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Science and industry

I found the editorial of the 25 January 1996 issue particularly intriguing and significant. The editors are clearly pointing the journal in the right direction – toward greater contact with industry. In the present world climate for science, it would be suicidal to do anything else. I do not believe that the readers of *Current Science* have been sufficiently exposed to the science – the evolution – of current thought on the value of science not connected to industry and applications. Unknown to most, the model of post World War II American 'pure' or 'basic' science – colonized by the lure of money from the public purse, and mesmerized by the big gadgets of big science – had wreaked havoc on the real purity of 'pure' science. Very few scientists like Tennyson's Ulysses chase 'knowledge like a sinking star'. They are almost totally constrained by money, by fads, by peers, by bureaucracy with necessarily 'political' agendas. As money becomes scarce, US academics, for the wrong reason, look to industry. The major correct reason for linking to industry is that is where interesting, exciting problems – the interface with unexplained manifestations of nature appear. The other reason, of course, is that it is a way to serve the public good, since it is public money we use.

However, paragraph 2 of the editorial is simply wrong. It is the misperception foisted on to the public by the American academics. University–industry interactions are *very weak* compared to the total effort, and they contributed at best in a minuscule way to making these 'great economic powers'. I showed at great length in my book, *Lost at the Frontier* (ISI Press, 1985) and in subsequent papers how the government support of science 'ruined' the pre-war American *real* science where independent universities with modest help from the private sector really linked these two worlds. In chemistry, Speed Marvel and Roger Adams were great examples. In physics, contrary to the general view, Bell Labs was the mecca where *applications-driven* basic science was taken to Nobel heights. The transistor was driven by the most elementary and obvious need to eliminate the filament current drain. It did not originate in quantum mechanical calculations of energy levels. The post World War II government entitlement programmes for academic research while winning lots of Nobel Prizes in abstract science (at some billions of dollars of public money for each) *severed* the university–industry ties.

What Indian scientists should learn is that US (and worldwide) major industries between 1991 and 1993 made the discovery that there is no R.O.I. on basic research. The bogus self-serving economic analyses by academics to the contrary, *basic (undirected) research not connected to a product can never pay to the funder.* (This is a precise integrated statement which should not be truncated at any point, if attributed to me.) The proof is absolute and simple. If 100% of all major corporations that practised the opposite for 40 years (lavishly funding such) have realized their mistake, how can an academic analyst prove that they are wrong. If a company cannot make basic research lab–development lab interaction pay, how can any reasonable scientist claim that the threadbare connection between the results of a lone university researcher be effectively coupled to any one of a thousand companies anywhere in the world.

Indeed the logic of this argument can take us much further – toward 'privatization' (with all sorts of incentives) of *all* undirected truly basic research. Lacking space I can only refer the reader to Cambridge biochemist Terence Kealey's just published book *The Economics of Scientific Research* (St. Martin Press, 1997). It is an absolutely essential resource for anyone thinking about government and industry support of university research. Kealey has marshalled facts and arguments which will dominate the discussion for many years. The core of Britain (and Germany's) basic science was funded in its greatest years by private philanthropy.

I write not as an opponent but as a strong, but highly disciplined advocate of such university–industry (U–I) coupling. MIT and Penn state are and have been neck and neck in the competition for the leading university position in U–I coupling, with nearly $50 million/yr involved. And in that, the Materials Research Lab I started and directed for a quarter of a century remains the outstanding player.

The salient new points for Indian thinking about U–I coupling in today's climate are:

1. The dollar amounts are much smaller than in government grants (factor of 5 to 10).
2. The necessary response times are much shorter (factor of 5).
3. The coupling partners are in globalized setting so that US companies can couple to Indian universities and Indian or Japanese companies can couple with US or European universities. (Our lab does this very effectively today.)
4. Doing this kind of quick response research aimed typically at maximizing the functions of 'cheaper, faster, or greener,' not necessarily performance only, requires a certain mindset not very common among (American) academics.

