on the unit disk whose range is in the right-half plane. Given a moment sequence $c$, associate with it a function $F$ on the unit disk

$$F(z) = c_0 + 2c_1z + 2c_2z^2 + \cdots.$$

The Caratheodory problem is the problem of finding necessary and sufficient conditions on $c$ so that $F$ is a Carathéodory function. This condition is that, for each $n$, the Toeplitz matrix $T_n = [c_{i-j}]_{i,j=0,1,\ldots,n}$ be positive-semidefinite. If each $T_n$ is positive-definite then the distribution function $\mu(\theta)$ for the measure in the trigonometric moment problem is unique and

$$d\mu(\theta) = \text{Re}\, F(\theta)\, d\theta.$$

This shows the equivalence of the two problems.

Since the unit disk and a half-plane are conformally equivalent, this problem is the same as that of analytic functions mapping the upper-half-plane onto itself. Such functions are called *Pick functions* and there is an integral representation for them due to Navanlinna.

All this would suggest that this is a rich subject where classical function theory, harmonic analysis and functional analysis come together. One of the very spectacular applications of this theory was the work of C. Loewner on operator monotone functions.

In the recent years there has been significant work on extensions of these ideas to matrix-valued analytic functions. Much of this has been motivated by problems in systems control engineering, particularly the $H^\infty$-control theory.

The book under review is a collection of papers on interpolation and completion problems. It has eight papers, three of which deal with completion problems and five with interpolation problems. All the authors are prominent workers in the subject. These papers should be of interest to experts in these areas, who are already familiar with the literature and know where the heart of the matter lies.

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**Constructive Dissent: A Case Study in Science & Technology Policy-Making. Lost at the Frontier.** Deborah Shapley and Rustom Roy. ISI Press, Philadelphia, PA 19104, USA.

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Published in 1985, the book bearing this intriguing title demonstrates the legitimacy of constructive dissent on policies relating to public funding in an open society. It focuses attention on the link between policy-making and national objectives in science & technology (S&T). It also illustrates the dangers of distortion of 'public interest' values over time even when a national policy is initially established on the basis of a broad consensus.

In a report prepared for President Roosevelt in 1945 by a Committee, headed by Vannevar Bush, of scientists and technologists outside the government hierarchy, the image of science as 'an endless frontier' was created. The report led to the establishment of the National Science Foundation and initiated US Federal funding for the development of basic and applied science. The Vannevar Bush Report highlighted the major contributions of science to the war effort and recommended that government should step in 'with generous and stable funding for research, especially basic research in universities'. The report described basic research 'as one of the steps in a chain of endeavours that leads to industrial advance, better public health and stronger national defence', and stressed the 'interconnectedness' of basic research to other parts – 'to applied science, to engineering, to technology, to public national needs'.

Shapley and Roy's book is a critique of the manner in which these recommendations were implemented in reality over the period 1950–1980. While some people anticipated 'a science-based industrial revolution', they are sceptical if 'the USA science profession (as structured in 1985) will serve that revolution as well as it should'. The theme of this closely argued book, which the authors call an 'experiment in science criticism', is 'the severance of ties between basic science and applications' and its adverse impact on many areas – industrial competitiveness, translation of innovation into saleable products and education.

## US Science funding (1950–1980)

The Vannevar Bush Report resulted in the creation of the National Science Foundation. The criticism of Shapley and Roy is about the effect of 'basic research is the best' mindset which became the dominant approach and which vested basic science 'with its own protected keep'; consequently, they point out that the 'interconnectedness' concept was effectively ignored. Alan Waterman, the first Director of the National Science Foundation is quoted in the book: 'basic research has certain characteristics which.... distinguish it from other forms of scientific activity. *The search is systematic but without direction save that which the investigator himself gives it to meet the challenge of the unknown. He is strictly on his own, guided primarily by his interest in learning more about the workings of nature*' (emphasis added). The implication was that basic scientific research, something separate by itself, yielded far-reaching benefits although the incubation period between discovery and product could be even 30 years. This belief led to many proposals for grants (even those of obvious potential practical use) being sponsored in the garb of undirected basic research to win acceptance. This attitude percolated to industry in due course so that even major industries rarely made use of the findings of their own 'gleaming laboratories'. The consequence was easy to see: when times got tough, they cut back much long-term applied work and 'closed down basic research to hunker down for what remains ... a long winter of discontent'. It required the shock of sustained competition from Japan for the US industry to wake up and discover the true importance of basic as well as applied science. The authors trace the decline in creativity and innovative skills and the consequent adverse impact on the national economy to this biased implementation of the Vannevar Bush Report's recommendations.

