

Materials Research Priorities

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Prof. C.N.R. Rao, Founder President of MRSI and, may I say, father figure to the Society, who has retained his tremendous enthusiasm for research as we saw just now, Prof. Chakravorty, Dr. Raghavan, Dr. Dipankar Banerjee, Prof. Subramanyam, distinguished materials scientists who have come for this meeting and friends, I thought that, in the brief time that has been given to me, I shall say a few things about what I consider as materials research priorities for India. Dr. Banerjee ♦ not Dipankar but our own brilliant Srikumar Banerjee ♦ and myself are now editing a book on Materials Research. It has got a little delayed because the receipt of a couple of articles got delayed. If you look at the subjects which have been covered, it is fascinating to see the excellent quality of the work that is going on in India in a variety of fields. We are looking both at the current scenario and the future projections. One thing is clear. More attention is needed to get technology pay-offs for India from the efforts that we are putting in.

Now one can divide the materials research work into areas though, of course, the boundaries are not sharp between basic research and applied research. In the field of materials research, you need advanced facilities, you need advanced analytical equipment for materials characterization. Unless you have well characterized samples, all data and results that you get become practically useless. This happened, for example, in the early days of high-Tc superconductors. Basic research is important, it is a cultural necessity. Any civilized country must provide an opportunity for its highest intellects to work on problems of their choice. Applied research is also necessary to catalyze indigenous technology growth. Research instrumentation has to be developed and there is weakness here. Of course, there are exceptions. A great deal of instrumentation has been developed in the Department of Atomic Energy and some other laboratories and institutions. You heard just now how Prof. CNR Rao ♦s group built the 15-tesla magnet for his magneto-resistance studies. But our instrumentation efforts are inadequate.

Frontier areas of basic research are often decided by the technology imperatives of developed countries. They have already reached a high level of technology development. They want to carry out research to reach even higher technology levels. Of course, we must be in these areas, pursuing them as basic research and also as an investment for future - or, if possible even current- technology development. At the same time, we should look also at the problems which are of immediate relevance for our own country.

Let me come now to the Synchrotron Radiation Sources (SRSs) which we are building for the first time in India at the Centre for Advanced Technology, Indore. Building an SRS is like building a nuclear reactor. It requires not one technology but a variety of scientific and engineering disciplines. For building a reactor you need reactor physics, water chemistry, reactor engineering, control instrumentation and a range of other engineering disciplines. In a similar way, for building an electron accelerator, an electron storage ring, it requires knowledge of high vacuum,

RF systems, high voltage, beam dynamics and other disciplines. The first electron storage ring INDUS-I, which gives vacuum ultra-violet and soft x-ray radiation is already functioning and INDUS-2, which is a 2.5-GeV ring with insertion devices and will give hard x-ray radiation, will be completed next year. INDUS-2 will have a couple of dozen beam lines. Here we have a lacuna. Eight of the beam line instruments are being built by BARC and CAT and a couple by IUC-DAEF. But not enough people are coming forward from the University system to build beam-line instruments, though there are excellent scientists in the country who would want to do basic research with the instruments once they have been built. We should examine what difficulties are holding them back from participating in instrumentation development and remove those impediments. Of course, it is time-consuming to build a beam-line equipment or any other advanced experimental equipment. But, unless we do that in a big way, we will not be able to catch up, as Prof. CNR Rao said, with the countries abroad.

If I come to applied research, a very successful example is the nuclear materials development program. Dr. Ganguly is here, one of the few persons who has worked with all kinds of nuclear fuel \diamond plutonium, uranium-233, natural uranium, enriched uranium. In applied materials research also, you need advanced facilities, advanced analytical equipment. In addition, you must also have production facilities and all this leads to indigenous technology development. The nuclear materials development program is not an isolated piece of development. You have to develop fuel, you have to develop cladding material, you have to develop fuel assemblies and test them for quality. You have to make the whole program consistent with the reactor's objectives. Dr. Rodrigues is here, who did a great deal for the development of fast breeder reactor at Kalpakkam, which is the second stage of our nuclear power program. Here we need mixed plutonium-uranium fuels, thorium-based fuels, breeding ratios have to be estimated. In applied materials research, we have been successful in specific advanced mission-oriented areas.

