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A High Energy Physics Perspective*

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ABSTRACT

The status of the Standard Model and role of symmetry in its development are reviewed. Some outstanding problems are surveyed and possible solutions in the form of additional "Hidden Symmetries" are discussed. Experimental approaches to uncover "New Physics" associated with those symmetries are described with emphasis on high energy colliders. An outlook for the future is given.

I. STANDARD MODEL OVERVIEW

A. Symmetry - A Historical Perspective [1]

Since antiquity, symmetry has been synonymous with beauty, simplicity, and harmony. As such, it inspired art, architecture, science, etc. of ancient civilizations. Nowhere is that influence more apparent than in Greek philosophy and mathematics. The Greeks viewed the circle and sphere as manifestations of nature's perfect symmetries. Their fascination with those forms led to the development of Euclidean Geometry, a tremendous intellectual advancement. It also engendered an appreciation for the regularity of celestial motion and the birth of astronomy. However, in that case symmetry became an obsession. The complex epicycle celestial model of Ptolemy with circles upon circles became accepted dogma. Failures of that model were perceived as observational distortions due to the imperfections of man and his methods. That viewpoint and the geocentric epicycle model lasted until the Renaissance years of 1500 A.D. Philosophical blindness had stifled the development of the scientific method and led to almost 2000 years of scientific stagnation. A lesson that we must always remember.

B. The Age of Reason

The fall of the geocentric epicycle model and rise of the scientific method resulted from the observations and studies of men such as Copernicus, Brahe, Kepler, and Galileo. It culminated with Newton's "Universal Theory of Gravity". Physics overcame metaphysics. Dynamics and equations of motion replaced the aesthetics of pure thought and the idealized symmetry of fantasies. Calculus was invented and the algebraic approach to problem solving largely replaced geometry. An "Age of Reason" resulted in which any problem scientific or social was viewed as solvable. Along with that view, the experimental approach prospered and modern science was born. Man's ability to understand the laws of nature made fast steady progress and culminated in the 1860's with Maxwell's equations and the mastery of electromagnetism. Classical physics became so well understood that Michelson made his famous pronouncement in 1894

> "The more important fundamental laws and facts of Physical Science have all been discovered, and these are now so firmly established that the possibility of their ever being supplanted in consequence of new discoveries is exceedingly remote... Our future discoveries must be looked for in the sixth place of decimals."

This insightful statement is often maligned as an end of physics message of despair (which was not the intention). It is then pointed out that in 1895 Becquerel's discovery of radioactivity ushered in the age of Modern Physics and its wonderous advances. Michelson's message is more appropriate today than it was 100 years ago. Will history repeat itself?

C. Symmetry Strikes Back

What happened to symmetry as a guiding principle during the great scientific advances of Newton... Maxwell? During the 19th century, the mathematics of symmetry was formalized by the development of Group Theory (Galois, Lie, and others). Symmetries and their associated conservation laws (energy, momentum, angular momentum etc.) were certainly known and used in physics problem solving, but they had little to do with fundamentals.

The importance of symmetry in physics was brought into prominence by the three great advances of the early twentieth century [1] 1) Special Relativity (1905), 2) General Relativity (1916), and 3) Quantum Mechanics (1925). The last of these, Quantum Mechanics, was particularly important for incorporating the language of group theory into the modern physics vocabulary. In that case, global symmetries were found to be powerful aids in classifying eigenvalue solutions to quantum equations. The elegance and importance of global symmetries was emphasized in the classic textbook by Wigner [2]. However, the prevailing view in these endeavors was that such symmetries were a useful tool but would be unnecessary if we could exactly solve the equations of motion. Physics respected certain symmetries but was not governed by them.

The more revolutionary view of symmetry as playing a fundamental role in physics came about from the work and insight of Einstein. He first showed that space and time were symmetric, a radical realization. That exact symmetry of nature had been present but hidden in Maxwell's equations. Its unveiling required the genius of Einstein. The resulting symmetry of Poincaré invariance is the basis of elementary particle physics and quantum field theory. The 10 generators of translations, rotations, and boosts provide a group structure for classifying elementary particles as irreducible representations labeled by their Casimir invariants of mass and spin.