## The 'basic research is the best' mindset

The authors delve into the history of the US science policy over the period 1950–1980 and trace the impact of the dominance of the 'basic research is the best' mindset on several related aspects of public interest. For example, NSF was extremely reluctant to deal with any science-oriented matter outside of basic research, such as reviewing science-related projects emanating from other US government agencies (such as the Navy). The authors believe that this attitude resulted in the institutionalization of the drift of science in 'an increasingly convenient, diverse and headless fashion'. The original recommendation that science requires a certain freedom and that scientists are best qualified to run their own affairs was institutionalized within this narrow perspective and successive science programmes turned out to be derivatives from previous efforts. This was also true of peer reviewing, which was the basis for acceptance of proposals for grants. The overall result, say the authors, was strong basic research and sometimes weak management of applications.

We should remind ourselves that the book is dated 1985; in fact, perhaps the most visible evidence of public awareness and discomfiture came when the space shuttle 'Challenger' failed at launch (early 1986) with a toll of all the astronauts (including a civilian teacher) on board. This might be considered the symbolic point when problems of team-based development and interplay of disciplines were laid out for the public.

The biased interpretation of the recommendations of the Vannevar Bush Report went even further. It percolated to universities wherefrom graduate students emerged believing that fundamental discovery, not invention, is the highest goal of science. This attitude impeded the achievement of excellence in university education in applied science. Eventually, when funding for basic research was curtailed, it produced a backlash of reduction in talented graduates pursuing Ph D or other science-oriented avenues.

## Synergy between basic research and commercial exploitation

The authors cite the synergistic management, over the same period of time within the Bell System, of human resources for basic research and development of commercially saleable end-products as evidence of how basic research, if properly directed, could benefit society. Electronic switching, material science discoveries and the transistor, the evolution of discrete device transistors and of integrated circuitry and the chain of discoveries (detection of noise from space, millimeter wave spectroscopy) – these examples are recalled to bear testimony to the value of a 'systems engineering' approach in everything that the Bell System embarked upon. A similar synergy had been witnessed in the agricultural sector also in the 1950s and 1960s. Vannevar Bush is again quoted: 'plants more resistant to disease, adapted to short growing seasons, prevention and cure of livestock diseases, control of insects, better fertilizers ... all stem from painstaking research'.

The general malaise, the 'industrial research drift', is seen as the ultimate result of flawed science policies, which did not promote this synergizing effort and systems engineering.

The impact was indeed felt in many ways: despite many obvious achievements in technology, the decline of innovation was reflected in growing loss of economic dominance. Lack of enthusiasm for Ph Ds to stick to science-oriented careers or even in pursuit of science-related advanced applications (nuclear engineering, for example) was perceptible even to a casual observer. During the years of Reagan and Bush presidencies, for reasons not connected with science *per se*, allocations for scientific research were reduced and focal attention was on defence-oriented development, with a consequential constraint placed on free flow of information and in some instances on international participation by US scientists. Concurrently, the strongly articulated political philosophy in the US of reducing government presence in many fields masked calls for national industrial policy planning.

The by-now-compulsive comparison with Japan is made: in Japan, it is stated, 'the tense but symbiotic relationship between the government, trade associations (which undertake joint research) and industry' (despite rivalry within the system and furious politicking over major decisions) enabled industry to become globally competitive. 'Japan has effective research and development on harbour and river management, postal service, high-speed rail, fire prevention and earthquake engineering', say the authors; unburdened by the myth of 'basic research is the best', Japan has been able to set consistent goals, assign institutions for the tasks, and develop practical technology.

## Science and society

By far the most interesting part of the essay for the Indian reader is the examination of the relationship between science and society. The authors enunciate two simply stated themes as the basis: one is that '.... a new age is dawning (in USA) characterized by an unparalleled impact of science and technology on our economic system and its products'; the second is that '... *rationale for federal funding for basic research ignores key changes in our society*'. They draw parallels from the futuristic concepts developed in Naisbitt's 'Megatrends': (a) the future strategic resource is not merely capital but also information and knowledge; (b) value is increased not by labour but by knowledge; (c) products offered on the market will be differentiated by their scientific content; (d) since knowledge increases economic value, the power associated with knowledge will be exercised not by the few but by the many. This explains the impetus to the formation of large numbers of small hi-tech companies exploiting scores of niche markets. For scientists this means that they have to operate in a volatile, fast-paced environment which demands reduced lead-time between scientific and technological research and its applications; they will also face sudden calls for re-design. All this implies that '*university scientists may be dragged from their ivory towers by the entrepreneurs, whether they like it or not*' (*emphasis added*).

