Science funding in India

The letter from Marvin Herndon[1] forces me not to rebut by responding, but to support and extend his remarks – since I have been studying and writing on US science policy for 30 years. I have written a book[2] and over a score of papers, letters to editors, and congressional testimonies on the topic of peer review and alternative funding mechanisms (see rustumroy.com).

Returning to Herndon's letter, while the advice in the penultimate paragraph is all solid, it does not do justice to the even bigger policy issues. Certainly his advice not to copy the present US system is an absolute for India. A major downside of the 'NSF-type' system was ignored by Herndon: The absurdly high cost in the wasted time of the best scientists of the country (because proposal writing and massaging and selling cannot be left to postdocs or junior faculty). The consensus estimates are that we spend a third of our time at this. (And note for perhaps 50% of applicants it yields nothing at all, time averaged.)

But I return to other even more serious objections to the system. The creation of the NSF resulted from a misreading of Vannevar Bush's report to President Roosevelt: 'Science: The endless frontier'. While Bush, Dean of Engineering at MIT, did indeed champion 'basic research', what got lost in the climate of the 1950s in the US was the enormous body of other boundary conditions that Bush also recommended. (They are dealt with in detail in chapter 1 of the book.)

The key issues countries like India and China face are: (i) How much public money can a country allocate to activities which can never repay the paying public? (That is, how remote from reality can 'basic research' get? Is particle physics and radioastronomy an appropriate topic for public funding?) (ii) Since 50 years of wide and deep experience has conclusively buried the 'linear theory' of science policy, what should be the new paradigm? The 'linear theory' can be summarized thus: Basic science leads to → applied science leads to → technology leads to → prosperity.

Most science agencies (excepting the world's largest one, United States Department of Defense (USDoD)) bought into this theory, as did the newly emerging major industrial research laboratories in the 1950s to some degree: GE, IBM, Rockwell, Dupont.

By the seventies, historians of science like Kranzberg at Georgia Tech and de Solla Price at Yale had thoroughly debunked this theory. Price summed it up in an aphorism, 'Thermodynamics (= science) owes more to the steam engine (= technology) than vice versa'.

The historical demise of the linear theory came in 1994–95 as all major industries worldwide recognized their error, and started to demolish their unconnected 'basic research' laboratories. The disappearance of these magnificent centres of research at Bell Labs, GE, IBM, Phillips Eindhoven, is proof positive that the linear theory does not work even in such a tightly-linked system as a single company. The long-standing alternative to the 'linear theory' and the 'NSF', the peer review baggage it created, has been used by most of industry worldwide, and by the world's largest government research unit, the USDoD, and within that agency its very first unit which many agree is still the US premier research supporting agency, the Office of Naval Research (ONR).

For short, it is: 'Applications-driven basic research', as it was called at Bell Labs. I believe it is the only socially responsible theory, in addition to being the most effective.

To those who, ludicrously uninformed, parrot the statement, 'Sure, peer review is very bad, but there are no alternatives', one has to say, 'Circumspice' (look around). In all of industry, informed managers make their decisions after a small committee advises. At ONR, the informed manager makes the decision sometimes after getting an opinion on the telephone from others. Those managers obviously use as key parameters, … the idea proposed, the track record of the proposer, the likelihood of success, the scientific reward which might justify high risk, etc.

The interested reader on ONR philosophy and procedures can refer to Paul Gaffney's[3]. It is a key comparison of the 'Linear Theory' with 'Applications driven-basic research.'

Another fund distributing mechanism was the 'formula funding' used by the US Congress, for example, for all 'Agricultural Research' at US universities for 100 years. Today such a mechanism can be easily tried and improved by modulating that formula by a 'performance modifier' using objective data such as citations, and money brought in from other sources, etc.

All these considerations support Herndon's advice to India to avoid copying US NSF and US NIH at all costs.

1. Herndon, J. M., *Curr. Sci.*, 2005, **88**, 1714.
2. Shapley, D. and Roy, R., *Lost at the Frontier*, ISI Press, 1985.
3. Gaffney, P., *Bull. Am. Ceram. Soc.*, 2002, **79**, 57–61.

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Shallow water reservoirs and vegetation

Water scarcity is one of the prime problems in Tamil Nadu. Perennial rivers with dams across them have become sources of dispute among the southern states of peninsular India. Tamil Nadu ranks first with respect to non-availability of sufficient water for human use, irrigation for raising crops and for fishery. The three famous lakes, viz. Kodaikanal lake, Yercaud lake and Ooty lake in Tamil Nadu are also under severe pollution threat. However, only limited information is available regarding the small reservoirs and water bodies, which offer much scope for both irrigation and enhancing inland fishery. In this connection, such freshwater bodies, spreading over Tamil Nadu, are of ecological concern.

It is quite common that sewage water admixtured with river water or raw town sewage as such, is discharged into many of the perennial and seasonal tanks. Many such water bodies harbour floating vegetation like lotus, *Eichhornia* sp., *Pistia* sp. and