Einstein's formulation of the equivalence principle and general relativity was even more fundamental than space-time symmetry. He showed that invariance under general coordinate transformations, a local symmetry, gave rise to gravitational field equations. The recognition that

"Symmetry Dictates Dynamics"

is Einstein's great legacy [1]. He gave us a profound understanding of how in the case of gravity local symmetry requirements give rise to fundamental interactions. Extensions of that insight are the bases for modern elementary particle physics and the standard model, as well as efforts to go beyond it. Indeed, Einstein's breakthrough was followed by 1) Herman Weyl's formulation of electromagnetism as following from local U(1) gauge invariance and the introduction of the gauge field potential $A_\mu(x)$. 2) The Yang-Mills generalization of local gauge invariance from U(1) to non-abelian SU(N) symmetries. 3) The Weinberg-Salam [3] $SU(2)_L \times U(1)_Y$ local symmetry of electroweak unification. 4) The emergence of local $SU(3)_c$ quark color symmetry as a complete theory of strong interactions, Quantum Chromodynamics (QCD). Collectively, those advances constitute the "Standard Model" of strong and electroweak interactions. Let me next recall the status of that very successful theory.

D. The Standard Model

The $SU(3)_c \times SU(2)_L \times U(1)_Y$ local gauge theory of strong and electroweak interactions accommodates all known elementary particles and elegantly incorporates the proven symmetries and successes of Poincaré invariance, quantum electrodynamics, the Four-Fermi V-A theory, quark model, etc. It correctly predicted weak neutral currents [3] as well as the observed properties of $W^\pm$, Z, and gluons. In addition, because that theory is renormalizable, its predictions can be scrutinized at the quantum loop level by high precision measurements. Remarkably, a wealth of experimental data has now been confronted at 1% or better without any signal of disagreement or inconsistency [4]. Those impressive successes have earned for the $SU(3)_c \times SU(2)_L \times U(1)_Y$ theory the title "Standard Model", a label that describes its acceptance as a proven standard or paradigm against which future experimental findings and alternative theories must be compared.

As a summary of the standard model content, I have illustrated in Table I its minimal spectrum of particles along with some of their basic properties. The fermions are grouped into three generations of leptons and quarks which span an enormous mass range. Experiments are consistent with massless neutrinos as required by the minimal standard model (i.e. no right-handed neutrinos and only a Higgs scalar doublet). There are, however, some hints of very small neutrino masses (and mixing) from solar and atmospheric neutrino experiments. Should non-zero neutrino masses be established, they could be easily accommodated but would point to "new physics". For example, many grand unified theories (GUTS) naturally predict small neutrino masses. All of the particles in that table have been observed (directly or indirectly) except for the Higgs scalar, H.

Quarks and leptons interact by exchanging gauge bosons as dictated by the local gauge symmetries. Eight massless gluons couple to the color $SU(3)_c$ charge and mediate strong interactions, while the $W^\pm$, Z, and γ of the $SU(2)_L \times U(1)_Y$ sector are responsible for weak and electromagnetic interactions. The $SU(3)_c$ gauge theory, quantum chromodynamics (QCD), taken on its own, has no arbitrary or free parameters. (It is a perfect theory.) It is scale invariant; so, even its gauge coupling constant can be traded in for a mass scale [5] (dimensional transmutation), Λ, which merely serves as a unit of mass. All low hadronic masses are proportional to Λ, $m_i = C_i\Lambda$, with the C_i calculable predicted numbers. In principle, nonperturbative schemes such as lattice QCD should be capable of computing the C_i. All that is needed is a powerful computer and clever algorithm. QCD is a beautiful theory, a simple yet elegant field theory capable of explaining all strong interaction dynamics. Nevertheless, exploring its rich dynamical consequences and subtleties of its non-linearity (confinement, exotic spectroscopy, proton structure, the quark-gluon plasma etc.) remains extremely interesting and may still reveal surprises. We must continue to study it.

In contrast with QCD, the electroweak sector has many arbitrary parameters. Most stem from the Higgs sector which is used to break the $SU(2)_L \times U(1)_Y$ symmetry and endow particles with masses. To accommodate observed phenomenology, a complex Higgs scalar isodoublet is appended to the electroweak theory via $\lambda\phi^4$ interactions (the linear sigma model). Remarkably, its vacuum expectation value $v \simeq 246$ GeV, the electroweak scale, is capable of generating all electroweak masses, mixing, and even CP violation. The disparity of particle masses in Table I is determined by the size of their coupling to the Higgs. Unfortunately, those coupling strengths are arbitrary and merely set by observation rather than predicted.

