Is there a future for high-energy physics?*

G. Rajasekaran

The present state of high-energy physics is critically examined. In spite of the spectacular success of the standard model, there is a serious crisis facing the field. Since the Planck scale is now recognized to be the true fundamental scale of physics, the importance of research on new methods of acceleration that can take us to superhigh energies is emphasized.

THE major events which culminated in the construction of the standard model of high-energy physics are presented in Table 1 in chronological order. Using nonabelian gauge theory with Higgs mechanism, the electroweak (EW) theory was already constructed in 1967, although it attracted the attention of most theorists only after another four years, when it was shown to be renormalizable. The discovery of asymptotic freedom of nonabelian gauge theory and the birth of QCD in 1973 were the final inputs that led to the full standard model.

On the experimental side, the discovery of scaling in deep inelastic scattering (DIS), which led to the asymptotic free QCD, and the discovery of the neutral current, which helped to confirm the electroweak theory, can be regarded as crucial experiments. To this list, one may add the polarized electron–deuteron experiment, which showed that $SU(2)_L XU(1)$ is the correct gauge group for electroweak theory, the discovery of gluonic jets in electron–positron annihilation, confirming QCD, and the discovery of W and Z in 1983, which established the electroweak theory. The experimental discoveries of charm, τ and beauty were fundamental for the concrete three-generation standard model.

However, note the blanks after 1973 and 1983 on the theoretical and experimental sides, respectively. Theoretical physicists have been working even after 1973 and experiments are being done even after 1983. But the tragic fact is that none of the bright ideas proposed by theorists in the past 20 years has received any experimental support and none of the experiments done in the past 10 years has led to any discovery. Even the famous W, Z discovery was only a confirmation of a theory proposed 16 years earlier†.

It is clear that if such a situation persists for long, it may become difficult to continue to be optimistic about the future of high-energy physics.

It may be argued that the current lean period of discoveries in high-energy physics is just a natural consequence of the spectacular success achieved in the past decades. The construction of the standard model is certainly a watershed. In the standard model we now have a theory for *all* that is known in high-energy physics. So, there is nothing more to do!

Clearly, the above sentiments are quite detrimental to the progress of any scientific field. In any case, there are too many loopholes in the standard model to be satisfied with it, the biggest of these being the omission of gravitation, the most important force of nature.

There are still many interesting questions and unsolved problems within the standard model: Higgs and symmetry breaking, QCD, neutrinos, CP, etc., and there may be other surprises and discoveries (supersymmetry, compositeness, etc.) which may take us beyond the standard model. Nevertheless, an examination of the current scene reveals a serious crisis facing high-energy physics – namely, the widening gap between theory and experiment. The primary factor that is responsible for this crisis is the recognition that quantum gravity is the next frontier of high-energy physics and that the true fundamental scale of physics is the Planck mass $M_p \sim 10^{19}$ GeV, which

Table 1. History of the standard model

	Theory		Experiment	
1950s	1954	Nonabelian gauge fields		
1960s	1964	Higgs mechanism		
	1967	EW theory		
			1968	Scaling in DIS
1970s	1971	Renormalizability of EW theory		
	1973	Asymptotic freedom →QCD	1973	Neutral current
			1974	Charm
			1975	τ-lepton
			1977	Beauty
			1978	$\vec{e}d$ experiment
			1979	gluonic jets
1980s			1983	W, Z
1990s				

G. Rajasekaran is in the Institute of Mathematical Sciences, Madras 600 113, India.

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†The evidence for top quark recently announced[1] is important, but top is expected within the standard model.

is the scale of quantum gravity. As a result, all present-day experimental activity in high-energy physics has been reduced to zero-energy physics. On the other hand, enchanted by the theories at the Planck mass, many active theorists are drowning themselves in the depths of mathematics. Physics is an experimental science. Hence, this gap between theory and experiment will ultimately spell the ruin of high-energy physics.

Can this energy barrier separating experiment and theory be surmounted? Are controlled experiments at M_p possible? Can Planck energy be obtained in the laboratory? The future of high-energy physics hangs on the answers to these questions.

