if QCD—treated nonperturbatively—is capable of confining quarks and gluons.

(4) In respect of the electroweak $SU(2) \times U(1)$, all known experiments on weak and electromagnetic phenomena below 100 GeV carried out to date agree with the theory which contains one theoretically undetermined parameter $\sin^2\theta = 0.230 \pm 0.009$ (Winter, 1979). The predicted values of the associated gauge boson ($W^\pm$ and Z^0) masses are: $m_W \approx 77-84$ GeV, $m_Z \approx 89-95$ GeV, for $0.25 \geq \sin^2\theta \geq 0.21$.

(5) Perhaps the most remarkable measurement in electroweak physics is that of the parameter $\rho = (m_W/m_Z \cos\theta)^2$. Currently this has been determined from the ratio of neutral to charged current cross sections. The predicted value $\rho = 1$ for weak *iso-doublet Higgs* is to be compared with the experimental[9] $\rho = 1.00 \pm 0.02$.

(6) Why does Nature favor the simplest suggestion in $SU(2) \times U(1)$ theory of the Higgs scalars being iso-doublet?[10] Is there just one physical Higgs? Of what mass? At present the Higgs interactions with leptons and quarks as well as their self-interactions are nongauge interactions. For a three-family (six-quark) model, 21 out of the 26 parameters needed are attributable to the Higgs interactions. Is there a basic principle, as compelling and as economical as the gauge principle, which embraces the Higgs sector? Alternatively, could the Higgs phenomenon itself be a manifestation of a dynamical breakdown of the gauge symmetry?[10]

(7) Finally there is the problem of the families; is there a distinct SU(2) for the first, another for the second, as well as a third SU(2), with spontaneous symmetry breaking such that the SU(2) apprehended by present experiment is a diagonal sum of these "family" SU(2)'s? To state this in another way, how far in energy does the $e-\mu$ universality (for example) extend? Are there more[11] Z^0's than just one, effectively differentially coupled to the e and the μ systems? (If there are, this will constitute minimodifications of the theory, but not a drastic revolution of its basic ideas.)

In the next section I turn to a direct extrapolation of the ideas which went into the electroweak unification, so as to include strong interactions as well. Later I shall consider the more drastic alternatives which may be needed for the unification of all forces (including gravity)—ideas which have the promise of providing a deeper understanding of the charge concept. Regretfully, by the same token, I must also become more technical and obscure for the nonspecialist. I apologize for this. The nonspecialist may sample the flavor of the arguments in the next section (Sec. IV), ignore the Appendices, and then go on to Sec. V, which is perhaps less technical.

IV. DIRECT EXTRAPOLATION FROM THE ELECTROWEAK TO THE ELECTRONUCLEAR

A. The three ideas

The three main ideas which have gone into the electronuclear—also called grand—unification of the electroweak with the *strong* nuclear force (and which date back to the period 1972–1974), are the following:

(1) First: the psychological break (for us) of grouping quarks and leptons in the *same* multiplet of a unifying group G, suggested by Pati and myself in 1972 (see Bjorken, 1972; Pati and Salam, 1973a). The group G must contain $SU(2) \times U(1) \times SU_c(3)$, and must be non-Abelian, if all quantum numbers (flavor, color, lepton, quark, and family numbers) are to be automatically quantized and the resulting gauge theory asymptotically free.

(2) Second: an extension, proposed by Georgi and Glashow (1974) which places not only (left-handed) quarks and leptons but also their antiparticles in the same multiplet of the unifying group.

Appendix I displays some examples of the unifying groups presently considered.

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[6] "To my mind the most striking feature of theoretical physics in the last thirty-six years is the fact that not a single new theoretical idea of a fundamental nature has been successful. The notions of relativistic quantum theory....have in every instance proved stronger than the revolutionary ideas....of a great number of talented physicists. We live in a dilapidated house and we seem to be unable to move out. The difference between this house and a prison is hardly noticeable"—Res Jost (1963), "In Praise of Quantum Field Theory" (Siena European Conference).

[7] Pati and Salam. See the review by Bjorken (1972). See also Fritzsch and Gell-Mann (1972), Fritzsch, Gell-Mann, and Leutwyler (1973), Weinberg (1973a,b), and Gross and Wilczek (1973). For a review see Marciano and Pagels (1978).