## Shapley and Roy recommendations

Shapley and Roy conclude their review of the relationship between science and society with the plea that the linkages between science and applications have to be strengthened. The successful US effort in defence and space illustrate, the authors state, the value of synergized organizational effort between basic and applied science, keeping application as the primary goal. Toward this end, they recommend the following: formation of an advisory National Board which will articulate visions for the government and industry (the vision will expand the endless frontier), encouragement of the formation of sound science-based companies; reconstitution of management priorities, promotion of industry–university partnerships; exchange of talent between universities and industry for specified durations and tasks, reconfiguration of the university system with focus on 'science for society', requirement for scientists and their students to devote some part of their time, *pro bono*, to general education in the sciences, commitment to free flow of information. The authors suggest that the sum spent on basic research should be considered an overhead for an enlightened society. Even so, they suggest revision of the procedures for federal grants such as to count the value of scientific work done by citations from select 'professionally excellent' journals, award funds on the basis of past performance and not the size of the proposal and make all awards relatively long-term (3–5 years).

The essay is unfair in its assessment of the value of actual achievements in basic and applied science. The essay does not recognize, for example, that the leadtime between a 'discovery' or 'invention' and its commercial use had begun to shorten (much less than the 30 years mentioned) by the eighties and that the compulsions of international competition have ushered in the directional change which the authors urge.

Developments in the last decade have validated the Shapley–Roy criticism. In a 1984 essay 'The value of fundamental science' (*Sci Am.*, **251**, November 1984), the Nobel Laureate Leon Lederman, mentioning several instances of how fundamental science directly contributes to commercial opportunities for public benefit (with many more potentially valuable leads), suggests a classification of priorities that clearly covers Shapley–Roy concerns. First, identify the fields of science that are the most remote from application and deserve to be called fundamental; secondly, trace the ongoing efforts in fundamental science discipline-by-discipline in order to show explicitly the connections between laboratory and industry and assess the performance of the industrial follow-up; thirdly, estimate the contribution made by fundamental science to the education of people 'needed by technology and the more applied sciences'. More recently, in a profile of the new Science Adviser (John Gibbons) to the US President (*Sci. Am.*, **268**, April 1993), John Horgan quotes Gibbons: 'some scientific fields, notably particle physics, have grown much faster than the overall economy ... That is known as a divergent series ... seems to me indefensible that science should have a rate of growth of support that is multiples of the growth of our resources'.

The directional change in funding thus started almost synchronously with the publication of the book. In recent years, cooperative research and development by otherwise competing firms in pursuit of a shared vision is being attempted. Federal funding is no longer married to their 'basic research is the best' philosophy. Industry–academia relationships in general have been expanded. The 'supercollider' project has been virtually given up, projects such as the trip to Mars are on the back-burner and the move is on to promote internationally shared costs in basic research, much on the lines of the European CERN.

## The Indian context

The relevance of the theme to the current situation in India is probably self-evident, regardless of whether one agrees with the metaphor of separate trees (p. 20) of basic science and applied science and engineering or not. Although the national environment in the US in 1985 was different from the situation in India today, the cause–effect linkage is relevant. The theme is pertinent to the current debate (in the hospitable columns of *Current Science*) on S&T policy-making, standards in scientific education, and the state of scientific research in India. The lack of synergy between scientific research and industry and the nonparticipation of science institutes in graduate education are also the results of a somewhat similar mindset. In fact, the invited responses reproduced as Appendices are themselves rich in relevance – some vintage 'truths': 'organize science and engineering as a systems continuum, joining basic research to applications and engineering' (William Baker, ex-Chairman, Bell Laboratories), creativity is 'competence to transfer the common denominator in all honestly pursued research from one field to an entirely different one' and age is no barrier to creativity if people make 'the centre of their life the intellectual life of the laboratory' (and not seeking dignity through pursuit of power over people) (Edwin Land, ex-Chairman, Polaroid)

One element not referred to in the Shapley–Roy essay is the motivational aspect of national or industry policies in S&T. In the context of their essay, the issue of how a large growing population of science-educated students (as in India) could be motivated first to seek an S&T career and thereafter to strive for excellence in their chosen area was not of immediate relevance to them.

Overall, an excellent piece of constructive criticism. Recommended reading for policy-makers and serious thinkers on the future of S&T in India.

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**Stereochemistry of Organic Compounds – Principles and Applications** (second edition). D Nasipuri. Wiley Eastern Limited, New Delhi. Price. Rs 200/-.

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Barring a few who are working in research areas steeped in stereochemistry, a majority of teachers and students treat this subject as a maze and a necessary evil. On second thoughts, this should sound unnatural, for we live in a three-dimensional world and feel the stereo-differentiation in our day-to-day lives. The problem is probably in the way we start teaching chemistry. At the formative levels, we introduce the subjects in a two-dimensional format and at a later stage we develop the third dimension. The problem has been further complicated by the lack of an authoritative text book for teachers for introducing the subject in the correct perspective. The book by E. Eliel served this purpose admirably from the college level onwards up to the level of advanced students in stereochemistry. This vast range had made the volume a treatise for intense reading. On the other hand, a general text book on organic chemistry finds limited space to do proper justification at an introductory level. An authoritative book bridging the gap has been long overdue. This volume by