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Keynote address: High energy physics in 2014 and its future

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Abstract. After a brief history, we focus on the present status of HEP and its possible future. Ideas to ensure a healthy growth of HEP in India are discussed. This involves a few major experimental projects in fundamental physics. None of these projects can succeed unless the crucial problem of manpower is solved. A few suggestions are offered towards this aim.

Keywords. History; Standard Model; Higgs boson; neutrinos; theories, experiments; manpower.

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1. Hundred years of fundamental physics

The earlier part of the 20th century was marked by two revolutions that rocked the foundations of physics: (1) Quantum mechanics and (2) relativity.

Quantum mechanics (QM) became the basis for understanding ATOMS, and then, coupled with special relativity, QM provided the framework for understanding the atomic nucleus and what lies inside. This history can be summarized as follows:

INWARD BOUND

$$\begin{array}{ccccccccc} \text{Atoms} & \longrightarrow & \text{Nuclei} & \longrightarrow & \text{Nucleons} & \longrightarrow & \text{Quarks} & \longrightarrow & ? \\ 10^{-8}\ \text{cm} & & 10^{-12}\ \text{cm} & & 10^{-13}\ \text{cm} & & 10^{-17}\ \text{cm} & & \end{array}$$

This inward bound path of discovery, unravelling the mysteries of matter and the forces binding it together – at deeper and ever deeper levels – has culminated, at the end of the 20th century, in the theory of fundamental forces based on nonabelian gauge fields, for which we have given a rather prosaic name:

The Standard Model of high-enegry physics

What is called high energy physics (HEP) is just the continuation of the era of discoveries that saw the discovery of: the electron, radioactivity and X-rays, the nucleus and the neutron, and cosmic rays and positron. HEP is the front end or cutting edge of the human intellect advancing into the unknown territory in its inward bound journey.

The construction of the Standard Model (SM) was complete in the 1970s. In the next four decades, experimenters have succeeded in confirming every component of the full SM. Higgs boson remained as the only missing piece. High Energy physicists searching for it in all the earlier particle accelerators had failed to find it. So the discovery of Higgs boson of mass 126 GeV at the Large Hadron Collider (LHC) at CERN in 2012 has been welcomed by everybody. This is a great scientific and engineering achievement.

More than 50 years ago when I started my research in High Energy Physics (HEP) which was then called Particle Physics, there was no Standard Model. We were all groping in the dark. I had the good fortune to witness the Standard Model being built step by step. After each step was taken, I learnt of it with a pang of regret that I did not do it. It was an agonizing period for me. But although I was not on the stage, I was almost in the first row, seeing history in the making.

By 1973, what we now call Standard Model (SM) was in place. This is now known to be the basis of almost all known physics except gravity. After that glorious period of the early 1970s, it has been a long sterile period of almost four decades. During this period, theorists have not been idle, but none of their theories has seen an iota of experimental support. Experimenters have also been busy, but all they have done in the last four decades is only to confirm one or other component of the full Standard Model with three generations of fermions and all their details. Even Higgs boson is only a part of the Standard Model.

Experimental HEP has not made a single discovery beyond the Standard Model, except for the discovery of neutrino mass that came 15 years ago. That is the importance of neutrino physics. Neutrino may be the portal to go beyond SM. Hence, expectations are high from the India-based Neutrino Observatory (INO) which is about to come up in Tamil Nadu. The INO laboratory (which will house a gigantic 50 kTon magnetized detector) will be built inside a mountain in Theni District and the Inter-Institutional Centre for High Energy Physics (IICHEP) that will function as the nodal centre of INO will be in the outskirts of Madurai City.

After the long sterile period, we now have at CERN, the Large Hadron Collider (LHC) which is capable of making discoveries. It can confirm or refute the numerous speculations that theorists have made. The day of reckoning for theoretical high energy physicists has come. That is the importance of LHC.

Unfortunately, in spite of the brilliant performance of LHC and its detectors, no discovery of anything beyond SM has been made so far, although many theories beyond SM have been constructed. But it is only the beginning. Many more years are to come. Hopefully, Nature will be kind to us and LHC will make discoveries. Our immediate situation is very positive. The LHC machine and its array of detectors ATLAS, CMS, ALICE and LHCb are all performing beautifully. Thousands of experimenters and theorists all over the world working together are bound to discover something new.

This is the importance of UNICOS2014 organized by Charanjit Aulakh and we have an exciting programme in front of us.

A word about India: More young people must be brought into HEP (both experiment and theory). This is the right time since LHC has started working. In India, many new institutions are opening up and we must see that strong HEP groups are built up in most of them. India is a big country. We must think big. No small measures or small steps will do. Our agenda is to discover whole new worlds.