It is generally believed that the simple Higgs mechanism is incomplete and "new physics" must emerge as shorter distances (higher energies) are probed. That conviction is based on shortcomings of the Higgs mechanism, e.g. $\lambda\phi^4$ is trivial (noninteracting) when considered alone and exhibits fine-tuning hierarchy problems when embedded in a grand unified theory or theory of gravity. In addition, one hopes that the truly final fundamental theory, we seek, will be free of arbitrary parameters and will elucidate the origin of mass.

What are the parameters of the standard model? If we define our mass units by the electron volt (with $\hbar = c = 1$), then the scale of QCD in an effective 5 flavor scheme [6] using modified minimal subtraction ($\overline{MS}$) is

$$\Lambda^{(5)}_{\overline{MS}} \simeq 209^{+93}_{-72}\ \text{MeV} \tag{1}$$

which corresponds to a gauge coupling at scale $\mu = m_Z$

$$\alpha_3(m_Z)_{\overline{MS}} = \frac{g_3^2(m_Z)}{4\pi} = 0.118 \pm 0.007 \tag{2}$$

In the $SU(2)_L \times U(1)_Y$ electroweak sector, one finds gauge couplings

$$\alpha_2(m_Z)_{\overline{MS}} = \frac{g_2^2(m_Z)}{4\pi} = 0.03382 \pm 0.00005$$

Table I: Elementary Particles and Their Properties

Particle	Symbol	Spin	Charge	Color	Mass (GeV)	
Electron neutrino	ν_e	1/2	0	0	$< 4.5 \times 10^{-9}$	
Electron	e	1/2	-1	0	0.51×10^{-3}	First
Up quark	u	1/2	2/3	3	5×10^{-3}	Generation
Down quark	d	1/2	-1/3	3	9×10^{-3}	
Muon neutrino	ν_μ	1/2	0	0	$< 1.6 \times 10^{-4}$	
Muon	μ	1/2	-1	0	0.106	Second
Charm quark	c	1/2	2/3	3	1.35	Generation
Strange quark	s	1/2	-1/3	3	0.175	
Tau neutrino	ν_τ	1/2	0	0	$< 2.4 \times 10^{-2}$	
Tau	τ	1/2	-1	0	1.777	Third
Top quark	t	1/2	2/3	3	174 ± 6	Generation
Bottom quark	b	1/2	-1/3	3	4.5	
Photon	γ	1	0	0	0	
W Boson	$W^\pm$	1	± 1	0	80.31 ± 0.16	
Z Boson	Z	1	0	0	91.188 ± 0.002	
Gluon	g	1	0	8	0	
Higgs	H	0	0	0	$64 \leq m_H < 800$	

$$\alpha_1(m_Z)_{\overline{MS}} = \frac{g_1^2(m_Z)}{4\pi} = 0.01694 \pm 0.00002 \qquad (3)$$

Note, the values in Eq. (3) are not so much smaller than the QCD coupling in Eq. (2). Indeed, the values of all three couplings can be related via quantum loop renormalizations if we embed the standard model in a grand unified theory (GUT) such as SU(5), SO(10) etc. (broken at $\sim 10^{16}$ GeV), and introduce new physics such as supersymmetry at an intermediate mass scale ~ 1 TeV.

The Higgs mechanism appends a complex scalar doublet and its potential

$$\begin{aligned} &\lambda_0(\phi^2 - v_0^2/2)^2 \\ \phi &= \frac{1}{\sqrt{2}}\begin{pmatrix}\phi_1 + i\phi_2 \\ \phi_3 + i\phi_4\end{pmatrix} \end{aligned} \qquad (4)$$

to the theory with a minimum at $|\phi| = v_0/\sqrt{2}$. The gauge coupling of the scalar to $W^\pm$ and Z bosons leads to natural bare masses and coupling relations

$$\begin{aligned} m_W^0 &= m_Z^0 \cos\theta_W^0 = g_2^0 v_0/2 \\ &\tan\theta_W^0 = \sqrt{\frac{3}{5}}\frac{g_1^0}{g_2^0} \end{aligned} \qquad (5)$$

The measured value of m_W then implies

$$v \simeq 246 \text{ GeV} \qquad (6)$$

as the scale of electroweak symmetry breaking and source of all electroweak masses. A remnant of that mechanism is a single physical scalar particle H, the Higgs. Its mass is set by the arbitrary parameter λ.

$$m_H^0 = \sqrt{2\lambda_0} v_0 \qquad (7)$$

Determination of λ requires a measurement of the Higgs mass, m_H, or a study of longitudinal gauge boson scattering, e.g. $W_L W_L \to W_L W_L$.