There has been some debate in recent literature between conventional science vs. strategic science. Personally I do not see a sharp border between the two. Take my own area of high pressure physics. High pressure as a parameter is fascinating. If you heat a material, by the time it expands by 10%, it has melted. But you can squeeze a material to half its volume and more, and still do experiments at room temperature or at other temperatures. You can do under isothermal conditions or you can send shock waves inside. Shock waves inevitably heat the material, along with compressing it. We started working on transition metals because Shri C.V. Sundaram brought a paper from Cambridge by Pettifor, who said that here the phase stability is decided by d-band occupancy. As you know, if you squeeze a material, the bands expand and there could be an s-d electron transfer and a phase transition can take place. We have played around with phase transition pressures; you can add vanadium to titanium, for instance, and bring down the transition pressure. Now, one could call this phase transition study as conventional science.

We have worked on rare earths and on actinides. Thorium is a 5f metal and if you want to get a good Equation of State (EOS) for thorium and the right pressure for its transition, you must study the broadening of the 5f-band. This can be very exact using the density functional method and a powerful computational resource. My colleagues have published papers on this in Physical Review. You could call this conventional science. But when we work on plutonium, which is a few atomic numbers up the periodic table and do the same thing, you could call it strategic science. If you want to predict exactly the yield of a fission weapon, you must know

exactly the EOS of plutonium and uranium. So, where does conventional science end and strategic science begin? So my feeling is that when we are doing materials research ♦ or, for that matter, any kind of research ♦ we should not get too worked up about such semantic boundaries between basic research vs applied research, small science vs big science, etc.

We should also forget about what the institutional attachment of a scientist is and concern ourselves only about the quality of his research. I remember the days many years back when I and Milan Sanyal were trying to get our group into the collaborative access team for building a beam-line instrument for the Advanced Light Source at Argonne. The potential members of the team were the Brookhaven National Lab, Argonne National Lab, a U.S. University and BARC. While the design discussions were going on, institutional affiliation did not concern anybody. We were willing to build a part of the instrument at our cost. Incidentally, the reason we got out of the project was that the question of paying for the maintenance of the beam line came up. Imagine having to continuously pay a fraction of the salaries of two American technicians for maintaining the beam line!

Why do we do materials research? If someone tries to understand and find out the behavior of materials just from the point of view of acquiring knowledge we don't have to ask why the study is being done ♦ only about the quality of the work. . But if the work is articulated as being related to technology development, the linkage to Indian industry has to be established early. What is the purpose of technology development, in general? Creating national wealth; improving quality of life, particularly in rural areas; and enhancing national security! And here comes the question of technology foresight. This is different from technology forecasting. If you ask if human cloning will work or will DNA computing become practical and look at it purely from a scientific and technological angle, that is forecasting. But add to it assessment from an economic, social, environmental and (in the case of human cloning) ethical points of view, it becomes technology foresight. Technology foresight analysis helps to decide what are the critical technologies for a country at any point of time. You would get different answers, depending on whether you are from USA or from India. We can do this for all materials ♦ related technologies. So when you talk of technology, you have to consider how it feeds into the Indian system. Are there Indian industries which are ready to take over the development of these technologies, or how they should be helped to do so? This was what was done for the nuclear materials ♦ related technologies.

Let me close with one last thought. In my new assignment as Principal Scientific Adviser to Government of India, it has become very clear to me that rural development-related technologies must be given the highest priority. Two thirds of Indians live in villages. Increase in agricultural productivity through use of known science and technology in the poorest rural parts of the country can take the people living there above the poverty line. Beyond that agriculture productivity can have limits, unless simultaneously we can create jobs outside the primary farm and related sectors. And this is being done in the Tenth Plan. Value addition by food processing, for instance! The other kind of technologies which can be developed with rural areas in mind can also become important, for adding value and for creating new job opportunities. In fact, sometime I am going to suggest to Prof. Subramanyam and Prof. Chakravorty that MRSI should have a new subject group on rural development-related materials. I feel many of us can improve the materials which rural artisans and agriculturists use. TIFAC, which is a part of the Department of Science & Technology has done some interesting work on use of coir, bamboo

etc. and Prof. Ramachandra Rao showed me recently in NML a simple device to remove drudgery from the work of a blacksmith. So we can contribute to materials and materials processing technologies related to rural development. MRSI over the years has grown from strength to strength in a way even I did not expect when Prof. CNR Rao started it and we should also now see how much we can contribute to rural development.

Subject Group on :

Materials for Rural Development

We are considering the formation of a Subject Group on ♦Materials for Rural Development♦. This will cover Building & Irrigation Materials, Materials for Tools, Clay Materials, High yielding & resistant seeds, Natural & Synthetic Fertilizers, Water-Conservation, Storage & Optimum consumption, Solar Energy Converters, etc. Suggestions on possible activities and for the formation of a core group are most welcome.

S V Subramanyam