[8] See the Tasso Collaboration (Brandelik *et al.*, 1979) and the Mark-J Collaboration (Barber *et al.*, 1979). See also the reports of the Jade, Mark-J, Pluto, and Tasso Collaborations to the International Symposium on Lepton and Photon Interactions at High Energies, Fermilab, August 1979.

[9] The one-loop radiative corrections to ρ suggest that the maximum mass of leptons contributing to ρ is less than 100 GeV (Ellis, 1979).

[10] To reduce the arbitrariness of the Higgs couplings and to motivate their iso-doublet character, one suggestion is to use supersymmetry. Supersymmetry is a Fermi-Bose symmetry, so that iso-doublet leptons like (ν_e, e) or (ν_μ, μ) in a supersymmetric theory must be accompanied in the same multiplet by iso-doublet Higgs. Alternatively, one may identify the Higgs as composite fields associated with bound states of a yet new level of elementary particles and new (so-called techni-color) forces (Dimopoulos and Susskind, 1979; Weinberg, 1979a; and 't Hooft) of which, at present low energy, we have no cognizance, and which may manifest themselves in the 1–100 TeV range. Unfortunately, both these ideas at first sight appear to introduce complexities, though in the context of a wider theory, which spans energy scales up to much higher masses, a satisfactory theory of the Higgs phenomena, incorporating these, may well emerge.

[11] See Pati and Salam (1974); Mohapatra and Pati (1975a,b); Elias, Pati, and Salam (1978a); and Pati and Rajpoot (1978).

Now a gauge theory based on a "simple" (or with discrete symmetries, a "semisimple") group G contains one basic gauge constant. This constant would manifest itself physically above the "grand unification mass" M, exceeding all particle masses in the theory—these themselves being generated (if possible) hierarchially through a suitable spontaneous symmetry-breaking mechanism.

(3) The third crucial development was by Georgi, Quinn, and Weinberg (1974) who showed how, using renormalization group ideas, one could relate the observed low-energy couplings $\alpha(\mu)$ and $\alpha_s(\mu)$ $(\mu \sim 100\ \text{GeV})$ to the magnitude of the grand unifying mass M and the observed value of $\sin^2\theta(\mu)$; ($\tan\theta$ is the ratio of the U(1) to the SU(2) couplings).

(4) If one extrapolates with Jowett,[12] that nothing essentially new can possibly be discovered—i.e., if one assumes that there are no new features, no new forces, or no new "types" of particles to be discovered, till we go beyond the grand unifying energy M—then the Georgi, Quinn, Weinberg method leads to a startling result: this featureless "plateau" with no "new physics" heights to be scaled stretches to fantastically high energies. More precisely, if $\sin^2\theta(\mu)$ is as large as 0.23, then the grand unifying mass M cannot be smaller than 1.3×10^{13} GeV (Marciano, 1979). (Compare with Planck mass $m_P \approx 1.2\times 10^{19}$ GeV related to Newton's constant where gravity must come in.)[13] The result follows from the formula (Marciano, 1979; Salam, 1979).

$$\frac{11\alpha}{3\pi}\ln\frac{M}{\mu}=\frac{\sin^2\theta(M)-\sin^2\theta(\mu)}{\cos^2\theta(M)}, \tag{1}$$

if it is assumed that $\sin^2\theta(M)$—the magnitude of $\sin^2\theta$ for energies of the order of the unifying mass M—equals 3/8 (see Appendix B).

This startling result will be examined more closely in Appendix B. I show there that it is very much a consequence of the assumption that the $\text{SU}(2)\times\text{U}(1)$ symmetry survives intact from the low regime energies μ right upto the grand unifying mass M. I will also show that there already is some experimental indication that this assumption is too strong, and that there may be likely peaks of new physics at energies of 10 TeV upwards.

B. Tests of electronuclear grand unification

The most characteristic prediction from the existence of the electronuclear force is proton decay, first discussed in the context of grand unification at the Aix-en-Provence Conference (1973) (Pati and Salam, 1973b). For "semisimple" unifying groups with multiplets containing quarks and leptons only (but no antiquarks nor antileptons) the lepto–quark composites have masses (determined by renormalization group arguments) of the order of $\approx 10^5$–10^6 GeV (Elias *et al.*, 1978b; Rajpoot and Elias, 1978). For such theories the characteristic proton decays (proceeding through exchanges of *three* lepto–quarks) conserve quark number + lepton number, i.e., $P = qqq \rightarrow lll$, $\tau_P \sim 10^{29}$–10^{34} years. On the contrary, for the "simple" unifying family groups like SU(5) (Georgi and Glashow, 1974) or SO(10) (Fritzsch and Minkowski, 1975, 1976; Georgi, 1975; Georgi and Nanopoulos, 1979) (with multiplets containing antiquarks and antileptons) proton decay proceeds through an exchange of *one* lepto-quark into an antilepton (plus pions, etc.) $(P \rightarrow \bar{l})$.