The main source of arbitrary electroweak parameters is the Higgs-fermion Yukawa couplings. In the quark sector, there are 18 independent complex couplings which connect the 3 left-handed doublets and 6 right-handed singlets to the Higgs. That constitutes 36 independent real parameters. Most are, however, unobservable. They reside in undetectable right-handed mixing angles or relative quark phases. Left-over are 6 masses and 4 parameters of the CKM (Cabibbo-Kobayashi-Maskawa) mixing matrix. The quark masses are given in Table I. The first two rows of CKM elements are (experimentally)

$$\begin{aligned} |V_{ud}| &= 0.9736 \pm 0.0007 \\ |V_{us}| &= 0.2196 \pm 0.0023 \\ |V_{ub}| &= 0.0036 \pm 0.0021 \\ |V_{cd}| &= 0.215 \pm 0.016 \\ |V_{cs}| &= 0.98 \pm 0.12 \\ |V_{cb}| &= 0.040 \pm 0.006 \end{aligned} \qquad (8)$$

They are related by and consistent with unitarity. The third row involves the top quark and currently can be only indirectly inferred from loop effects and unitarity considerations. One finds

$$\begin{aligned} |V_{tb}| &= 0.9992 \pm 0.0003 \\ |V_{ts}| &\simeq |V_{cb}| \\ |V_{td}| &\simeq 0.01 \end{aligned} \tag{9}$$

with $|V_{td}|$ roughly determined by B_d^0-$\bar{B}_d^0$ oscillations. It is amusing that $|V_{tb}|$ is the best known of the CKM elements.

Lepton masses are also determined by their Yukawa couplings to the Higgs doublet. If neutrinos have mass, then one also expects mixing in the lepton sector analogous to the CKM matrix.

Given the central role of the elementary Higgs mechanism in electroweak symmetry breaking and mass generation, one would like experimental confirmation or negation of the H's existence. In the simple Higgs doublet scenario, it is instructive to examine the $\lambda\phi^4$ sector of the theory after identifying

$$\begin{aligned} w^\pm &= (\phi_1 \mp i\phi_2)/\sqrt{2} \\ z &= \phi_4 \\ H &= \phi_3 - v_0 \end{aligned} \tag{10}$$

In terms of those fields, the Higgs potential becomes [7]

$$\mathcal{L}_{\rm int} = -\lambda_0 \left(w^+ w + \frac{1}{2} z^2 + \frac{1}{2} H^2 + v_0 H \right)^2 \tag{11}$$

From the quadratic terms, we find three massless Goldstone bosons $w^\pm$, z which become longitudinal components of the $W^\pm$, Z gauge fields and the physical Higgs scalar, H, with mass $\sqrt{2\lambda_0}\, v_0$. In a sense, the $w^\pm$ and z were discovered when massive $W^\pm$ and Z bosons were found and only the H remains to be uncovered. Finding that remnant of spontaneous symmetry breaking or whatever "new physics" replaces it, is the major goal of high energy physics.

How will the Higgs scalar be discovered? The likely means of finding the Higgs depends on its mass, m_H; so, let me briefly discuss mass constraints. Searches at LEP have failed to find the Higgs and provide the lower bound

$$m_H \gtrsim 64 \text{ GeV} \qquad \text{(LEP)} \tag{12}$$

LEP II will push the Higgs search to about 80 GeV (hopefully somewhat higher $\approx$ 90 GeV). Beyond that probably requires a new collider facility, although the Fermilab $p\bar{p}$ collider with its upgraded luminosity may be able to cover the 80–130 GeV region. The LHC should be capable of finding a Higgs in the mass range 80–800 GeV. At the lower end of that mass range, one searches for the (rare) loop induced decay $H \to \gamma\gamma$ or $W + H \to b\bar{b}$. That covers the 80–130 GeV region. From 130–182 GeV, the decay $H \to ZZ^*(Z^* = \text{virtual } Z) \to 4$ leptons provides the discovery. For $m_H \gtrsim 182$ GeV, $H \to ZZ \to 4$ leptons should be discernible up to about 800 GeV. Above 800 GeV, the Higgs width becomes rather broad and the signal fades. Indeed, for $m_H \gg m_W$ the Higgs width into gauge boson pairs

