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INTERNATIONAL CENTRE FOR THEORETICAL PHYSICS

OVERVIEW OF PARTICLE PHYSICS

Abdus Salam

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International Atomic Energy Agency
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I INTRODUCTION

I have been asked to give an overview of the situation of Particle Physics at the end of 1985.

Before I do this, however, let me say that physics is an incredibly rich discipline: it not only provides us with the basic understanding of the Laws of Nature, it also is the basis of most of modern high technology. This remark is relevant to our developing countries. A fine example of this synthesis of a basic understanding of Nature with high technology is provided by liquid-crystal physics which was worked out at Bangalore by Professor S. Chandrasekhar and his group. In this context, one may note that because of this connection with high technology and materials' exploitation, physics is the "science of wealth creation" par excellence. This is even in contrast to chemistry and biology which - though as important for development - are "survival sciences". This is in the sense that chemistry in application is concerned with fertilisers, pesticides, etc., while biology is concerned with medical sciences. Thus, together, chemistry and medical sciences, provide the survival basis of food production as well as of pharmaceutical expertise. Physics takes over at the next level of sophistication. If a nation wants to become wealthy, it must acquire a high degree of expertise in physics, both pure and applied.

II OVERVIEW OF PARTICLE PHYSICS

In the past, Particle Physics was driven by a troika which consisted of (1) Theory, (2) Experiment, and (3) Acccelerator and Detection-Devices technology. To this troika have been added two more horses. Particle Physics is now synonymous with (4) Early Cosmology (from 10^{-43} sec. upto the end of the first three minutes of the Universe's life) and it is strongly interacting with (5) Pure Mathematics. One may recall Res Jost who made the statement (towards the end of the 1950's) that all the mathematics which a particle physicist needed to know was a rudimentary knowledge of Latin and Greek alphabets so that one can populate ones' equations with indices. No more now.

The situation in this regard has changed so drastically that now a theoretical particle physicist must know algebraic geometry, topology, Riemann surface theory, index theorems and the like. More mathematics that one knows, the deeper the insights that one may aspire for.

In the last decade or so, in particle physics, we are experiencing an age of great syntheses and of great vitality. At the same time, this is an age of great danger for the future of the subject in the sense that we need higher and higher accelerator energies, and more costly non-accelerator and passive experiments which take a higher injection of funds as well as of experimentation time, for discovering new phenomena or for testing the truth or the inadequacy of theoretical concepts. This is in contrast to the time when I started research (late forties and early fifties) when we had ever-increasing quantities of undigested experimental data, but little coherent theoretical corpus of concepts.

III THREE TYPES OF IDEAS

I shall divide my remarks into three topics: A) Ideas which have been tested or will soon be tested with the accelerators which are presently being constructed; B) Theoretical ideas whose time has not yet come, so far as the availability of accelerators to test them goes; and C) Passive experiments which have tested - but not conclusively so far - some of the theories of the 1970's. To give a brief summary of what I want to dwell on, consider each of these three topics:

A) Ideas which have been tested or will soon be tested. These include (i) the standard model based on the symmetry group $SU_c(3) \times SU_L(2) \times U(1)$, with which there is no discrepancy known at the present time. (ii) Light Higgs which may be discovered at SLC after 1987 or at LEP after 1989. (iii) Preons of which quarks may be made up. Because of the low momentum transfers involved, it is very unlikely, but preons may be discovered at HERA (after 1991) and may fetch a totally new slant to bear on the family problem, and on the problem of quark elementarity.

B) Theoretical ideas whose time has not yet come; basically because no accelerators are being constructed to test them. These ideas include (i) $N = 1$ supersymmetry and $N = 1$ supergravity - the lower limit for supersymmetric partners for presently known particles appears to be rising and may now be as large as 50 GeV. Persuasive theoretical arguments would lead us to expect that such supersymmetric partners of quarks and leptons may exist below 1 TeV. To find these, we shall need LHC (large hadron collider in the LEP tunnel), or SSC (super conducting supercollider being considered in the USA), or an e^+e^- collider with centre of mass energy in the TeV range. For any of these, there has been no sanction from the European or US Governments. If at all, such accelerators may not arrive before the years 1995 - 2000.

Other related ideas in this category which also need higher energies are (ii) Right-handed weak currents, (iii) Extended supergravities, and (iv) Super string ideas. I shall discuss these later.

C) The set of ideas for which non-accelerator and passive experiments have been mounted, are mostly concerned with grand unification of electroweak and strong forces in its multifarious ramifications. These include (i) proton decays, (ii) $n\bar{n}$ oscillations, ($\tau_{n\bar{n}} > 1 \times 10^6$ sec. 90% C.L.) (iii) neutrino masses and oscillations, (iv) monopoles, (v) dark matter and the like. A number of these experiments have been tried, but not with much success so far.

Let us now turn to each of these topics in turn.

IV IDEAS WHICH HAVE BEEN TESTED OR WILL SOON BE TESTED

Since in this context, we shall be concerned with the early availability of accelerators, I shall start with Table I which gives a list of already existing, soon to be commissioned, as well as the proposed accelerators.