An intriguing possibility in this context is that investigated recently for the maximal unifying group SU(16)—the largest group to contain a sixteenfold fermionic family $(q, l, \bar{q}, \bar{l})$. This can permit four types of decay modes: $P \rightarrow 3l$ *as well as* $P \rightarrow \bar{l}, P \rightarrow l$ (e.g., $P \rightarrow l^- + \pi^+ + \pi^+$), and $P \rightarrow 3\bar{l}$ (e.g., $N \rightarrow 3\bar{\nu} + \pi^0, P \rightarrow 2\bar{\nu} + e^+ + \pi^0$), the relative magnitudes of these alternative decays being model-dependent on how precisely SU(16) breaks down to $\text{SU}(3)\times\text{SU}(2)\times\text{U}(1)$. Quite clearly, it is the central fact of the existence of the proton decay for which the present generation of experiments must be designed, rather than for any specific type of decay modes.

Finally, grand unifying theories predict mass relations like (Buras *et al.*, 1978):

$$\frac{m_d}{m_e}=\frac{m_s}{m_\mu}=\frac{m_b}{m_\tau}\approx 2.8$$

for six (or at most eight) flavors *below the unification mass*. The important remark for proton decay and for mass relations of the above type as well as for an understanding of baryon excess[14] in the universe,[15] is that for the present *these are essentially characteristic of the fact of grand unification—rather than of specific models*.

"Yet each man kills the thing he loves" sang Oscar Wilde anguishedly in his famous Ballad of the Reading

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[12] The universal urge to extrapolate from what we know today and to believe that nothing new can possibly be discovered is well expressed in the following:

> "I come first, My name is Jowett
> I am the Master of this College,
> Everything that is, I know it
> If I don't, it isn't knowledge"
>
> — The Balliol Masque.

[13] On account of the relative proximity of $M \approx 10^{13}$ GeV to m_P (and the hope of eventual unification with gravity), Planck mass m_P is now the accepted "natural" mass scale in particle physics. With this large mass as the input, the great unsolved problem of grand unification is the "natural" emergence of mass hierarchies $(m_P,\ \alpha m_P,\ \alpha^2 m_P,\ \dots)$ or $m_P \exp(-c_n/\alpha)$, where c_n's are constants. $[m_e/m_P \sim 10^{-22}.]$

[14] See Yoshimura (1978), Dimopoulos and Susskind (1978), Toussiant *et al.* (1979), Ellis *et al.* (1979), Weinberg (1979b), and Nanopoulos and Weinberg (1979).

[15] The calculation of baryon excess in the universe—arising from a combination of CP and baryon number violations—has recently been claimed to provide teleological arguments for grand unification. For example, Nanopoulos (1979) has suggested that the "existence of human beings to measure the ratio n_B/n_γ (where n_B is the number of baryons and n_γ the number of photons in the universe) necessarily imposes severe bounds on this quantity: i.e., $10^{-11} \approx (m_e/m_P)^{1/2} \lesssim n_B/n_\gamma \lesssim 10^{-4}$ $(\approx 0(\alpha^2))$." Of importance in deriving these constraints are the upper (and lower) bounds on the numbers of flavors (≈ 6) deduced (1) from mass relations above, (2) from cosmological arguments which seek to limit the numbers of massless neutrinos, (3) from asymptotic freedom, and (4) from numerous (one-loop) radiative calculations. It is clear that lack of accelerators as we move up in energy scale will force particle physics to reliance on teleology and cosmology (which, in Landau's famous phrase, is "often wrong, but never in doubt").

Gaol. Like generations of physicists before us, some in our generation also (through a direct extrapolation of the electroweak gauge methodology to the electronuclear)—and with faith in the assumption of no "new physics," which leads to a grand unifying mass $\sim 10^{13}$ GeV—are beginning to believe that the end of the problems of elementarity as well as of fundamental forces is nigh. They may be right, but before we are carried away by this prospect, it is worth stressing that even for the simplest grand unifying model [Georgi and Glashow's SU(5) with just two Higgs (a **5** and a **24**)], the number of presently *ad hoc* parameters needed by the model is still unwholesomely large—22, compared with 26 for the six-quark model based on the humble $SU(2)\times U(1)\times SU_c(3)$. We cannot feel proud.