$$\Gamma(H \to W^+W^- \text{ or } ZZ) \simeq \frac{3g_2^2}{128\pi} \frac{m_H^3}{m_W^2} \tag{13}$$

grows like m_H^3. (At $m_H \simeq 1$ TeV, $\Gamma_H \simeq 500$ GeV.) The reason for the Γ_H growth is easily seen from Eq. (11). The HW^+W^- coupling for longitudinal W's is given by

$$HWW \text{ coupling} = -2i\lambda_0 v_0 \simeq -ig_2 \frac{m_H^2}{2m_W} \tag{14}$$

The Higgs mass grows like $\sqrt{\lambda}$; so, large Higgs mass corresponds to very strong coupling and probably indicates underlying new dynamics. If λ is very large, we find various pathologies in the model. For example, examining the S-matrix for $W_L^+W_L^- \to W_L^+W_L^-$ at large s, one finds that perturbative partial wave unitarity in the $J = 0$, $I = 0$ channel requires [8]

$$\left| \frac{5\lambda}{16\pi} \right| < 1/2 \tag{15}$$

which implies $m_H \lesssim 780$ GeV. For larger λ, unitarization of the S-matrix suggests "new physics" such as ρ-like spin 1 mesons. Such resonances would manifest themselves in WW scattering (analogous to $\pi\pi$ scattering), but sorting out signal from background will be difficult.

Although the Higgs scalar is a focus of our quest, it is generally believed that the Higgs mechanism is only part of a larger underlying structure waiting to be uncovered. There may be a whole spectrum of new particles and interactions which provide a deeper understanding of mass generation, CP violation etc. Suggestions regarding what new physics might be expected are based on ideas about symmetry as well as responses to the outstanding problems some of which I briefly recall.

II. OUTSTANDING PROBLEMS AND COMPELLING QUESTIONS

Although the standard model accommodates all known phenomenology and must be viewed as one of the great scientific triumphs of the twentieth century, it cannot be the final word. There remain too many open issues which must be resolved. The ad hoc description of mass generation via the Higgs mechanism and unexplained pattern of fermion masses and mixing (including CP violation) are the most unsatisfactory aspects. There are, in addition, many other problems and questions which must also be confronted before we can claim to understand the basic laws of nature. I mention below a few of the compelling questions

Electroweak Symmetry Breaking: Is there an elementary or dynamical Higgs scalar? What is its mass? What are its properties and origin?

Top Quark Physics: Why is top so heavy? What are its properties? Alternatively, why are the other fermions so light?

Fermion Masses, Mixing, and CP Violation: What is the underlying physics of fermion mass generation? How well can we test standard model predictions for quark mixing and CP violation?

Neutrino Masses and Mixing: Do neutrinos have non-zero masses? Are they part of dark matter? Do neutrinos oscillate?

Generations: Why are there 3 generations? Are there exotic heavy fermions?

Parity: Why is parity violated? We accommodate but do not understand the chiral structure of electroweak symmetry.

Non-Standard CP Violation: Is there CP violation beyond the Standard Model? Is it related to the matter-antimatter asymmetry of the universe?

QCD Dynamics: What is the structure of the proton? Can we better understand quark confinement? Are there exotic quark-gluon bound states? Is there *a* quark-gluon plasma?

Grand Unification: Can we confirm grand unification of strong and electroweak interactions? Is proton decay observable?

Gravity: What is the connection between gravity and the standard model?

III. POSSIBLE ANSWERS - ADDITIONAL SYMMETRIES

Given the success of local gauge invariance in explaining strong and electroweak interactions, it is not surprising that we continue to seek guidance via possible additional symmetries. In fact, most conjectured solutions to the above problems entail local symmetry enlargements which remain hidden until new physics associated with them is uncovered. Let me mention a few leading possibilities.

i) Extra Gauge Bosons: Enlarging the Standard Model gauge group by appending an $SU(2)_R$ or $U(1)'$ symmetry would lead to additional $W_R^{\pm}$ and Z' gauge bosons. The $SU(2)_R$ appendage has the nice feature of providing Left-Right symmetry. Additional $U(1)'$ symmetries could result from superstrings or GUTS. Currently, the Fermilab $p\bar{p}$ collider explores the gauge boson mass range $\sim$ 500 GeV and has not seen evidence for such particles. With anticipated luminosity upgrades, they can reach $\sim$ 1 TeV. The LHC should probe as high as 5 TeV.