Already, grand unification had pushed the fundamental scale to 10^{15} GeV and quantum gravity takes it to 10^{19}GeV. Preoccupation with such superhigh energies without the sobering control of experiments is bound to lead to metaphysics. *The pre-eminence of experiments in physics must be reestablished.* So it is imperative that physicists and technologists put their minds together to solve this crucial problem of the energy barrier. After all, there is no law of nature (such as the second law of thermodynamics) which forbids the attainment of such energies in the laboratory*. Human ingenuity knows no bounds and a method will be found to reach the superhigh energies so that *controlled laboratory experiments* can be done to test quantum gravity, superstrings or even theories beyond.

How do we reach Planckian energies? Can we envisage a Planckian accelerator? Before we answer this question we examine a few indirect methods.

Cosmology and early universe

If current ideas in cosmology and astrophysics are correct, then early universe provides us with a high-energy physics laboratory where particle energies were not limited by any budget cuts or other restrictions. Hence, it is thought that all our theories of high-energy physics are testable in principle by appealing to events in the early universe. We seem to have come a long way from Landau's dictum: 'Astrophysicists are often wrong but seldom in doubt'.

However, we know of only one universe and the events presumably occurred only once, that too, quite a long time ago. Modern science owes its existence to the advent of repeatable experiments under controlled conditions, whereas history provides only a single sequence of events. History cannot be a substitute for science. Cosmology cannot provide crucial and definitive tests for fundamental theories of physics. On the other hand, high-energy physics can definitely be applied to the study of the early universe. Laws of physics inferred from and tested in laboratory experiments can and must be applied to the study of the history of the universe. *No definitive law of physics can be inferred from speculative theories of the beginning of the universe.*

In other words, it is advocated that the only healthy traffic between high-energy physics and cosmology is a one-way traffic:

$$\text{High-energy physics} \rightarrow \text{cosmology.}$$

Nonaccelerator particle physics

Although the characteristic scale of weak interactions is 100 GeV, this did not deter physicists from learning much of weak interaction phenomenology through experiments at the available lower energies ever since the discovery of β decay. Similarly, even theories with characteristic scales at 10^{15}–10^{19} GeV are expected to leave their signals (albeit weak) in the lower-energy phenomena. Proton decay, neutrino masses and mixing, neutrino oscillations, double β decay and fifth force are such signals and experiments dedicated to the study of these phenomena provide us with indirect but important windows on the superhigh-energy scales.

The importance of deep underground laboratories for nonaccelerator physics experiments is well recognized. In this context, we must record here the unfortunate closure of the deep mine at Kolar, which was an important asset for this country, especially because of the absence of high-energy accelerators in this part of the world. An excellent opportunity to develop this facility into a first-rate underground laboratory for non-accelerator particle physics has been lost.

In spite of the importance of nonaccelerator particle physics experiments, these must be regarded as only our first and preliminary attack at the unknown frontier. These experiments can give us only indirect evidence on the physics at superhigh-energy scales. Just as the real nature of the weak force, namely that it is a gauge force mediated by gauge bosons, becomes manifest only at 100 GeV, in the same way the real nature of the unknown physics beyond the standard model will become clear only by experiments at the superhigh-energy scales.

Monopoles

Grand unified theories predict the existence of magnetic monopoles with masses of the order of 10^{16} GeV. If such superheavy monopoles exist in nature and if monopoles and antimonopoles can be caught in sufficient numbers and kept in separate 'bottles', then by letting the monopole and antimonopole collide and annihilate each other, we can create fireballs with superhigh energies (~ 10^{16} GeV) right here in the laboratory:

$$M + \overline{M} \rightarrow 10^{16}\,\text{GeV}.$$

*If there is such a law, prove it!

However, success of this venture obviously depends on our luck in catching these rare objects!

Planckian accelerator

None of the above avenues – historical research on the early universe, nonaccelerator experiments and monopole search – can compare with dedicated experiments in the laboratory directly bearing on the superhigh Planckian energies. Physicists cannot remain satisfied with indirect attacks on the superhigh-energy frontier. Planck energy must be attained in the laboratory.

