HEP research in the Indian context

I wish to make a few comments on the special section on high energy physics (*Curr. Sci.*, 1996, **71**, 109–127) in this note. G. Rajasekaran says that new ideas for particle accelerators have to be discovered or there will be an end to HEP by about 2010 AD. Is Indian science ready to entertain new ideas? The idea to discuss HEP in the 21st century itself is not new; it is a borrowed one. Ramachandran lists outstanding questions for the early 21st century suggested by Gross, Witten and Kane. There is nothing wrong in relying upon experts, but the country's science leadership almost always follows the frontiers set in the West. It is unfortunate, but true, that the research environment in our country is so sterile and conformist that an original mind has to opt out of the system to sustain creativity. I doubt, if C. J. Joshi whose leading contribution in the plasma beat wave accelerator has been noted by A. Sen, would have got necessary support had he been in India.

Idea-wise, I think alternatives to the experimental philosophy embodied in the 1911 Rutherford scattering experiment might be worth looking at. After all, the aim of HEP is to search for a fundamental entity and foundational theory; and for that maybe going to higher and higher energies doing Rutherford scattering-like experiment is not the right choice. A plethora of elementary particles could be just the energy clumps created due to accelerating fields, having no fundamental significance. Why cannot a few massless particles be the building blocks of matter? Though neutrino oscillation and finite non-zero mass are of current interest in view of the unresolved solar neutrino problem, a neat theory with massless neutrinos could be envisaged for composite structure of photon and this together with the electron, might be the basic constituents of matter. De Broglie did consider this hypothesis, however, at that time muon neutrino was not known and the objections to this idea essentially originated from quantum theory, and the massive photon predicted in this theory conflicted with massless photon. Recently the possibility of topological quantization as an alternative to quantum theory has been explored, and some significant advances in the knot theory made. In particular, the discovery of the Jones polynomial in 1984 which can distinguish mirror image of a knot, and Witten's physical interpretation given in 1988 using 2+1 dimensional field theory are important for the present discussion. One might revive the idea that neutrino is a space–time trefoil knot in the light of these new discoveries (H. Jehle in 1975 considered a trefoil model). These ideas are at present what Rajasekaran calls the 'crazy ideas', however, such alternatives need to be given attention since the physics at Planck energy and early universe is very speculative, and the standard model has at least two major drawbacks, i.e. large number (as many as 20) of adjustable parameters and mathematical weakness of dealing with infinities in renormalization procedure.

Rajasekaran's proposal for a task force of a mutidisciplinary team of experts for new discovery in accelerators is endorsed by Ramachandran. In the most successful laboratories the world over, this approach has been in practice since long; unfortunately in India such proposals adorn the recorded speeches of eminent scientists, and are seldom implemented. Unless the problems at implementation level are sorted out, the think-tank proposal by Rajasekaran is likely to remain wishful thinking.

Ramachandran notes the criticisms voiced against heavy funding for doing science for the sake of it, and tries to justify public support for HEP, citing its impact on other branches of science and high-tech fall-outs of accelerator R&D. In 1994, the *CERN Courier* highlighted the spin off and technology transfer from HEP research. Hoffman in an article (*CERN Courier* 1994, **34**, 7–14) provided an exhaustive discussion on the subject. My question is: what is the spinoff of HEP in India? Ramachandran has not given any example from India. Unless a definite 'market value' in terms of indigenous technology development is specified, the proposal for building high energy accelerator (in the range of 10 GeV–20 GeV) shall be an avoidable burden on the public funding.

S. C. Tiwari

1, Kusum Kutir,
Mahamanapuri,
Varanasi 221 005, India

Rajasekaran and Ramachandran reply:

S. C. Tiwari's comments appear to be somewhat obtuse, generally lamenting on the poor impact of Indian science. The special section on the contrary, is a status report on High Energy Physics in the global context, although it includes some suggestions relevant to this country. The criticism that proposals such as research on new methods of acceleration are seldom implemented in India is a valid one. The point of repeating such proposals, nevertheless, is the hope that it will stimulate some young minds to think in this direction. It may be appropriate to mention here that for the first time, a School on Accelerator Physics is going to be held in India (School on 'The Physics of Beams' at the Centre for Advanced Technology, Indore, 13–25 January 1997). Hopefully this will be the first step towards the goal of generating new ideas on particle acceleration.

Instead of pursuing the beaten track of scattering-type experiments, can one envisage alternative experimental routes to deeper regions of space–time? This is an interesting question. However, Tiwari's list of speculations on basic structure does not answer this question. They are rather vague and even if they could be made a part of a consistent theoretical structure, they also have to be confronted by experiments. What is required is a new experimental paradigm.

We shall not enter into the broader question of public support to fundamental science here. It is an important question already discussed at various fora.

G. Rajasekaran
R. Ramachandran

The Institute of Mathematical Sciences,
CIT Campus, Taramani,
Madras 600 113, India