ii) Grand Unification: Embedding the standard model in a simple gauge group such as SU(5), SO(10), E_6...has some very attractive features. It leads to strong-electroweak unification $\alpha_3^0 = \alpha_2^0 = \alpha_1^0 = \alpha_G^0$ at very short-distances. There is in fact some evidence for such unification in supersymmetric GUTS. Grand Unification also implies proton decay, which if observed would be a revolutionary discovery. The unification scale of $\sim 10^{16}$ GeV is too high for direct high energy probes. Instead, one will have to rely on precision measurements (remember Michelson's prophecy) and searches for forbidden reactions to uncover such very short-distance hidden symmetries.

iii) Technicolor Dynamics: Just as $SU(3)_c$ local gauge invariance leads to rich QCD dynamics, a much stronger local $SU(N)_{TC}$ symmetry called technicolor would dynamically break $SU(2)_L \times U(1)_Y$ and endow the $W^{\pm}$ and Z with masses. Such a scenario is attractive but looses appeal when one attempts to generate fermion masses. Very complicated extended technicolor models have been proposed to accomplish that task, but they lack simplicity and are problematic on several fronts. A generic prediction of such models is a plethora of new technicolor spectroscopy at $\mathcal{O}$(1 TeV) as well as lower mass pseudo-goldstone bosons. So far, there is no experimental support for technicolor. Progress in that area will likely require experimental guidance. If a new strong dynamics symmetry like technicolor occurs at $\sim$ 1 TeV, much work will be required to resolve its properties and new high energy colliders will be of central importance in that effort.

iv) Supersymmetry: The most radical, most appealing, most ambitious new symmetry is supersymmetry (SUSY). It is also the most likely possibility in the opinion of many theorists. The basic idea is to enlarge the Poincaré algebra with an additional spinor generator Q_α (or several such spinors). The resulting graded Lie algebra is the only known way to consistently expand the concept of space-time. That symmetry enlarges irreducible particle representations to include both bosons and fermions. If supersymmetry were exact, every known fermion (boson) would have a degenerate boson (fermion) partner. Since that is not the case, supersymmetry must be broken. But is the breaking at the Planck scale $\sim 10^{19}$ GeV or much closer to the electroweak scale $\sim$ 250 GeV?

Motivation for supersymmetry comes from various sources. Extending global supersymmetry to general coordinate transformations leads to supergravity which finds a natural origin in superstrings. Such a scenario can give a finite theory of quantum gravity and solve the hierarchy problem (why is $m_W \ll m_{\text{planck}}$?). It may also turn out to be unique and parameter free. Superstrings could revolutionize both physics and mathematics.

Is SUSY relevant for experimental particle physics? If the scale of SUSY particles is $\lesssim$ 1 TeV, the answer is certainly yes. It would imply that every known elementary particle has a supersymmetric partner waiting to be discovered. There are also other exciting implications. Minimal SUSY predicts 5 spin 0 scalars with the lightest Higgs like particle $\lesssim$ 130 GeV. The lightest supersymmetric particle (presumably a spin 1/2 neutralino) would be stable and weakly interacting. It is a leading cold dark matter candidate. Wouldn't it be amazing if most of the mass in our universe turns out to consist of supersymmetric particles.

If supersymmetry is manifested at low energies, then much will be discovered by the next generation of colliders. In fact, SUSY would be a much bigger prize than the Higgs scalar, since it dramatically alters our view of space-time. Currently, the only evidence for SUSY comes from the unification of couplings in a SUSY GUT framework. It will be interesting to see if that hint is in fact the first harbinger of SUSY or merely a coincidence.

IV. EXPERIMENTAL APPROACHES

Given the success of local symmetries and promise of superstrings, perhaps experiments are no longer needed. Instead, one might contemplate an all out theoretical blitz to find an elegant, aesthetically appealing, possibly unique superstring model which explains everything we currently know. Indeed, such a view is consistent with Einstein's famous quote from his 1933 Herbert Spencer Lecture:

> "I am convinced that we can discover by means of purely mathematical constructions the concepts and the laws connecting them with each other, which furnish the key to the understanding of natural phenomena."