V. ELEMENTARITY: UNIFICATION WITH GRAVITY AND NATURE OF CHARGE

In some of the remaining parts of this lecture I shall be questioning two of the notions which have gone into the direct extrapolation of Sec. IV—first, do quarks and leptons represent the correct elementary[16] fields, which should appear in the matter Lagrangian, and which are structureless for renormalizability; second, could some of the presently considered gauge fields themselves be composite? This part of the lecture relies heavily on an address I was privileged to give at the European Physical Society meeting in Geneva in July this year (Salam, 1979).

A. The quest for elementarity, prequarks (preons and pre-preons)

If quarks and leptons are elementary, we are dealing with $3\times 15=45$ elementary entities. The "natural" group of which these constitute the fundamental representation is SU(45) with 2024 elementary gauge bosons. It is possible to reduce the size of this group to SU(11) for example (see Appendix A) with only 120 gauge bosons, but then the number of fermions increases to 501 (of which presumably $3\times 15=45$ objects are of low and the rest of Planckian mass). Is there any basic reason for one's instinctive revulsion when faced with these vast numbers of elementary fields?

The numbers by themselves would perhaps not matter so much. After all, Einstein, in his description of gravity (Einstein, 1916), chose to work with 10 fields $[g_{\mu\nu}(x)]$ rather than with just one (scalar field) as Nördstrom [(1912; 1913a, b; 1914a, b); see also Einstein (1912a, b)] before him. Einstein was not perturbed by the multiplicity he chose to introduce, since he relied on the sheet-anchor of a fundamental principle (the equivalence principle) which permitted him to relate the ten fields for gravity $g_{\mu\nu}$ with the ten components of the physically relevant quantity, the tensor $T_{\mu\nu}$ of energy and momentum. *Einstein knew that nature was not economical of structures:* only of principles of fundamental applicability. The question we must ask ourselves is this: Have we yet discovered such principles in our quest for elementarity, to justify having fields with such large numbers of components as elementary?

Recall that quarks carry at least three charges (color, flavor, and a family number). Should one not, by now, entertain the notions of quarks (and possibly of leptons) as being composites of some more basic entities[17] (prequarks or preons), which each carry but *one* basic charge? (Pati and Salam, 1975a; Pati *et al.*, 1975a; Harari, 1979; Schupe, 1979; Curtright and Freund, 1979) These ideas have been expressed before but they have become more compulsive now, with the growing multiplicity of quarks and leptons. Recall that it was similar ideas which led from the eightfold of baryons to a triplet of (Sakatons and) quarks in the first place.

The preon notion is now new. In 1975, among others, Pati, Salam, and Strathdee (1975a) introduced 4 chromons (the fourth color corresponding to the lepton number) and 4 flavons, the basic group being SU(8)—of which the family group $SU_F(4)\times SU_C(4)$ was but a subgroup. As an extension of these ideas, we now believe these preons carry magnetic charges and are bound together by very strong short-range forces, with quarks and leptons as their magnetically neutral composites (Pati and Salam, 1980). The important remark in this context is that in a theory containing *both* electric and magnetic generalized charges, the analogs of the well-known Dirac quantization condition (Dirac, 1931) give relations like $eg/4\pi=n/2$ for the strength of the two types of charges. Clearly, magnetic monopoles[18] of strength g and mass $mw/\alpha\approx 10^4-10^5$ GeV of opposite polarity, are likely to bind much more tightly than electric charges, yielding composites whose nonelementary nature will reveal itself only for very high energies. This appears to be the situation at least for leptons if they are composites.

In another form the preon idea has been revived this year by Curtright and Freund (1979), who, motivated by ideas of extended supergravity (to be discussed in the next subsection), reintroduce an SU(8) of 3 chromons (R, Y, B), 2 flavons, and 3 familons (horrible names). The family group SU(5) could be a subgroup of this SU(8). In the Curtright–Freund scheme, the $\mathbf{3\times 15=45}$ fermions of SU(5) (Georgi and Glashow, 1974) can be found among the $\mathbf{8+28+56}$ of SU(8) [or alternatively the $\mathbf{3\times 16=48}$ of SO(10) among the vectorial **56** fermions of SU(8)]. (The next succession after the preon level may be the pre-preon level. It was suggested at the Geneva

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[16] I would like to quote Feynman in a recent interview in *Omni* magazine: "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 skeptical about the gauge theories.... I was expecting mist, and now it looks like ridges and valleys after all."