TABLE I

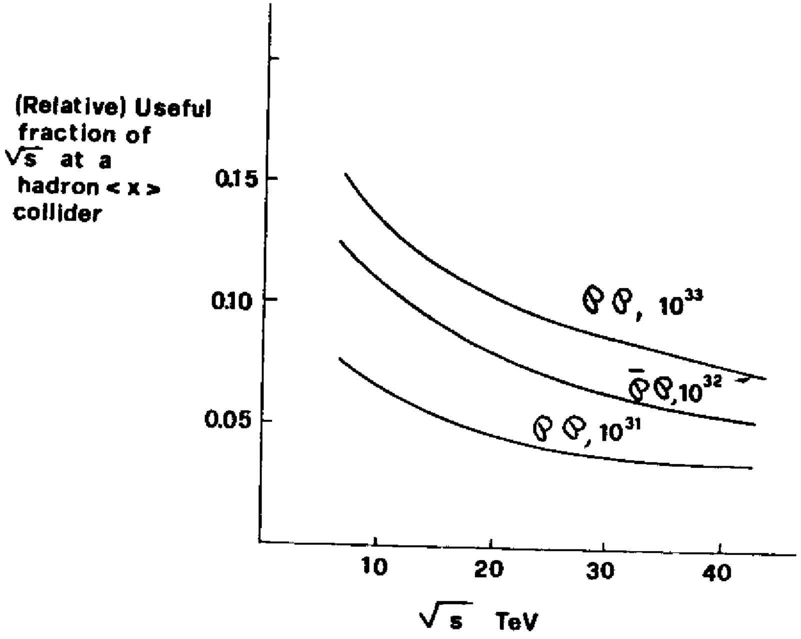
ACCELERATORS IN THE FORESEEABLE FUTURE

Year	Machine	√s (GeV)	Constituent √s (peak-Max, GeV)	Luminosity	Locality
1986	SppS	900	100 - $200_{qq,q\bar{q}}$	> 10^{30}	CERN
1986	Tevatron	2000	200 - $600_{qq,q\bar{q}}$	< 10^{31}	FERMILAB
1987	TRISTAN (e^+e^-)	60	60	< 10^{32}	Japan
1987	SLC (e^+e^-)	100	100	10^{30}	Stanford
1987	Bepc (e^+e^-)	4	4		Beijing
1989	LEP (I) (e^+e^-)	100	100	10^{31}	CERN
?	LEP (II) (e^+e^-)	200	200	10^{31}	CERN
1990	UNK	3000	300 - $900_{qq,q\bar{q}}$		Serpukhov
1991	HERA (eq)	320	100 - 170	>10^{31}	Hamburg
?	LHC	8000-16,000	2000		CERN
?	SSC	40,000	4000	10^{33}	USA
?	e^+e^-	4000 GeV	4000 GeV	10^{33}	?

Note the important role of luminosity in Figures 1 and 2 due to G.L. Kane, which exhibit the windows for discovering heavy quarks or leptoquarks and which show that the construction of the SSC with its higher luminosity (as well as higher energy) is imperative.

Figure 1

Windows for given mass (m), defined to detect the effect barely, not study it.

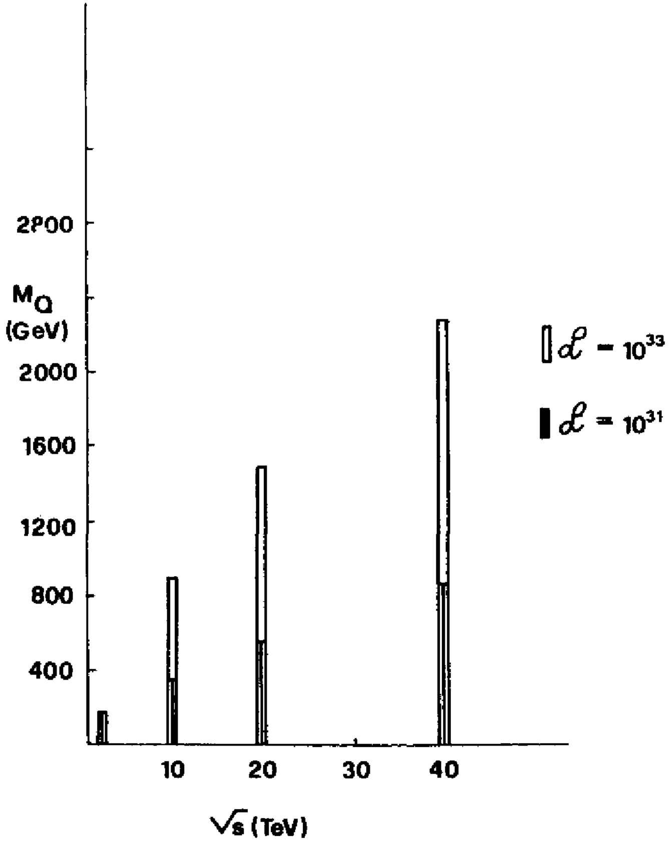


$$\langle x\rangle = 1/5 \sum_{j,Q,W^+W^-,H,\tilde{g}} (m/\sqrt{s})$$

Compare energy, luminosity and beams;
Could use 2m, or geometric mean, so absolute size of <x> is somewhat arbitrary.)

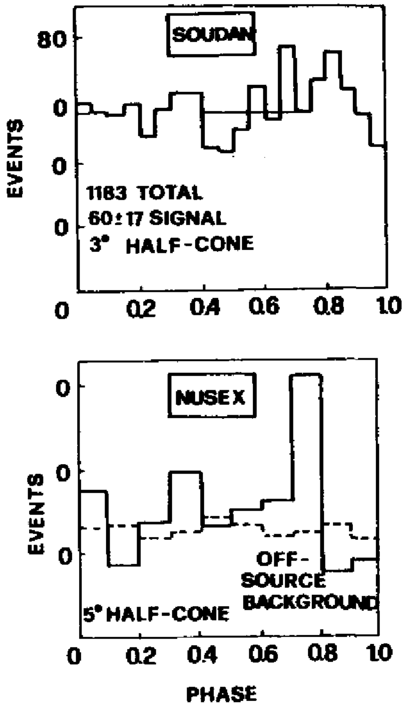
Figure 2

Windows for heavy quarks, squarks or leptoquarks.



While we are discussing the availability of future accelerators, let me make the following remark. The highest electric field gradients (which determine the size of an accelerator) achievable with to-day's technology, are no higher than 1/10 GV per metre. Twenty years hence, when we may have perfected the technology of laser beat-wave plasma accelerators, this gradient may go up by a factor of 1000 - i.e. 1/10 TV per metre. This may mean that a 30 Km accelerator may produce $\sqrt{s} = 10^4$ TeV. An accelerator circling the moon may generate 10^6 TeV. (This was suggested by Arthur C. Clarke).

Figure 3



Phase structure of Cygnus X3 underground muon signals.