It is not possible to find a counter quote from someone with Einstein's credentials. Instead, I borrow from the fictional super sleuth Sherlock Holmes who said:

> "It is a capital mistake to theorize before one has data. Insensibly one begins to twist facts to suit theories, instead of theories to suit facts."

Anyone who has a pet theory can recognize the truth in that statement.

Ultimately, I believe that Einstein will be correct, but it is much too premature to abandon experiments. We have the experimental capabilities to find the Higgs or whatever is responsible for electroweak symmetry breaking. Supersymmetry and other new particles may also be within reach. In addition, revolutionary discoveries may come from non-accelerator physics, e.g. proton decay, cosmic neutrinos etc. We have the technology to push further and that knowhow must be exploited.

To advance our knowledge and address the many compelling questions before us, requires a broad diverse experimental program with lots of discovery potential. It must be capable of testing the standard model but at the same time be sensitive to "new physics". The program should utilize accelerators but also support non-accelerator physics initiatives. Roughly speaking, we must push as hard as possible on the High Energy, High Intensity, and High Precision frontiers.

High energy accelerators take us to new domains where top, Higgs, and "new physics" can be directly produced and studied. Right now, the Fermilab 1.8 TeV $p\bar{p}$ collider has the highest center-of-mass energy of any accelerator in the world and thus has unique discovery potential. Ongoing luminosity upgrades will make it our premier high energy tool for the next decade. The LHC, scheduled for 2005 will take us to 14 TeV with very high luminosity $\simeq 10^{34}$ cm^{-2}s^{-1}. Besides finding the Higgs, it will be capable of uncovering supersymmetry, Z' bosons, technicolor or many other scenarios with "new physics" $\lesssim 1$ TeV. Beyond those facilities, new ideas and technologies are required. The Next Linear Collider (e^+e^-) offers an exciting viable possibility. Recently, there has also been growing enthusiasm for a $\mu^+\mu^-$ collider with high energy $\gtrsim 4$ TeV and luminosity $> 10^{34}$ cm^{-2}s^{-1}. Such a facility, if feasible, would be a significant technological leap forward.

A different approach to finding "new physics" involves studies of very rare, or even forbidden processes, including CP violation. Searches for rare μ, K, B, and τ decays, proton decay, neutrino oscillations, electric dipole moments, etc. are all well motivated and could provide big payoffs. Indeed, a discovery in any of those areas would revolutionize our thinking and open up new areas of research. To illustrate the state of affairs, I have given in Table II some current bounds on muon number violating μ and K reactions along with projected capabilities of ongoing experiments and future possibilities. Hopefully, rare B and τ decays, such as $\tau \to \mu^+\mu^-\mu^+$ can make similar advances.

A third means of testing the standard and searching for "new physics" relies on high precision measurements of fundamental parameters such as m_W, m_Z, Γ_Z, $\sin^2\theta_W$, CKM parameters, etc. Those experiments probe predicted quantum loop effects. A deviation from expectations would signal the presence of physics beyond the standard model. Ultimately, this approach may provide our best test of GUT and superstring structure. (Remember Michelson's quote.)

V. FUTURE COLLIDERS

High energy experiments are in somewhat of a lull. We are anxiously waiting for the next dramatic experimental discovery which will rekindle our imaginations. Fortunately, anticipated future collider facilities offer broad discovery potential. B factories will provide new ways to explore CP violation. LEPII will push its e^+e^- center-of-mass energy to $\sqrt{s} \simeq 190$ GeV. If a standard model or SUSY Higgs with mass $\lesssim 90$ GeV exists, it should be found. I think there is a reasonable chance. Perhaps, they will also get a first glimpse of SUSY. On the bread and butter side, the $W^\pm$ mass will be measured to about ± 50 GeV at LEPII. That will provide an interesting constraint on the Higgs mass via quantum loop relations.

On the hadronic collider front, the Fermilab main injector upgrade will allow the $p\bar{p}$ Tevatron to operate at $\sqrt{s} = 2$ TeV and luminosity $10^{32} \sim 10^{33}$. Those improvements broaden the discovery potential while allowing precision measurements and searches for rare B and τ decays. The Higgs mass region of $80 \sim 130$ GeV may be explored via $W^\pm H$ and ZH associated production if the $H \to b\bar{b}$ mode is resolvable [9]. We might also get a glimpse for SUSY.