This must be regarded as the most important problem in high-energy physics. A breakthrough in the discovery of a new mechanism of acceleration which can take us to Planck energy will advance high-energy physics much more than all the beautiful theories at Planck energy which the theorists are constructing. This will be a revolution.

Some of the ideas being pursued are: lasertron, wakefield acceleration, switched power linac, collective accelerator, laser-driven grating linac, inverse free-electron laser, inverse Cerenkov acceleration, plasma accelerator, laser beat-wave method*, etc. What we need are a hundred crazy ideas. May be, one of them will work.

A word about India. We need not feel disheartened by our lag in accelerator technology. Perhaps there is no point in repeating all the well-tried accelerator mechanisms (which may be irrelevant as far as the Planckian accelerator is concerned). We may be able to leap-frog on accelerator technology!

A look at the past history of accelerators will show that the growth of the energy of the accelerators over the years has been phenomenal[3]. In an overall sense, the energy of the accelerators increases by a factor of 10 in every 6 years. We interpret this exponential growth of the energy as an optimistic sign for the future of high-energy physics. Pessimists will point out that the required money as well as the dimensions of the accelerator also grow exponentially. This is true for conventional methods of acceleration. What we are envisaging are newer methods and newer technologies which will overcome these limitations.

For machines employing the same technique of acceleration, the growth lines have a shape that is not an exponential, but taper off. It is only the overall growth, including all types of accelerators, that is an exponential with the slope given above. This only shows that the growth of accelerators of a given kind has generally slowed down after the associated technology has matured and emphasizes the importance of new ideas of acceleration at every stage, in order to go further.

By an optimistic extrapolation of this exponential growth, one can show[4] that a Planck energy of 10^{19} GeV in the centre of mass (c.m.) system can be reached in the year 2086 AD. If this scenario is realized, the c.m. energies available for high-energy physics experiments in future will be as in Table 2. So, by 2062, we will produce the X bosons (the leptoquarks) and other objects of the grand unified theories, and by 2086, higher dimensions of space–time will no longer be hidden, superstring theory will be directly tested and quantum gravity experiments ill be done in the laboratory! Of course, entirely new things not contemplated by any theorist so far may be discovered.

The period we have to wait may look too long. But, if one compares it with the time which elapsed between our first glimpse of the weak decays (Becquerel's discovery of radioactivity in 1896) and the production and identification of the carrier of the weak force in the laboratory (1983), it is not much longer.

To put the whole thing into proper perspective, let us contemplate Maxwell's equations for electrodynamics,

$$\nabla \cdot \boldsymbol{E} = 4\pi\rho\,, \tag{1}$$

$$\nabla \times \boldsymbol{E} + \frac{\partial \boldsymbol{B}}{\partial t} = 0\,, \tag{2}$$

$$\nabla \cdot \boldsymbol{B} = 0\,, \tag{3}$$

$$\nabla \times \boldsymbol{B} - \frac{\partial \boldsymbol{E}}{\partial t} = 4\pi \boldsymbol{j}\,, \tag{4}$$

and compare them with the dynamical equations of the standard model:

$$\nabla \cdot \boldsymbol{E}_i + \cdots = 4\pi\rho_i\,, \tag{5}$$

$$\nabla \times \boldsymbol{E}_i + \frac{\partial \boldsymbol{B}_i}{\partial t} + \cdots = 0\,, \tag{6}$$

$$\nabla \cdot \boldsymbol{B}_i + \cdots + = 0\,, \tag{7}$$

$$\nabla \times \boldsymbol{B}_i - \frac{\partial \boldsymbol{E}_i}{\partial t} \cdots = 4\pi \boldsymbol{j}_i\,, \tag{8}$$

where i goes over 1 to 12 corresponding to the four electroweak gauge fields γ, W^+, W^-, Z and the eight gluons. The dots in eqs (5)–(8) refer to the complications

Table 2. Progress towards Planck energy

Year	1990	1996	2002	2020	2062	2086
Energy in GeV	10^3	10^4	10^5	10^8	10^{15}	10^{19}

*C. Joshi and his colleagues[2] at the University of California, Los Angeles, have succeeded in using this method to produce an accelerating electric field of 2.8 GeV/m, which is the largest coherent man-made accelerating field yet produced, and is 30 times larger than the limit imposed by radiofrequency breakdown in conventional accelerators.

arising from the nonabelian nature of the gauge fields of the standard model.