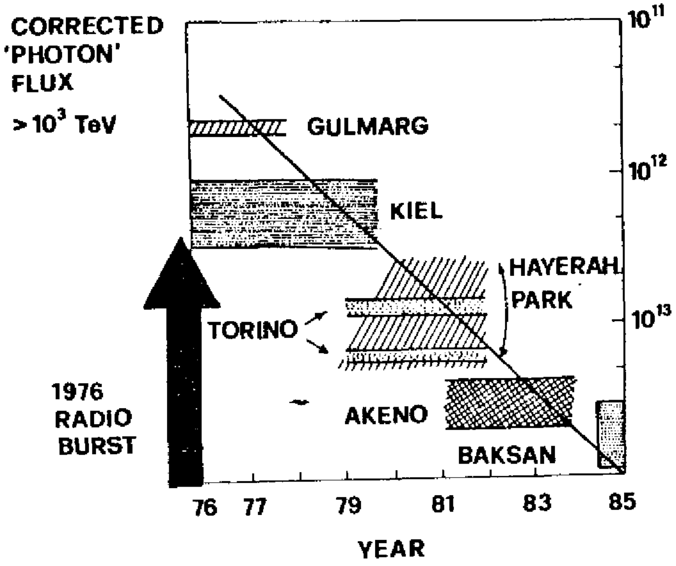
An accelerator circling the earth, as Fermi once conceived, may be capable of $\sqrt{s} \approx 10^7$ TeV, while an accelerator extending from earth to the sun would be capable of $\sqrt{s} \approx 10^{11}$ TeV. In the same crazy strain, for an accelerator to be capable of generating $\sqrt{s} = 10^{16}$ TeV (the theoretically favoured, Planck mass m_p) one would need 10 light years! Clearly one must eventually fall back on the highest energy cosmic rays - to study, for example, the likes of the recently discovered high energy muon signals in the Nusex (Mont Blanc) and Soudan I experiments. These muons (produced in the atmosphere), can apparently be traced back to a cosmological accelerator associated with Cygnus X3 - an X-ray source discovered in 1966, some 37 thousand light years distant from us, which has a duty cycle of 4.8 hours and an integrated luminosity of 10^5 suns.

From the muon signals, these recent Nusex and Soudan experiments have claimed that Cygnus X-3 is beaming to us radiation of a new kind (light photinos, neutral light quark nuggets?) of energy 10^4 TeV. (I shall not discuss here why most of the familiar particles are ruled out.)

If this experimental evidence is taken at its face value, how is the radiation beamed at us by Cygnus X3 generated? One speculative idea is that the Cygnus system may consist of a binary - a conventional main sequence star and a pulsar or a black-hole. Matter from the coventional star accretes around the compact pulsar or the black hole, forming a disc. The protons thus accelerated (up to maximum energies of $\approx 10^5$ TeV) go into a beam dump, where is created the mysterious radiation, which hits our atmosphere and makes the observed muons.

One interesting aspect of the situation is that (as warned by Halzen at the Bari Conference in Summer 1985), the Swan may be dying (Figure 4) - the emitted flux seems to be decreasing at the rate of a decade over three years, "much as if the beam dump was being blown away".

Figure 4



Time-dependence of the photon flux from Cygnus X3

Are there likely to be more intense, more energetic sources than Cygnus X-3 in the sky? Will cosmology come to the rescue of experimental particle physics?

V THE STANDARD MODEL AND THE LIGHT HIGGS

The standard model of to-day's particle physics describes three families of quarks and leptons. The first family consists of (u_L, d_L) and (u_R, d_R) quarks; each quark comes in three colours: red, yellow and blue. There are, in addition, 3 leptons, (e_L, ν_L) and e_R. Thus this family has 12 quarks and 3 leptons (i.e. 15 two-component objects).

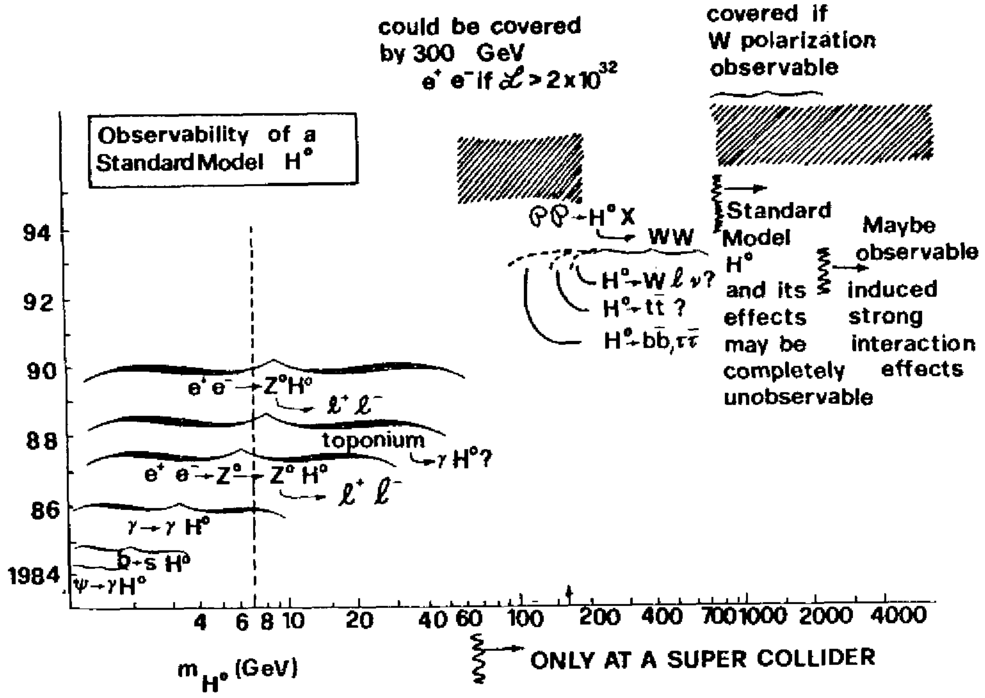
The second family has charm and strange quarks (replacing the up and down quarks) while the electron and its neutrino are replaced by the muon and its neutrino. Like the first family, there 15 two-component objects. The third family likewise consists of top and bottom quarks plus the tauon and its neutrino.

In addition to these 45 spin 1/2 two-component objects there are the 12 Yang-Mills-Shaw gauge spin 1 mediators corresponding to the symmetry $SU_c(3) \times SU_L(2) \times U(1)$ – the photon γ, $W^\pm$, Z^0 and 8 gluons. Of these, nine particles (γ and eight gluons) are massless. In addition, there should be one physical spin zero Higgs H^0 giving a total of 118 degrees of freedom for the particles in the standard model. All particles except the Higgs in this list have been discovered and their masses and spins determined (though the top-quark is still disputed). In this context it is worth remarking that CERN data on $Sp\bar{p}S$, has confirmed the theoretical tree-level expectation of $W^\pm$, Z^0 masses to within 1%.