In the longer term ($\sim$ 2005), the LHC pp collider with $\sqrt{s} = 14$ TeV should find the Higgs scalar or tell us it doesn't exist. If SUSY exists $\lesssim 1$ TeV it will be discovered. Hopefully, completely unexpected discoveries will also be made.

Beyond the LHC, various collider options are possible. The Next Linear Collider (NLC) would start e^+e^- collisions at $\sqrt{s} = 500$ GeV and be upgradable to 1–1.5 TeV. It would have high luminosity $> 5 \times 10^{33}$ and e^- polarization. The NLC also offers $\gamma\gamma$, e^-e^-, and $e^-\gamma$ collider options which expand its physics potential. Recently, there has been discussion of possible e^+e^- colliders with $\sqrt{s} \simeq 5$ TeV, a major step if achievable. The NLC is a superb tool for studying the Higgs, SUSY, Technicolor etc. [10].

Less advanced ideas are the $\mu^+\mu^-$ collider and Really Large Hadron Collider (pp with $\sqrt{s} \simeq 100$ TeV). The muon collider concept is extremely interesting, but how can one demonstrate the technology? An effort at BNL will aim to produce very intense muon beams and use them to do physics (such as $\mu^- N \to e^- N$). Such hands on efforts combined with a vigorous R&D effort could lead to the First Muon Collider, but at what energy, 91 GeV, 500 GeV, 4 TeV? In my view, the 4 TeV facility is most complementary to the LHC and currently best

Table II: Existing and anticipated bounds (at 90% C.L.) on various muon-number violating reactions. The last column lists some speculations on how far the bounds might be pushed at upgraded existing or contemplated new facilities.

Reaction	**Current Bound**	**Ongoing Exp.**	**Future (?)**
$B(\mu^+ \to e^+e^-e^+)$	$< 1.0 \times 10^{-12}$	—	$\sim 10^{-13}$
$R(\mu^- N \to e^- N)$	$< 1 \times 10^{-12}$	$\sim 10^{-13}$ (SINDRUM II)	$\sim 10^{-16}$!
$B(\mu^+ \to e^+\gamma)$	$< 4.9 \times 10^{-11}$	$\sim 7 \times 10^{-13}$ (MEGA)	$\sim 10^{-14}$
$B(K_L \to \mu e)$	$< 2.4 \times 10^{-11}$	$\sim 8 \times 10^{-13}$ (BNL 871)	$\sim 2 \times 10^{-14}$
$B(K^+ \to \pi^+ \mu e)$	$< 2.1 \times 10^{-10}$	$\sim 3 \times 10^{-12}$ (BNL 865)	$\sim 5 \times 10^{-14}$

motivated.

The Really Large Hadron Collider with $\sqrt{s} \simeq 100$ TeV and $\mathcal{L} \simeq 10^{35}$ looks technically feasible but is very expensive. People are working on new ideas to significantly reduce the cost. An interesting study would be a comparison of pp vs. $\mu^+\mu^-$ physics potential. Hopefully, such a study will be started here at Snowmass.

VI. OUTLOOK AND COMMENTARY

"The future isn't what it used to be," but we do have the Standard Model. It represents a tremendous scientific achievement and guide to future exploration. Many outstanding questions remain. The primary issue, the source of electroweak symmetry breaking and mass generation is nearly within grasp and will be addressed by the next generation of colliders, particularly the LHC.

Where do we go from here? In my view the NLC physics case is extremely compelling. Such a facility must be built, but where and at what cost? Whatever country rises to that challenge is likely to be the leader in high energy physics during the first half of the next century. Upgrades of such a facility offer decades of forefront physics.

The muon collider concept is an idea whose time has come. Now it requires serious study and R&D. It has the attractive feature of fitting on an existing laboratory site and using the existing infrastructure. If it can work, it should be built.

Does a Really Large Hadron Collider with $\sqrt{s} \simeq 100$ TeV have viability? Our SSC experience suggests a prohibitive cost and difficult construction issues because of its size. However, interesting new ideas about inexpensive magnets and tunnels offer hope.

Perhaps it is appropriate to recall the words of the great experimentalist Ernest Rutherford

"We haven't got the money, so we have to think"

We must find the source of electroweak symmetry breaking and mass generation, open new frontiers, find new symmetries, and continue Einstein's legacy. We don't want to be responsible for another 2000 years (or even 20 years) of scientific stagnation.

Think Good Thoughts

VII. REFERENCES

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