[17] One must emphasize, however, that zero mass neutrinos are the hardest objects to conceive of as composites.

[18] According to 't Hooft's theorem, a monopole corresponding to the $SU_L(2)$ gauge symmetry is expected to possess a mass with the lower limit m_W/α ('t Hooft, 1974; Polyakov, 1974). Even if such monopoles are confined, their indirect effects must manifest themselves, if they exist. (Note that m_W/α is very much a lower limit for a grand unified theory like SU(5), for which the monopole mass is α^{-1} times the heavy leptoquark mass.) The monopole force may be the techni-force of Footnote 10.

TABLE I. Prognosis for the next decade.

Decade	1950–1960	1960–1970	1970–1980	1980 →
Discovery in early part of the decade	The strange particles	The 8-fold way, Ω^-	Confirmation of neutral currents	W, Z, Proton decay
Expectation for the rest of the decade		SU(3) resonances		Grand Unification, Tribal Groups
Actual discovery		Hit the next level of elementarity with quarks		May hit the preon level, and composite structure of quarks

Conference (see Salam, 1979) that with certain developments in field theory of composite fields it could be that just two pre-preons may suffice. But at this stage this is pure speculation.)

Before I conclude this section, I would like to make a prediction regarding the course of physics in the next decade, extrapolating from our past experience of the decades gone by (See Table I).

B. Post-Planck physics, supergravity, and Einstein's dreams

I now turn to the problem of a deeper comprehension of the charge concept (the basis of gauging)—*which, in my humble view, is the real quest of particle physics.* Einstein, in the last 35 years of his life, lived with two dreams: one was to unite gravity with matter (the photon)—he wished to see the "base wood" (as he put it) which makes up the stress tensor $T_{\mu\nu}$ on the right-hand side of his equation $R_{\mu\nu}-\frac{1}{2}g_{\mu\nu}R=-T_{\mu\nu}$ transmuted through this union, into the "marble" of gravity on the left-hand side. The second (and the complementary) dream was to use this unification to comprehend the nature of electric charge in terms of space-time geometry in the same manner as he had successfully comprehended the nature of gravitational charge in terms of space-time curvature.

In case someone imagines[19] that such deeper comprehension is irrelevant to quantitative physics, let me adduce the tests of Einstein's theory versus the proposed modifications to it [Brans–Dicke (Brans and Dicke, 1961) for example], Recently (1976), the *strong* equivalence principle (i.e., the proposition that gravitational forces contribute equally to the inertial and the gravitational masses) was tested[20] to one part in 10^{12} [i.e., to the same accuracy as achieved in particle physics for $(g-2)_e$] through lunar-laser ranging measurements [Williams *et al.* (1976), Shapiro *et al.* (1976). For a discussion see Salam (1977)]. These measurements determined departures from Kepler equilibrium distances of the moon, the earth, and the sun to better than ±30 cm and triumphantly vindicated Einstein.

There have been four major developments in realizing Einstein's dreams:

(1) The Kaluza-Klein (Kaluza, 1921; Klein, 1926) miracle: An Einstein Lagrangian (scalar curvature) in five-dimensional space-time (where the fifth dimension is compactified in the sense of all fields being explicitly independent of the fifth coordinate) precisely reproduces the *Einstein-Maxwell* theory in four dimensions, the $g_{\mu 5}(\mu=0,1,2,3)$ components of the metric in five dimensions being identified with the Maxwell field A_μ. From this point of view, Maxwell's field is associated with the extra components of curvature implied by the (conceptual) existence of the fifth dimension.

(2) The second development is the recent realization by Cremmer, Scherk, Englert, Brout, Minkowski, and others that the compactification of the extra dimensions (Cremmer and Scherk, 1976a, b, c; Minkowski, 1977)—(their curling up to sizes perhaps smaller than Planck length $\lesssim 10^{-33}$ cm and the very high curvature associated with them)—might arise through a spontaneous symmetry breaking (in the first 10^{-43} sec) which reduced the higher-dimensional space-time effectively to the four-dimensional that we apprehend directly.