So far as the Higgs particle is concerned, theory does not give its mass. Defining a light Higgs as an object with a mass < 300 GeV and a heavy Higgs as an object with a mass beyond, upto 1 TeV, one may remark that a heavy Higgs would have a large width. Thus the concept of a particle for it would be lost. (In case the Higgs is heavy, the W and Z would interact strongly. One would expect a new spectroscopy of bound states and Regge trajectories, which may include spin 1 resonances and which would modify the properties of W and Z. No one likes this possibility, but it could happen.)

Figure 5 (due to G. Kane) shows the possible signals of the standard model Higgs. As one can see, beyond a mass of 60 GeV, one would need LEP II and eventually the SSC supercollider.

Figure 5



One of the measurements which has been carried out during 1985, relevant to the number of families in the standard model, is the estimate of the number of light neutrinos which may couple to the Z^0 particle. THis number has been estimated from the collider measurements (on Z^0 width) to be $\leqslant 5.4 \pm 1$ - amazingly consistent with the number 3 or 4 which cosmological data would appear to favour. This cosmological data is deduced from He^3 and He^4 abundances. Re-examining this data (and taking into account its errors) Ellis et al. have suggested that cosmology may even be consistent with 5 or 6 light neutrinos (three light ν_R in addition to $\nu_L\uparrow$). No longer can one say with Landau "Cosmologists are seldom right, but never in doubt.". They could be right this time!

An important set of experiments which would be carried out at SLC and at the LEP accelerator concern the radiative corrections to the tree level predictions of the standard model in the electroweak sector.

As an example, in the next Table II are presented data due to Lynn, Peskin, Stuart and Verzegnassi relevant to these radiative corrections.

TABLE II

B. Lynn, M. Peskin, R. Stuart SLAC-PUB-3725

One-Loop Physics	$\delta A_{LR} = \delta A_{rpol}$	δA^{FB}	δA_I	δM^W(MEV)
GSW Weak				
$m_t = 30$	-0.03	-0.01	.005	-180
$M_H = 100$	-			
Heavy Top Quark				
$m_t = 180$ GeV	0.03	0.0075	0.004	780
Heavy Higgs = 1 TeV	-0.01	-0.0045	-0.003	-160
Heavy Quark Pair				
a) Large I Splitting	0.02	0.01	0.007	300
b) Degenerate	-0.004	-0.002	-0.004	-42
Heavy Lepton Pair				
a) Large I Splitting m = 0		0.012		0.006
0.004	300			
b) Degenerate	-0.0013	-0.0006	-0.0004	-14
Heavy Squark Pair				
a) Large I Splitting	0.02	0.01	0.007	300
b) Degenerate	0	0	0	0
Heavy Slepton Pair				
a) Large I Splitting	0.012	0.006-	0.004	300
b) Degenerate	0	0	0	0
Winos				
a) $m_{3/2} < 100$ GeV	0.005	0.0025	0.001	100
b) $m_{3/2} > 100$ GeV	<0.001	<0.001	<0.001	<10
Technicolor				
$SU_8 \times SU_8$	-0.04	-0.018	-0.012	-500
O_{16}	-0.07	-0.032	-0.021	-500
Strong Interaction Uncertainty	±.0033	±.0016	+.001	±25 MeV

Assuming that Z^0 mass will be measured with extreme accuracy at SLC or LEP (up to 50 MeV or possibly better), the parameters of the standard model could be chosen as α , G_F, m_Z) and (m_t and m_H). One can thus propose clean tests of the electroweak theory at the one loop level. These could consist of measurements of longitudinal polarization, measurement of W mass (LEP 2), and measurement of neutrino $\sigma(\nu e)/\sigma(\bar{\nu}e)$ ratios (charm 2) to one loop level.

Consider the case of the longitudinal polarization in A_{LR} $= \dfrac{\sigma_{e^+e^-_L} - \sigma_{e^+e^-_R}}{\sigma_{e^+e^-_L} + \sigma_{e^+e^-_R}} \rightarrow \mu\bar{\mu}$. On top of the Z^0 resonance, the full one loop prediction is $A_{LR} = -.03$ for $m_H = 100$ GeV, $m_t = 30$ GeV. A (new) heavy quark pair would contribute +.02, a heavy scalar lepton pair another +.012 and so on. Thus one may hope to determine from the comparative measurements of δA_{LR} , δM_W etc., the top quark mass or the Higgs mass or the existence of new heavy quark pairs etc. in an indirect fashion.

VI IDEAS WHOSE TIME HAS NOT YET COME

(A) The most important ideas of this category are of N = 1 supersymmetry and N = 1 supergravity. N = 1 supersymmetry is the hypothetical symmetry (between fermion and bosons) which decrees that a spin 1/2 must be accompanied by two spin zeros: a spin one gauge particle must be accompanied by a massless spin 1/2 particle (gaugino): a massless spin 2 graviton must be accompanied by a massless spin 3/2 gravitino, and so forth. (For N = 2 extended supersymmetry, one would group in one multiplet, two spin 0, two spin 1/2's and one spin 1 object. For the maximal N = 8 extended supersymmetry, there is just one multiplet containing one spin 2, accompanied by 8 spin 3/2's, 28 spin 1, 56 spin 1/2 and 70 spin 0 states.)

Supersymmetry is an incredibly beautiful theory - a compelling theory if there is one, even though there is no physical evidence of the existence of supersymmetry partners to the known particles, upto 30 GeV (or even perhaps upto 50 GeV).