2. Theoretical speculations

Grand unification: Grand unification idea is more than 40 years old. Although there are indirect evidences for its correctness such as the meeting of the three gauge coupling constants at around 10^{16} GeV, the crucial prediction of proton decay is still not borne out. On the other hand, one can turn the table around and claim that a spectacular success for grand unification is the natural explanation of the longevity of the proton. As one more evidence for unification, I would like to cite the meeting of the mixing angles in the quark sector with those in the leptonic sector.

Supersymmetry: This theoretical idea postulates the existence of a boson corresponding to every known fermion like electron and vice versa. This is a very elegant symmetry that leads to better quantum field theory than the one on which SM has been built. But, if it is right, we have to discover a whole new world of particles equalling our known world; remember we took a 100 years to discover all the particles of the SM, starting with the electron. Patience is needed.

Technicolour: A whole new world of strong interactions! Having lived through the old strong interactions in the 1950s and 1960s without knowing what it is, that is not my cup of tea. But if Nature had decided to repeat her tricks, who are we to refute her? There is one point that is striking. Technicolour had to be replaced by extended technicolour and then came walking technicolour. All this has to be done to take care of one phenomenological detail or other. Are we building epicycle after epicycle? After the discovery of Higgs, perhaps technicolour is not that popular. But the idea of compositeness cannot be ruled out.

Extra dimensions: Again we are building a whole new world of extra dimensions. It took us thousands of years to understand the four (three space plus one time)-dimensional world where we live. Now the theorists are constructing worlds with more dimensions added to the three plus one. Can this be done so fast? Many of the constructions in extra dimensions again remind us of Ptolemy: fitting phenomenological details with epicycles after epicycles.

Is there a Balmer formula? In the SM, all the 12 fermion masses are arbitrary parameters fixed only by experiment. Perhaps one has to extend the SM to include a theory of generations for understanding the pattern of fermion masses. Enormous amount of theoretical work has been done to solve this problem, but there is no memorable result.

However in 1982, Yoshio Koide found a remarkable empirical formula:

$$m_e + m_\mu + m_\tau = \frac{2}{3}\left(\sqrt{m_e} + \sqrt{m_\mu} + \sqrt{m_\tau}\right)^2,$$

which is satisfied to an accuracy of 1 part in 10^5. There does not exist any other relation of comparable accuracy in all of HEP (except of course the precision results calculated from QED and electroweak theory). But to this day nobody has succeeded in deriving the Koide relation from any theory. Is this the much needed Balmer formula which can serve as the guide-post for discovering the correct theory of generations?

Quantum gravity and string theory: The biggest loophole in SM is that it leaves out gravity. The most successful attempt to construct quantum gravity is string theory, in which field Indian theorists have made fundamental contributions. But, to incorporate it into physics, one needs experiments bearing on the Planck scale of energy which is far far beyond our present capabilities.

Preons: A brief look at the history of atoms, nucleons and then quarks would suggest that preons must be the next natural step. Nature may really be a never-ending layered structure. In the heydays of S-matrix and bootstrap philosophy in the early 1960s, it was even proposed that we have reached the end of the road and no more constituent structure below the hadrons was possible. But the subsequent development of physics has shown this to be wrong. We now know that hadrons are made of quarks. Are quarks in turn made of preons? Many preonic models were proposed in the past, but none is as yet required by experimental data. Down to a distance scale of 10^{-17} cm, quarks and leptons behave like point particles. Nevertheless, Nature might already have chosen one preonic model and future experiments might reveal it!

Dark matter: In contrast to all the above topics, dark matter is already established to exist. This discovery is due to astronomers. But its nature is left to physicists to discover. Dark matter is more abundant than visible matter (about 4–5 times). Dark matter also may have all the variety and complication of visible matter which we took 100 years to understand. So, characterizing dark matter by one or two parameters (like the relic density and the mass of the dark matter particle) may be far from the truth.

Cosmology: If current ideas in cosmology and astrophysics are correct, then early Universe provides us with a HEP laboratory where particle energies were almost unlimited. So it is believed by many of us that all our theories of HEP can be tested by appealing to events in the early Universe. At the risk of getting a flak from many of my respected colleagues, I would like to strike a note of caution.

There is no doubt that the era of ‘precision cosmology’ dawned with the measurement of CMBR anisotropies whose accuracy is awe-inspiring. The recent measurement of the *B*-mode polarization is creating waves. 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 controllable 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, laws of physics inferred from and tested in laboratory experiments can and must be applied to the study of the Universe and its history. In other words, the only healthy traffic between HEP and cosmology is a one-way traffic:

HEP $\longrightarrow$ cosmology.