(3) So far we have considered Einstein's second dream, i.e., the unification of electromagnetism (and presumably of other gauge forces) with gravity, giving a space-time significance to gauge charges as corresponding to extended curvature in extra bosonic dimensions. A full realization of the first dream (unification of spinor matter with gravity and with other gauge fields) had to await the development of supergravity[21]—and an extension to extra fermionic dimensions of superspace (Salam and Strathdee, 1974a) (with extended torsion being brought into play in addition to curvature). I discuss this development later.

(4) And finally there was the alternative suggestion by Wheeler (Fuller and Wheeler, 1962; Wheeler, 1964) and Schemberg that electric charge may be associated with space-time topology—with worm-holes, with space-time Gruyère-cheesiness. This idea has re-

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[19] The following quotation from Einstein is relevant here. "We now realize, with special clarity, how much in error are those theorists who believe theory comes inductively from experience. Even the great Newton could not free himself from this error (*Hypotheses non fingo*)." This quote is complementary to the quotation from Einstein at the end of Sec. II.

[20] The *weak* equivalence principle (the proposition that all but the gravitational force contribute equally to the inertial and the gravitational masses) was verified by Eötvös to 1:10^8 and by Dicke and Braginsky and Panov to 1:10^{12}.

[21] See Freedman, van Nieuwenhuizen, and Ferrara (1976) and Deser and Zumino (1976). For a review and comprehensive list of references, see Freedman (1979). See also Arnowitt, Nath, and Zumino (1975), Zumino (1975), Wess and Zumino (1977), Akulov, Volkov, and Soroka (1975), and Brink *et al.* (1978).

cently been developed by Hawking[22] and his collaborators (Hawking, 1978, 1979a, b; Gibbons *et al.*, 1978).

C. Extended supergravity, SU(8) preons, and composite gauge fields

Now so far I have reviewed the developments in respect of Einstein's dreams as reported at the Stockholm Conference held in 1978 in this hall and organized by the Swedish Academy of Sciences.

A remarkable new development was reported during 1979 by Julia and Cremmer (Cremmer *et al.* (1978); Cremmer and Julia (1978, 1979); see also Julia (1979)] which started with an attempt to use the ideas of Kaluza and Klein to formulate extended supergravity theory in a higher (compactified) space-time—more precisely, in eleven dimensions. This development links up, as we shall see, with preons and composite Fermi fields—and even more important—possibly with the notion of composite gauge fields.

Recall that simple supergravity[23] is the gauge theory of supersymmetry[24]—the gauge particles being the (helicity ±2) gravitons and (helicity $\pm\frac{3}{2}$) gravitinos.[25] *Extended supergravity* gauges supersymmetry combined with SO(N) internal symmetry. For $N=8$, the (tribal) supergravity multiplet consists of the following SO(8) families.[26]

Helicity	± 2	1
	$\pm\frac{3}{2}$	8
	± 1	28
	$\pm\frac{1}{2}$	56
	0	70

As is well known, SO(8) is too small to contain $\mathrm{SU}(2) \times \mathrm{U}(1) \times \mathrm{SU}_c(3)$. Thus this tribe has no place for $W^{\pm}$ (though Z^0 and γ are contained) and no places for μ or τ or the t quark.

This was the situation last year. This year, Cremmer and Julia (see Footnote 26) attempted to write down the $N=8$ supergravity Lagrangian explicitly, using an extension of the Kaluza–Klein ansatz which states that *extended supergravity* [with SO(8) internal symmetry] has the same Lagrangian in four space-time dimensions as *simple supergravity* in (compactified) eleven dimensions. This formal—and rather formidable—ansatz, when carried through, yielded a most agreeable bonus. *The supergravity Lagrangian possesses an unsuspected SU(8) "local" internal symmetry* although one started with an internal SO(8) only.

The tantalizing questions which now arise are the following.

(1) Could this internal SU(8) be the symmetry group of the 8 preons (3 chromons, 2 flavons, 3 familons) introduced earlier?

(2) When SU(8) is gauged, there should be 63 spin-one fields. The supergravity tribe contains only 28 spin-one fundamental objects which are not minimally coupled. Are the 63 fields of SU(8) to be identified with composite gauge fields made up of the 70 spin-zero objects of the form $V^{-1}\partial_\mu V$? Do these composites propagate, in analogy with the well-known recent result in CP^{n-1} theories (D'Adda *et al.*, 1978), where a composite gauge field of this form propagates as a consequence of quantum effects (quantum completion)?