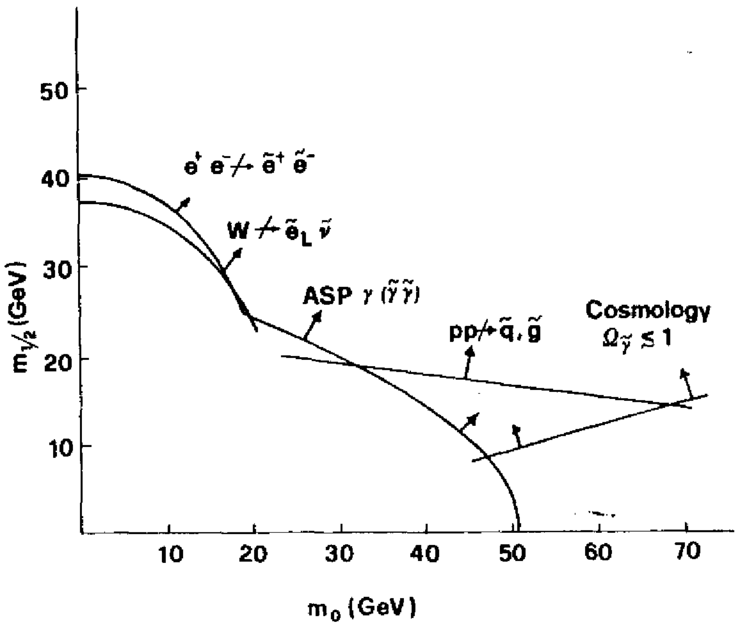
One aspect of its compellingness lies in its superior renormalisability properties and the possibility which these open up of understanding why the large numbers which occur in particle physics could be "naturally" stable.

Consider as an example the large number $m_P/m_W \approx 10^{17}$ where m_P is Planck mass. (Planck mass occurs in gravity theories: large numbers similar to m_P/m_W can however occur in all grand unification theories which synthesise electroweak with strong forces.) Only in supersymmetric theories can one show that such a number, *once fixed at the tree level*, would be unaffected by radiative corrections. This is one of the virtues of supersymmetric theories.

But supersymmetry must be a highly broken symmetry. What is the supersymmetry breaking mass? Or more physically, where do the missing supersymmetry partners of quarks, leptons, photons, $W^{\pm}$ and Z^0 lie? The theoretical expectation seems to be BELOW a (flexible) upper limit of 1 TeV, *if supersymmetry is relevant to the electro-weak phenomena*. (To to one loop for example, $\delta\ m_H^2 = \alpha/\pi\ |\ m_F^2 - m_B^2\ |$; if $\delta\ m_H^2 \simeq m_W^2$, we get the estimate that $|\ m_F - m_B\ | \lesssim 1$ TeV.)

What is the mass limit at present, of supersymmetry partners not having been found? Estimates vary, but for a conservative recent (*model-dependent*) estimate, see Figure 6 presented by J. Ellis at the Kyoto Conference (September 1985).

Figure 6



Compilation of experimental constraints of SUSY breaking parameters.

Here m_0 and $m_{1/2}$ are two supersymmetric parameters in terms of which $m_{\tilde{q}}$, $m_{\tilde{l}}$, $m_{\tilde{W}}$, $m_{\tilde{Z}}$ etc. can be parameterised. (The symbol ~ denotes a supersymmetric partner.) For example, $m_{\tilde{q}}^2 \simeq m_0^2 + 7m_{1/2}^2$, $m_{\tilde{l}}^2 \simeq m_0^2 + .5m_{1/2}^2$, $m_{\tilde{g}} \simeq 3m_{1/2} \simeq 7m_{\tilde{\gamma}}$. Ellis has examined all relevant data and concluded that possibly the lower limit for supersymmetric partners may be as high as 50 GeV.

To conclude, it is expected that supersymmetry may make itself manifest with highly luminous accelerators with centre of mass energy in excess of 1 TeV (e.g. *LHC, SSC or an e^+e^- linear collider of > 1 TeV*). Supersymmetry may manifest itself at lower energies as an indirect *phenomena*, (*supersymmetry* was claimed with monojets, dijets, trijets at UA1, but the present backgrounds happen to be too large to draw unambiguous conclusions).

(B) SUPERSYMMETRY AND N = 1 SUPERGRAVITY

Note the following points:

1) The N = 1 supersymmetrisation of the standard model will need two multiplets of Higgs particles (plus, of course, their Higgsinos).

2) The Signature of supersymmetry is the R quantumm number which is defined as +1 for all known particles and -1 for their supersymmetric partners. Thus these new partners must be produced in pairs, and among the expected supersymmetry particles there must be a lowest mass stable object which must be neutral in order to survive the Big Bang. Further, it must be weakly coupled otherwise it will be concentrated in condensed form in the galaxies. The favourite candidates for this object are scalar neutrinos $\tilde{\nu}$, photinos $\tilde{\gamma}$ or gravitinos - the spin3/2 partners of the gravitons.

3) If N = 1 supersymmetry comes, N = 1 supergravity cannot be far behind. The argument goes as follows: the major theoretical question regarding supersymmetry is supersymmetry breaking. The only decent known way to break supersymmetry is to break it spontaneously. For this to work, one starts with a gauge theory of supersymmetry - i.e. a supergravity theory which (for the N = 1 case), would contain spin 3/2 gravitinos in addition to spin 2 gravitons. One would then postulate a super-Higgs effect - i.e. a spin 1/2 and spin zero matter multiplet (of "shadow" matter which interacts with known particles only gravitationally). The spin 1/2 member of this multiplet would be swallowed by the spin 3/2 gravitino - the latter becoming massive in the classic Higgs fashion to break supersymmetry spontaneously. The $(\text{mass})^2$ of the gravitino - in analogy with the standard Higgs effect - would be of the order of the gravity coupling parameter $(1/m_P^2)$ times the expectation value of the supersymmetry breaking potential $(m_{3/2}^2 \simeq 1/m_P^2 < 0 \mid V \mid 0)$.