Hence I subscribe enthusiastically to your editorial's last paragraph: 'it *will* enrich the Indian scientific scene'. But I add to your need for 'imagination and drive' the willingness to change. Perhaps Indian science could learn the lesson (totally ignored by post World War II American science), which Albert Einstein put to a Caltech adulatory crowd in 1931: 'It is not enough that you should understand about applied science in order that your work may increase man's blessings. Concern for man himself and his fate must always form the chief interest of all technical endeavors, concern for the great unsolved problems of the organization of labor and the distribution of goods – in order that the creations of our minds shall be a blessing and not a curse to mankind. Never forget this in the midst of your diagrams and equations.'

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Flawed policy of DST and DoE

I wish to state that the statement about DST imposing two draconian conditions on S&T projects submitted to them is not true (P. N. Andhare, *Curr. Sci.*, 1997, **72**, 158–159). As far as support for basic Science and Engineering Research is concerned, the scientific merit is the only ground on which funds are provided. Only on projects of applied nature, it is the policy to nurture linkages between research community and industry, and preference is given to projects involving industrial participation. During the year 1995–96, Science and Engineering Research Council has supported over 300 projects and spent Rs 37 crores approximately and scientific merit has been the only criterion in supporting these projects.

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P. N. Andhare accuses the DST and DoE of imposing 'draconian conditions that they nip in the bud *any* S&T proposal even before its evaluation on scientific merit' (emphasis added). The two conditions are (paraphrasing) (i) every proposal must have financial support from industry, and (ii) industry must give an undertaking in advance to productionize the R&D resulting from the proposal. In my capacity as the Chairman of the Programme Advisory Committee on Robotics and Manufacturing of the Department of Science and Technology, I would like to make it clear that the above-stated requirements are *not* preconditions for a proposal to be funded by DST.

Let me begin by giving a few statistics: The PAC-RM (and its predecessor, the PAC on Manufacturing Technology) have over the years made it a policy to nurture interaction between the research community and industry. Since 1991, the PAC has funded (after approval by SERC) a total of 39 projects at an outlay of Rs 512 lakhs. Of these, 18 projects have received partial funding from industry and other agencies to the tune of Rs 178 lakhs. Thus, the industrial support has been approximately one-third of the total funding. On the other hand, it can be seen that the majority of projects funded by PAC-RM have *not* received any industrial funding. Thus, Andhare is not correct in stating that 'any' project must have industrial funding in order to be considered. The statistics above (from a PAC that is among the most 'practical' in DST) bear this out.

Speaking as an individual, I would say that in order to qualify for funding, a research project must *either* consist of top-quality basic research, *or* address a problem of relevance to industry. In the former case, the criteria for assessing a project are the familiar ones, namely the past record of the researchers, the likelihood of the outcome of the research being published in top-quality journals, and so on. In the latter case, a major criterion for judging the relevance of a project to Indian industry must surely be the willingness of the industry to underwrite the cost of the project. There is nothing unreasonable about this criterion.

Unfortunately what happens far too often in our country is that people try to pass off as 'practical' projects whose only notable feature is a total absence of any novel theoretical ideas. Thus the operative presumptions seems to be that if a project will not contribute to basic research, it must therefore be deemed to be 'practical'. In such situations, I see nothing wrong in calling the bluff of such proposers by asking them which industry is interested in their work, and why such industries are not paying at least a part of the cost of the project.

Let me repeat that if a person is doing basic research work at an internationally competitive level, then industrial participation is not called for at all. On the other hand, stringent steps must be taken to guard against persons trying to pass off third-rate theoretical or 'applied' work as 'practical' R&D.

Finally, I cannot agree with Andhare's railing against 'market' forces in S&T. Whether he acknowledges it or not, there are *always* 'market' forces at work in every arena, not excepting S&T. Even carrying out 'basic' research is subject to 'market' forces. Why else do new ideas suddenly become 'hot' while others becomes 'cold', even in purely theoretical subjects such as mathematics? In order to publish a paper, even in purely theoretical subjects, it is necessary to make a substantial contribution *on a topic in which the research community is interested*. No journal will publish a paper on a topic that is deemed by the community to be outdated or not of interest to anyone. Unfortunately what some persons want in our country is not academic freedom, but rather freedom from accountability. But that would be licence and not freedom.

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