The entire development I have described—the unsuspected extension of SO(8) to SU(8) when extra compactified space-time dimensions are used, and the possible existence and quantum propagation of composite gauge fields—is of such crucial importance for the future prospects of gauge theories that one begins to wonder how much of the extrapolation which took $\mathrm{SU}(2) \times \mathrm{U}(1) \times \mathrm{SU}_c(3)$ into the electronuclear grand unified theories is likely to remain unaffected by these new ideas now unfolding.

But where in all this is the possibility to appeal directly to experiment? For grand unified theories, it was the proton decay. What is the analog for supergravity? Perhaps the spin-$\frac{3}{2}$ massive gravitino, picking its mass from a super-Higgs effect [Cremmer *et al.* (1979); see also Ferrara (1979) and references therein] provides the answer. Fayet (1977, 1979) has shown that for a spontaneously broken globally supersymmetric weak theory the introduction of a local gravitational interaction leads to a super-Higgs effect. Assuming that supersymmetry breakdown is at mass scale m_W, the gravitino acquires a mass and an effective interaction, but of conventional weak rather than of the gravitational strength—an enhancement by a factor of 10^{34}. One may thus search for the gravitino among the neutral decay modes of J/ψ—the predicted rate being 10^{-3}–10^{-5} times smaller than the observed rate for $J/\psi \to e^+e^-$. This will surely tax all the ingenuity of the great men (and women) at SLAC and DESY. Another effect suggested by Scherk (1979) is antigravity—a cancellation of the attractive gravitational force with the force produced by spin-one gravi-photons which exist in all extended supergravity theories. Scherk shows that the Compton wavelength of the gravi-photon is either smaller than 5 cm or is between 10 and 850 meters in order that there will be no conflict with what

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[22] The Einstein Lagrangian allows large fluctuations of metric and topology on Planck-length scale. Hawking has surmised that the dominant contributions to the path integral of quantum gravity come from metrics which carry one unit of topology per Planck volume. On account of the intimate connection (de Rham, Atiyah-Singer) (Atiyah and Singer, 1963) of curvature with the measures of space-time topology (Euler number, Pontryagin number) the extended Kaluza-Klein and Wheeler-Hawking points of view may find consonance after all.

[23] See Freedman, van Nieuwenhuizen, and Ferrara (1976), and Deser and Zumino (1976). For a review and comprehensive list of references, see Freedman (1979).

[24] See Gol'fand and Likhtman (1971), Volkov and Akulov (1972), Wess and Zumino (1974), Salam and Strathdee (1974a,b,c). For a review, see Salam and Strathdee (1978).

[25] Supersymmetry algebra extends Poincaré group algebra by adjoining to it supersymmetric charges Q_α which transform bosons to fermions. $\{Q_\alpha, Q_\beta\} = (\gamma_\mu P_\mu)_{\alpha\beta}$. The currents which correspond to these charges (Q_α and P_μ) are $J_{\mu\alpha}$ and $T_{\mu\nu}$—these are essentially the currents which in gauged supersymmetry (i.e., supergravity) couple to the gravitino and the graviton, respectively.

[26] See Footnote 23 and Cremmer, Julia, and Scherk (1978) and Cremmer and Julia (1978, 1979). See also Julia (1979).

TABLE A.I. Examples of grand unifying groups.

Semisimple groups[27]	Multiplet	Exotic gauge particles	Proton decay
(with left-right symmetry)	$G_L \to \begin{pmatrix} q \\ l \end{pmatrix}_L, \quad G_R \to \begin{pmatrix} q \\ l \end{pmatrix}_R$	Lepto-quarks → $(\bar{q}l)$	Lepto-quarks → W + (Higgs) or
Example $[SU(6)_F \times SU(6)_c]_{L\to R}$	$G = G_L \times G_R$	Unifying mass $\approx 10^6$ GeV	Proton $= qqq \to lll$
Examples Family groups → SU(5) or SO(10) ↓ ↓ Tribal groups → SU(11) SO(22)	$G \to \begin{pmatrix} q \\ l \\ \bar{q} \\ \bar{l} \end{pmatrix}_L$	Diquarks → (qq) Dileptons → (ll) Lepto-quarks → $(\bar{q}l), (ql)$ Unifying mass $\approx 10^{13} - 10^{15}$ GeV	$qq \to \bar{q}l$ i.e. Proton $P = qqq \to \bar{l}$ Also possible, $P \to l, P \to 3\bar{l}, P \to 3l$

is presently known about the strength of the gravitational force.