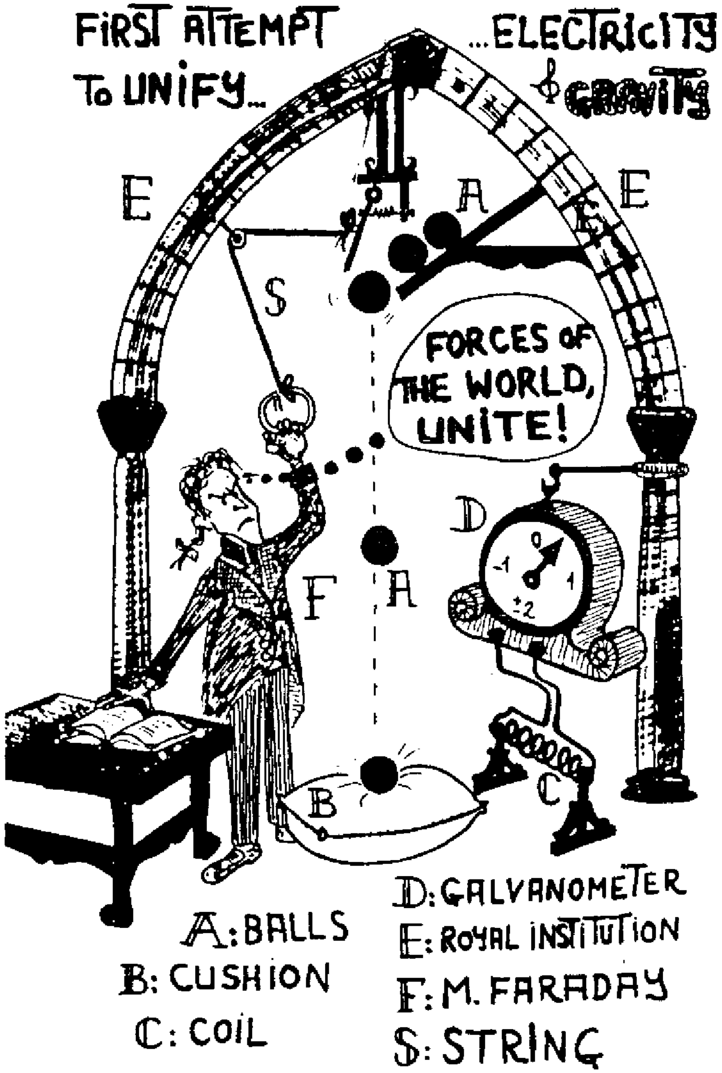
4) What could be the numerical estimates for the supersymmetry breaking parameter $(0 \mid V \mid 0)$. Chamseddine et al., Weinberg, and Nilles et al. and others noted (1982-1983) that with the reasonable requirement of a vanishing

cosmological constant, one finds that this (spontanously broken $N = 1$) supergravitisation of the (supersymmetric) standard model automatically leads to a breaking of $SU_L(2) \times U(1)$. Thus, $m_{3/2} \simeq m_W$ is motivated, and with this one may estimate, $(0 \mid V \mid 0)^{1/4} \simeq 10^{10}$ GeV. (An alternative no scale model which generates automatically a zero cosmological constant is the favourite at CERN. Here $m_{3/2}$ is not tied to m_W.)

C) UNIFICATION OF GRAVITY WITH OTHER FORCES

So far we considered ($N = 1$) supergravity as following on the heels of ($N = 1$) supersymmetry in order to provide for an orderly breaking of supersymmetry - there was no true unification of gravity with other forces. Let us now discuss a true unification of gravity with the rest of particle physics.

The first physicist to conceive of this and to try to find experimental evidence of such a phenomenon was Michael Faraday. In a symbolic drawing - due to A. de Rujula - one may see the equipment which Faraday used at the Royal Institution in Piccadilly, London (and which is on exhibition even to-day). The failure of his attempts did not dismay Faraday. He wrote afterwards: "If the hope should prove well founded, how great and mighty and sublime in its hitherto unchangeable character is the force I am trying to deal with, and how large may be the new domain of knowledge that may be opened to the mind of man."



The first semi-successful theoretical attempt (in 1920's) to unify gravity with electromagnetism was that of Kaluza and Klein who showed in a theory based on a 5 dimensional space-time, that the appropriate curvature component in 5th dimension, corresponds to electromagnetism. Further, if the fifth dimension is compactified to a scale R, and charged matter is introduced, one can show that α and G must be related as $\alpha = G/R^2$. These ideas were beautifully generalised in an extended supergravity context, when Cremmer and Julia discovered in 1979 that the extended $N = 8$ supergravity in 4 dimensions emerges as the zero mass limit of the compactified $N = 1$ supergravity in 11 dimensions. An incredible achievement. Since 1979, all supergravitors have lived in higher dimensions.

At that time, this theory was hailed as the first T.O.E. (Theory of Everything). The compactification of $d = 11$ Kaluza-Klein supergravity down to four dimension would give as its zero mass sector, gravitons as well as gauge particles like spin one γ , $W^{\pm}$ and Z as well as the 56 fermions in one multiplet of $N = 8$ supergravity. Unfortunately, the $N = 8$ theory and this particular multiplet suffered from two fatal defects: the fermions were not chiral and the theory did not have the content of the standard model so far as quarks, leptons or even $W^{\pm}$ were concerned. In addition to the zero mass sector, there would, of course, be higher Planck mass particles $((\text{mass})^2 =$ multiples of $1/R^2$ - the so-called pyrgons, providing an embarrassment of riches.

Can one ever obtain direct evidence for the existence of higher dimensions? The answer is, possibly yes. If the extra dimensions happen to be compactified to-day, why should they remain so? Why should they not share the Universal expansion? Could $\dot{R} \neq 0$? Since α , G and R are expected to be related to each other. If we are fortunate and if $\dot{\alpha}/\alpha$ and/or $\dot{G}/G$ should turn out to be non-zero at the present experimental level, this might most simply be explained by postulating extra dimensions. At present the experimental limits are less than 1×10^{17} years^{-1} for $\dot{\alpha} / \alpha$ while $\dot{G}/G$ is less than 1×10^{11} years^{-1}.

D) ANOMALY-FREE SUPERGRAVITIES

Where do we stand to-day so far as higher dimensions and extended supergravity theories are concerned? It would appear that the only theories which may combine chiral fermions and gravity are $N = 1$ in $d = 10$ dimensions or $N = 2$ in $d = 6$ or 10. In order that such theories contain the known chiral quarks and leptons as well as the W's and Z and photons and gluons the most promising is the $N = 1$, $d = 10$ supergravity, but it would have to be supplemented with a supersymmetric Yang Mills multiplet of matter in addition to the supergravity multiplet. Likewise, the $N = 2$ theory in $d = 6$ would need, not only an extra Yang Mills field, but also non linearly realised (σ-model) matter fields. Thus a pure Kaluza-Klein supergravity will never be enough. Higher dimensions, yes; but to generate the known gauge theories of electroweak and strong forces, we need (higher dimensional) super-Yang-Mills in addition.