All the accelerators so far are based on electrodynamics. As compared to electrodynamic technology, the technology of the standard model is in a very primitive stage; we are perhaps at a level comparable to the study of electricity by rubbing amber on wool! We now know that electrodynamics does not stand alone; it is only a part of the unified electroweak dynamics. The deeper implications of the electroweak unification may be as profound and far-reaching as those of Faraday's unification of electricity and magnetism or of Maxwell's unification of electrodynamics and optics. Our understanding of QCD is at an even more primitive stage, because of colour confinement. But chromodynamics will be mastered and chromodynamic technology also will come. Electrodynamic technology led to acceleration of particles up to TeV energies. By releasing the forces of the standard model and putting them to work, the goal of acceleration up to Planckian energies may be achieved even earlier than the prediction above. 'Prediction is a difficult art, especially when it concerns the future'.

1. Abe, F. *et al.*, *Phys. Rev.*, 1994, **D50**, 2966.
2. Everett, M. *et al*, *Nature*, 1994, **368**, 527.
3. Llewellyn Smith, C. H, Oxford preprint, 1986, 34/86.
4. Rajasekaran, G., in *Proc. VIII HEP Symp.*, (eds. Pal, M. K. and Bhattacharya, G.), SINP, Calcutta, 1986, vol. II, p. 399; Rajasekaran, G., in *Particle Phenomenology in the 90s* (eds. Datta, A. *et al*), World Scientific, Singapore, 1991, p. 1.

MEETINGS/SYMPOSIA/SEMINARS

Third International Conference on Vibration Problems

Date: 27–29 November 1996
Place: Jalpaiguri, India

Topics include: Vibrations of beams, plates and shells; Thermal and mechanical vibrations of structures; Shock-induced vibrations; Random vibrations; Buckling and post-buckling behaviour; Acoustics and vibration (problems of physics); Vibration problems in bio-medical engineering; Structural dynamics and extreme load analysis; Inelastic behaviour of solids and structures; Elastic waves; Seismic response analysis and design; Elastic–plastic vibration problems; computational and numerical methods – their applications to vibration problems; Transport problems; Vibration problems in off-shore engineering; Vibration problems in nuclear power reactors; Recent developments in related fields.

Contact: Dr M. M. Banerjee/Dr P. Biswas
Co-Chairmen, Executive Committee, ICOVP-96
Vibration Conference Secretariat
Department of Mathematics
A. C. College
Jalpaiguri 735 101, India
Phone: 91-3561-23620/34221/22146/22107
Fax: 91-3561-23115

National Seminar on Environmental Impact Assessment of Small-Scale Mineral Resource Utilization

Date: 6, 7 October 1995
Place: Udaipur

Contact: Dr P. S. Ranawat
Department of Geology
M. L. Sukhadia University
51 Saraswati Marg
Udaipur 313 001
Phone: 294-28366 (off), 294-529986 (res)
Fax: 294-525959

IX International Conference on Plant Pathogenic Bacteria

Date: 26–29 August 1996
Place: Madras, India

Contact: Prof. A. Mahadevan
Chairman, IX ICPBB
CAS in Botany
University of Madras
Guindy Campus
Madras 600 025, India
Phone: 91-44-2350401
Fax: 91-44-566693

National Symposium on Environmental Dimensions of Palynological Science

Date: 11–12 April 1995
Place: Thiruvananthapuram

Thrust areas include: Palynology and environmental health (biopollution, aerobiology, marine palynology, allergens and immunology); Palaeoenvironment and ecology (palynostratigraphy, vegetational history, paleoclimate, ecology); Pollen biodiversity and conservation (micromorphology, systematics, cryopreservation); Pollen biology and biotechnology (development, physiology, pollen as vector); Palynology in agriculture and forestry (crop palynology, pollen selection, apiary, reproductive biology).

Contact: Dr R. Satheesh
Organizing Secretary, National Symposium
Environmental Resources Research Centre
P.B. No. 1230, Peroorkada, Thiruvananthapuram 695 005