New ideas on particle acceleration: Physics is not theory alone. Even beautiful theories have to be confronted with experiments and either confirmed or discarded. Here, we encounter a serious crisis facing HEP. In the next 25 years, new accelerator facilities with higher energies such as the Linear Electron Collider will be built so that the prospects for HEP in the immediate future appear bright. Beyond that period, the current accelerator route seems to be closed because known accelerator methods cannot take us perhaps beyond 100 TeV. It is here that one turns to cosmology and non-accelerator particle physics, such as from underground, underwater or underice laboratories. However, these must be regarded as only our first and preliminary attack on the unknown frontier. These can give only hints of new physics. Physicists cannot remain satisfied with hints and indirect attacks on the superhigh energy barrier.

There are many interesting fundamental theories taking us to 10^{16}–10^{19} GeV, but unless the experimental barrier is crossed, these will remain only as metaphysical theories. Either new ideas of acceleration have to be discovered, or there will be an end to HEP by about the year 2040.

In the last 30 years, many ideas on laser–plasma acceleration are being pursued. Using laser excitation of plasma wakefields, electrons have been successfully accelerated to 1 GeV in 1 cm (compared to kilometre-size conventional accelerators to get similar energies). So table-top accelerators are perhaps not far way. This may lead to breakthroughs that will help us to cross the superhigh energy barrier. What we need are a hundred crazy ideas. One of them may work!

By an optimistic extrapolation of the growth of accelerator technology in the past 70 years, one can show that even 10^{19} GeV can be reached before the end of the 21st century. But this is possible only if newer methods and newer technologies are continuously invented.

3. The experimental projects

I now describe a few major experimental projects in fundamental physics that are either on already, or being seriously considered in India, or must be started.

(1) The only experimental programme in HEP that is pursued so far in the country is the participation of Indian groups in international accelerator-based experiments. This is inevitable at the present stage, because of the nature of present-day HEP experiments that involve accelerators, experimental groups and financial resources that are all gigantic in size.

(2) While our participation in international collaborations must continue with full vigour, at the same time, for a balanced growth of experimental HEP in the country, we must have in-house activities also. Construction of an accelerator in India, in

a suitable energy range which may be initially 10–20 GeV, and its utilization for research as well as for training will provide this missing link.

(3) As already explained, known methods of particle acceleration cannot take us beyond tens of TeV or utmost 100 TeV. Hence, in order to ensure the continuing vigour of HEP in the 21st century, it is absolutely essential to discover new principles of acceleration, such as laser–plasma acceleration or something even newer. Here lies an opportunity that our country should not miss. (I have been stressing this at every opportunity for the past 30 years.)

(4) A multi-institutional neutrino collaboration is creating the India-based Neutrino Observatory (INO). A neutrino oscillation experiment using atmospheric neutrinos will be performed in a gigantic (50 kton) magnetized iron detector to be mounted inside a huge cavern that has to be dug inside a mountain in Theni District, Tamil Nadu. This detector will be even larger than the huge detectors which are taking data at the Large Hadron Collider (LHC) at CERN, Geneva. So our students will be able to work in the construction of such a detector and use it, right here in India.

(5) Search for neutrinoless double beta decay (NDBD) is the most fundamental of all neutrino experiments as it will tell us about the nature of the neutrino itself (whether it is a Dirac or Majorana particle). This will be also installed in the INO cavern.

(6) A low-energy neutrino experiment called low energy neutrino spectroscopy (LENS) which will detect the *pp* neutrinos from the Sun. These are the most abundant neutrinos from the Sun (amounting to more than 90% of the solar neutrinos) and have not been detected so far. Hence, LENS has the capability of revolutionizing solar neutrino physics once again. This experiment which will use indium-loaded liquid scintillator will be mounted either in the INO cavern or another existing cavern or tunnel inside a mountain.

(7) Astronomers have discovered that most of the matter in the Universe is not the kind we are familiar with. It is called dark matter since it does not emit or absorb light. Although this discovery has already been made, nobody knows what this dark matter is and only physicists can discover that. A dark matter experiment will be mounted in INO cavern (suitably extended). This has been called dark matter at INO (DINO) and this will be preceded by a smaller experiment at a shallower depth.

(8) A neutron–antineutron oscillation experiment is being thought of in India. In fact a workshop to discuss this, was held in Kolkata two years ago. Such an experiment will put India back in the world scene for the search for baryon number violation which has not yet been observed.

(9) A gravitational wave detector is being planned to be set up in India. This is the goal of the Indian gravitational wave observatory (INDIGO) project. Although this is of great importance in astronomy, direct detection of gravitational waves predicted by Einstein's general relativity is also an important area of fundamental physics. So we include it here.