As if this was not trouble enough, both $d = 6$ or $d = 10$ theories were shown to be anomalous and gravitationally replete with infinities. This impasse was broken only in Autumn 1984 by Green and Schwarz who showed that $d = 10$ supergravity with Yang Mills SO (32) or E8 x E8 could be made anomaly-free by an addition of certain numbers of new terms. They further showed that these additional terms were already present in the spinning supersymmetric string theories in ten dimensions, invented earlier. (Whether these string theories were free of gravity infinities beyond one loop was (and still is) an open question.) And this brings us to the new world of superstrings and the new version of A THEORY OF EVERYTHING (T.O.E.).

E) SPINNING SUPERSYMMETRIC STRINGS

Consistent, Lorentz-covariant and conformally symmetric string theories had been written down in 2, 10 or 26 dimensions, (the last being relevant only to bosonic strings) already in the 1970's. A closed string is a loop which replaces a space-time point. Its quantum oscillations correspond to particles of higher-spins and higher masses, which may be strung on a linear trajectory in a Regge spin-versus-mass^2 plot. If the slope parameter of this trajectory – the only parameter in the theory – is adjusted to equal Newtonian constant, one can show – quite miraculously – that in the zeroeth order of the closed bosonic string, there emerges, from the string theory, Einstein's gravity in its fullness! (The higher orders give (Planck)$^{-1}$ range corrections.)

Furthermore, the supersymmetric 10 dimensional spinning string theory (descended from 26 dimensions) could exist in a "heterotic" form, invented by Gross et al., with a built-in Yang Mills gauge symmetry with a gauge group G of rank 16 which can be either $G = SO(32)/Z_2$ or $E_8 \times E_8$. This theory, though chiral, is anomaly-free. The descent from 26 to 10 dimensions is accomplished by compactification on a sixteen-torus (26 - 10 = 16) which using the beautiful results of Frenkel and Kac, in fact reproduces the full tally of 496 Yang Mills massless gauge particles associated with $SO(32)/Z_2$ or $E_8 \times E_8$ even though we started with only 16 gauge particles corresponding to the 16 tori. The remaining 480 gauge particles start life as solitons in the theory. The theory is gravitational (and chiral) anomaly free. The hope is that this theory may also be finite to all loop orders – the only finite theory of physics containing quantum gravity.

Can we proceed from 10 down to 4 physical dimensions? Witten et al. have shown that the 10 dimensional theory can indeed be compactified to 4 dimensional Minkowski spacetime x an internal six-dimensional space with SU(3) holonomy (a Calabi-Yau space) which preserves a residual N = 1 supersymmetry in 4-dimensions. A number of families emerge; their count is equal to 1/2 of the Euler number of the compactified space. The Yukawa couplings allowed in the theory are expected to be topologically determined.

It is all these remarkable features of superstring theories which make the string-theorist "purr" with deserved pride.

F) STRING THEORY AS THE "THEORY OF EVERYTHING" (T.O.E.

Could this be the long-awaited unified theory of all low energy phenomena in Nature? There is a fair prospect of this. But would such a theory be a

T.O.E. – a Theory of Everything? The answer in my opinion is NO. As remarked before, all theories which descend from higher to lower dimensions must contain massive particles in multiples of Planck mass $m_p \approx 1/R = \sqrt{\alpha/G}$. Since no *direct* tests of existence or interactions of such objects can ever be feasible, there will always remain the experimentally unexplored area of these higher masses and energies – in addition, of course, to the mystery of the quantum of action. What I am saying is that before they can be called a T.O.E., one must prove, at the least, a *uniqueness* of our present theories – a theorem which states that if a theory fits all known phenomena at low energies, it can have only *one* extrapolation to higher energies. From all past experience, this is unlikely – even as regards to the framework. (Think of the framework of Newtonian gravity versus that of Einstein's gravity.)

There arises an important question at this stage whether the string theories represent a wholly *new* attitude so far as the fundamental theory is concerned, or whether we are dealing with a a relativistic quantum field theory of the familiar type in 2-dimensions.

The latter point of view has been argued for by a number of authors: by Polyakov and those who have followed him, and latterly by Weinberg and in Trieste. This is the point of view of the so-called "first-quantised" string – represented by 2-dimensional gravity theory where the conventional Einstein action is the Euler number. The "string" Lagrangian of Nambu, Gotto, Nielson, Susskind and others (which for the bosonic case is represented by a set of 26 fields), is the preonic-matter Lagrangian, in interaction with this 2-d (non-propagating) gravity. Physics arises through topology which is determined by the number of handles on a 2-d sphere, on which the theory lives. (The number of these handles is specified by the Einstein action – which as I said earlier – is the Euler number for the manifold in two (and only) two dimensions.) A phase transition which makes the 26 preonic fields acquire expectation-values predicates the transition to the "familiar" *chart* of d = 26, space and time. Like all such theories, this relativistic quantum field theory is expected to be unitary so far as the basic twenty-six preonic fields are concerned. (Conformal invariance plays a crucial role in this.)

This formalism must be supplemented by composite-field expressions (for example, for the likes of W^+, Z^0, etc.), in terms of the basic 26 preonic fields (vertex operators which must respect conformality). Expressions for these can be written down in terms of products of preonic fields and their derivatives. The unitarity of this composite field theory presents a familiar challenge - familiar in the sense that this problem is on par with the unitarity problem of theories of composite hadrons - protons and neutrons - in the context of a fundamental quark theory.

The other point of view favours the "second quantised" version of the string theory. Here, unitarity problems for the likes of W's, Z's, photons (and also quarks and leptons in a spinning string version), present no difficulties. (It is as if one was writing the local field theory of hadrons.) Within the light-cone framework - the only one where the full supersymmetric version of the second quantised theory exists - the fact that "<u>local</u>" field theories of <u>extended</u> objects - like strings - should exist at all, is highly non-trivial. That such theories (of open and closed strings) number no more than seven altogether, is an added bonus.