Let me summarize: it is conceivable of course, that there is indeed a grand plateau—extending even to Planck energies. If so, the only eventual laboratory for particle physics will be the early universe, where we shall have to seek for the answers to the questions on the nature of charge. There may, however, be indications of a next level of structure around 10 TeV; there are also beautiful ideas (like, for example, those of electric and magnetic monopole duality) which may manifest at energies of the order of $\alpha^{-1}m_W (= 10\text{ TeV})$. Whether even this level of structure will give us the final clues to the nature of charge, one cannot predict. 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, as I did at the 1978 Stockholm Conference, 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" (J. R. Oppenheimer, Reith Lectures, BBC, 1953).

APPENDIX A: EXAMPLES OF GRAND UNIFYING GROUPS

Appendix A is contained in Table A.1: Examples of grand unifying groups.

APPENDIX B: DOES THE GRAND PLATEAU REALLY EXIST

The following assumptions went into the derivation of the formula (*I*) in the text.

(a) $SU_L(2) \times U_{L,R}(1)$ survives intact as the electroweak symmetry group from energies $\approx \mu$ right up to M. This intact survival implies that one eschews, for example, all suggestions that (i) low-energy $SU_L(2)$ may be the diagonal sum of $SU_L^{I}(2), SU_L^{II}(2), SU_L^{III}(2)$, where I, II, III, refer to the (three?) known families; (ii) or that the $U_{L,R}(1)$ is a sum of pieces, where $U_R(1)$ may have differentially descended from a $(V+A)$-symmetric $SU_R(2)$ contained in G, or (iii) that U(1) contains a piece from a four-color symmetry $SU_c(4)$ (with lepton number as the fourth color) and with $SU_c(4)$ breaking at an intermediate mass scale to $SU_c(3) \times U_c(1)$.

(b) The second assumption which goes into the derivation of the formula above is that there are no unexpected heavy fundamental fermions, which might make $\sin^2\theta(M)$ differ from $\frac{3}{8}$—its value for the low mass fermions presently known to exist.[28]

(c) If these assumptions are relaxed, for example, for the three family group $G = [SU_F(6) \times SU_c(6)]_{L\to R}$, where $\sin^2\theta(M) = \frac{9}{28}$, we find the grand unifying mass M tumbles down to 10^6 GeV.

(d) The introduction of intermediate mass scales [for example, those connoting the breakdown of family universality, or of left-right symmetry, or of a breakdown of 4-color $SU_c(4)$ down to $SU_c(3) \times U_c(1)$] will as a rule push the magnitude of the grand unifying mass M upwards [see Salam (1979) and Shafi and Wetterich (1979)]. In order to secure a proton decay life, consonant with present empirical lower limits ($\sim 10^{30}$ years) (Learned *et al.*, 1979) this is desirable anyway. (τ_{proton} for $M \sim 10^{13}$ GeV is unacceptably low $\sim 6 \times 10^{23}$

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[27] Grouping quarks (q) and leptons (l) together implies treating lepton number as the fourth color, i.e., $SU_c(3)$ extends to $SU_c(4)$ (Pati and Salam, 1974). A tribal group, by definition, contains all known families in its basic representation. Favored representations of tribal SU(11) (Georgi, 1979) and tribal SO(22) [Gell-Mann (1979) *et al.*] contain **561** and **2048** fermions!

[28] If one does not know G, one way to infer the parameter $\sin^2\theta(M)$ is from the formula:

$$\sin^2\theta(M) = \frac{\Sigma T_{3L}^2}{\Sigma Q^2} = \left[\frac{9N_q + 3N_l}{20N_q + 12N_l}\right].$$

Here N_q and N_l are the numbers of the fundamental quark and lepton SU(2) doublets (assuming these are the only multiplets that exist). If we make the further *assumption* that $N_q = N_l$ (from the requirement of anomaly cancellation between quarks and leptons) we obtain $\sin^2\theta(M) = \frac{3}{8}$. This assumption, however, is not compulsive; for example, anomalies cancel also if (heavy) mirror fermions exist (Pati *et al.*, 1975b; Pati and Salam, 1975b,c; Pati, 1975). This is the case for $[SU(6)]^4$ for which $\sin^2\theta(M) = 9/28$.

years unless there are 15 Higgs.) There is, from this point of view, an indication of there being in particle physics one or several intermediate mass scales which possibly start from around 10^4 GeV upwards. *This is the end result which I wished this Appendix to lead up to.*

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