Technology: Although all these projects concern high energy physics, nuclear physics or astronomy, one must not lose sight of the technology and material science component involved in all of them. Building an accelerator needs accelerator engineers; discovering new principles of acceleration needs laser and plasma physicists. The RPC-based magnetized detector to be set up in the INO cavern will require 30,000 sensitive detector elements and 3 million electronic channels. The NDBD, LENS and DINO will need sophisticated cryogenics, chemistry, semiconductor crystal fabrication and other techniques of modern material science. Construction of gravitational wave detector will require sophistication at an unimaginable level. Hence, execution of the above fundamental physics projects will lead to the development of state-of-the-art infrastructure in all these fields. This important off-shoot of 'aiming for the Moon' must be kept in mind.

4. Manpower creation

But where is the manpower for all this? None of the above projects can succeed unless the crucial problem of manpower is solved. A few suggestions are offered here towards this aim.

(1) Much of the manpower for the Department of Atomic Energy came from the innovative Training School started by Homi Bhabha in 1957. Inspired by this, INO started its own training programme six years ago. The scope of this programme could be enlarged to cover the other experiments. However, we need more people at the faculty level to train these young students.

(2) We have to contact those bright young Indian scientists who went abroad in search of fertile pastures and lure them back with assurance of those fertile pastures here. There are many good experimental physicists who would be willing to return. A high-level drive has to be undertaken to achieve this. Heads of scientific institutions must go with 'a blank cheque' during their travel abroad and offer jobs straightaway when they meet deserving candidates.

That is what Bhabha did in the 1950s and 60s and that is how the School of Mathematics, the Cosmic Ray group, the Radio Astronomy group and the Molecular Biology group, all at TIFR, were built by K Chandrasekaran, Bernard Peters, Govind Swarup and Obaid Siddiqui, all of whom Bhabha brought. (Of course the times were very different then, but those glorious examples can light our path even now.) Recently, the Chinese followed this path very successfully. There are many reputed Indian physicists abroad who can identify good candidates and help us in such a recruitment drive (the reverse brain-drain).

(3) Where are these new recruits to be placed? All of them need not and should not go to the established institutions such as TIFR, IISc or SINP. We must persuade the IITs, IISERs and the Central Universities to recruit the bulk of the returning experimental physicists. We have already got positive response from the heads of a few of these institutions and we must continue to try and extract similar response from the other institutions also. IISERs and NISER have been founded especially to attract bright youngsters into science. What better way to attract them than to show them the possibility of joining front-ranking fundamental science experiments in India? The bright students in their fourth year must be put into project work connected to one of the experiments in the list above.

(4) Many privately funded engineering institutions have come up in the country. Unfortunately most of them are money-making institutions rather than the money-spending variety. We need the latter. Academic institutions must earn, not money,

but the reputation of excellence in the advancement of knowledge. Nevertheless one must not write them off. Some of them are showing promise; they are capable of aiming for excellence. We may be able to induct good science and engineering faculty into them.

(5) I now come to the most important aspect of the manpower problem. It is a sad fact that because of the continuing neglect of our more-than-four-hundred universities and thousands of colleges, most of these languish in academic slumber. As our student power lies in these institutions, it is no wonder that all our plans for major scientific projects suffer from lack of manpower. So, it is clear that mobilizing the universities and coupling them to the National Science Projects is the only correct way forward. It will be a remedy for both these ills.

However, this is a gigantic task. I shall restrict myself to three brief points. Because of the importance of this problem, I suggest that DST should confer with UGC and come out with innovative solutions. Second, each one of us must try to influence the universities in the physical as well as intellectual neighbourhood of each of us and persuade them to facilitate the participation of their students in a major scientific project. Third, in many of the university departments, a large fraction of the faculty has been kept vacant for many years. These must be filled with experimenters who can contribute to one of the experiments in the list above.

There may be many more ideas, but what is needed is action.

Imagine the enormous excitement the students will get by seeing front-line fundamental physics experiments right here. No wonder students are not excited by science and hence do not enter the field in large numbers. This can be changed only by ensuring that experiments are done here.

Manpower in theoretical HEP: Theoretical HEP continues to attract the best students and as a consequence its future in the country appears bright. However, this important national resource is being underutilized. Well-trained HEP theorists are ideally suited to teach any of the basic components of physics such as QM, relativity, QFT, gravitation and cosmology, many-body theory, statistical mechanics and advanced mathematical physics as all these ingredients make up the present-day HEP theory. Ways must be found to absorb a large fraction of these bright young theoretical physicists in the universities. Even if just one of them joins each of the 400 universities in the country, there will be a qualitative improvement in physics teaching throughout the country. But, this will not happen unless the young theoreticians gain a broad perspective and train themselves for teaching-cum-research careers.