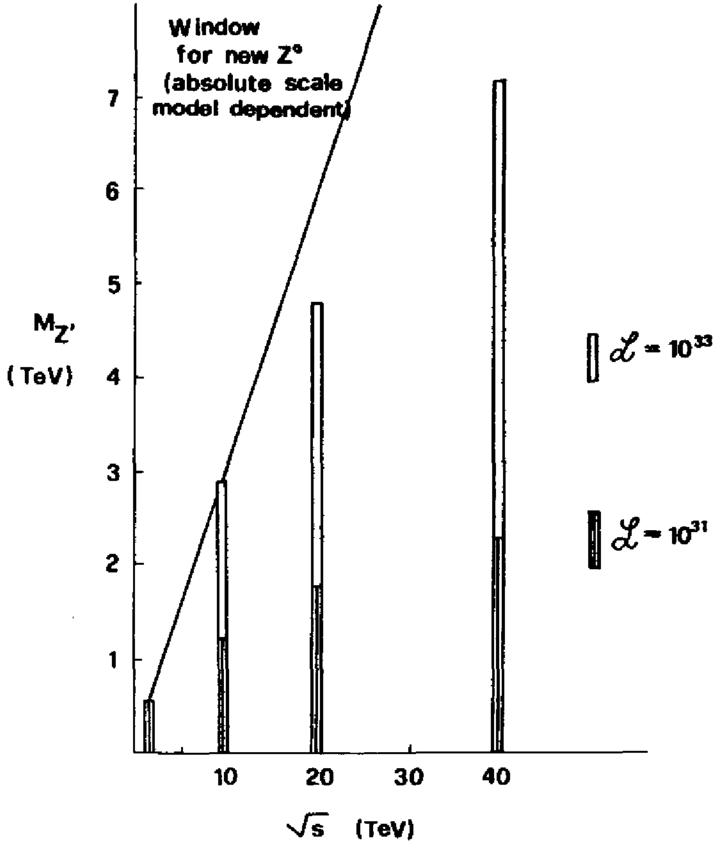
Perhaps the profoundest aspects of this second quantised formulation are represented by Witten, who derives the basic interaction in such a theory (at least for the purely bosonic open string), within a non-commutative geometry context - <u>freed of dependence on space-time charts</u>. If this point of view succeeds, the string theories would be ushering in an era, like that of quantum theory in 1925 and 1926 when a new epistemology came into existence to replace the humbler point of view represented by the "old" quantum theory.

Which of these two points of view will yield deeper insights, and in the end prevail, time will tell.

But apart from these matters of interpretation, the one crucial question which our experimental colleagues are entitled to ask, is this: what are the compelling experimental consequences of string theories?

The emergence of (necessarily a supersymmetric) standard model with the right number of families may, of course, be a triumph, but will it establish the superiority of the string attitude? At present, there are few unambiguous <u>new</u> predictions. One of them concerns the existence of one or two new Z^0's. In Figure 7 is shown the window for such Z^0's.

<u>Figure 7</u>



Unfortunately, their masses - even their existence - are not firmly predicted by the theory. A possibly firmer and more spectacular prediction is the existence of fractionally charged non-confined dyons which would, of course carry the appropriate integral magnetic monopolarity in accordance with the Dirac formula.

VII PASSIVE AND NON-ACCELERATOR EXPERIMENTS

(A) Next we come to the passive experiments which are mainly concerned with testing cosmological gauge aspects of grand unification theories (unifying electroweak and strong nuclear interactions). These are tests for (i) monopoles (topological defects of the π_2 type). Though predicted by the gauge theories concerned at high temperatures prevailing in the early Universe, one would not like too many monopoles around now; (note the claimed detection during 1985 of the South Kensington monopole) (ii) cosmological strings (π_1) which are good for galaxy seeding and (iii) domain walls (π_0), which apparently are a cosmological disaster. Surely, this set of predictions present a mixed bag of desirables and undesirables! In addition there is the question mark on varieties of remnant dark matter, endemic to a vast variety of theories and whose ever-lengthening list is given in Table III. (I shall not dwell on the role of inflation in cosmology in this context, which apparently can also resolve the problem of over-abundance of monopoles.)

TABLE III

EXPECTED DARK MATTER

		Mass	Origin (Time, Temperature)
	Invisible Axion	10^{-5}ev	10^{-30} sec (10^{12} GeV)
	ν	30 ev	1 sec (1 MeV)
Light	$\tilde{\gamma}$, gravitino	kev	10^{-4} sec (100 MeV)
Heavy	$\tilde{\gamma}$, gravitino,		
	Axiano, sneutrino, ν,	GeV	
	Monopole	10^{16} GeV	10^{-34} sec (10^{14} GeV)
Kaluza-Klein	Particles & Shadow Matter	10^{18}-10^{19}GeV	10^{-43} sec (10^{15} GeV)
	(Maximons, Pyrgons etc.)		
	Quark Nuggets	10^{15} grams	10^{-5} secs (300 MeV)
	Primordial Black Holes	$>10^{15}$ grams	$>10^{12}$ sec

(B) Among the most celebrated passive experiments is proton decay. As we know, a limit on $P \to e^+ + \pi^0 > 2.5 \times 10^{32}$ years partial decay-time is suggesed by the IMB collaboration. There are, however, claims for (seven) candidate-events for $P \to e^+ + K^0$ and $N \to \nu + \eta^0$ and $N \to \nu^0 + K^0$ modes, by the Kolar Gold Fields collaboration, Kamiokande and Nusex. (A firm detection of K's would signal supersymmetry and also explain the longer life-time.) A worrisome background is due to atmospheric neutrinos which would make it difficult, on earth, to be sure of a real signal for proton decay if its life much exceeds 10^{33} years. Pati, Sreekantan and Salam have suggested experiments on the moon where even though the primary flux of cosmic rays is unhindered by the existence of an atmosphere or magnetic fields, an experiment carried out in a tunnel or a cavern with 100 metres of moon-rock surrounding it on all sides, would cut down the backgrounds - in particular of ν_e neutrinos - to a figure less than 1/100 of the background on earth. If the proton life-time lies within the range of 10^{33} and 10^{35} years, experiments on the moon may become necessary for unambiguous detection.

The cost of such moon experiments consists in taking around 50 tons of detecting material to the moon, plus the cost of the making of the cavern; it may come to around one billion dollars. Such outlays would become feasible if moon colonization programmes are pursued seriously. I have no doubt that this will happen if there is a banning of nuclear weapons, since technological, advanced societies must spend funds on high technology projects, in order to keep the overall economy healthy.

This concludes my brief overview of particle physics in the middle of this decade.