If it ain't fixed, don't break it....

Cecil H. Fox

Value-judgements about the need for more 'goal-directed' or 'basic' scientific research beg the question of how the publicly funded scientific enterprise works. An efficient management would start to put things right.

PEOPLE in countries where science is supported from taxes generally approve of using public money for scientific work. Unfortunately, there is rarely a fixed plan for obtaining these funds, nor does any western government have a hard and fast rule of how much money should be available. This means that support of science is dependent on the political and economic climate, creating repeated cycles aimed at making science more responsive to needs (applied science) or to wishes (basic science). Taxpayers (and most scientists) seem content to let politicians regulate the flow of money for science, but politicians are ill-prepared to deal with the everyday delivery of science, its integrity or the structural organization of the process. Mercifully, they seldom ask for an accounting in cash for scientific results.

In the United Kingdom there are elements of the government that believe science has a basic obligation to demonstrate its utility and cost-effectiveness — put simply, that all science should be applied science. In the United States, still recovering from the dark ages of the Reagan–Bush years, public funding for science is being examined from the opposing point of view. At the National Institutes of Health there is a pall of depression arising from uncertainties in the intramural research programme (see ref. 1) and a new emphasis on ill-defined 'basic' research. In Congress there is concern as to why universities are being subsidized through 'indirect costs' with money intended for scientific research. Because these funds may equal the amounts awarded to scientists for their work, radicals have even questioned why publicly supported science should be done in academic institutions or why scientists employed by private universities should require supplementary income from federal research grants.

Other factors essential to the support of science with public money are management and leadership. A frequently cited failure of applied science is the "war on cancer" in the 1970s, a concept of Richard Nixon (probably initiated by Benno Schmidt Sr). The idea was that if a problem such as cancer is carefully analysed and compartmentalized into attainable management-school-like research objectives, a solution could be expeditiously reached. A 'pie' chart showing the different programme objectives was drawn and waggishly labelled "the wheel of fortune". But these goals were often rapidly subverted for the convenience of scientists who intended to continue doing the research that they had always done. Worse still, managers of the programme were amateurs in the form of researchers converted to bureaucrats. Nobody got fired (indeed many are still at the same jobs), transferred or reprimanded for not accomplishing the goals set for them. These 'leaders' of the programme never appeared in the labs to assess progress. The only measure of success was in the numbers of papers produced; results were not effectively analysed and coordinated. I think the final death of the wheel of fortune occurred with the appearance of the AIDS epidemic and the rush to show that HIV was a tumour virus.

It seems reasonable to expect that scientists who become managers should have studied management in a Master of Business Administration programme before being allowed to control the expenditure of public money. The scientific skills of scientist-managers would then expedite, not impede, the work of bench scientists. In my opinion, the best-managed programme in science today, both in discovering promising young scientists and in nurturing mature ones, is the Howard Hughes Institute, a fraction of the size of the NIH. It boasts managers with experience, compassion and intelligence, and is, fortunately, immune to government politics and politicians.

Fields[2] has used the polio paradigm to argue that because research unrelated to polio resulted in conceptual advances leading to an effective vaccine, the future course of AIDS research should be guided by the tactic of supplying more money for basic and perhaps unrelated research. If one examines the history of polio, however, one sees that 'basic' researchers spent years passing the same strain of virus from one animal brain to another without considering the obvious enteric pathobiology of the disease. The stopgap killed vaccine (still being used) derived from this work protects only a single individual at a time and resulted in the injection of millions of children with SV40 (refs 3,4). Recognition of polio as an enteric infection, the oral vaccine (not without risk) and the resulting acquisition of herd immunity occurred only when the 'basic' discovery of cell-culture techniques was combined with 'applied' research on pathobiology of the disease. In contrast to Fields' argument, I suggest that all science needs support for progress in any area.

HIV research is still at a stage similar to the brain transfer work in polio. Countless cultures of the same HIV strain (LAI) of virus have been used by putative basic researchers, yielding few results with any relevance to patients. Instead, an AIDS industry sprang up early. Much of the money intended for HIV research was spent on administrative and indirect costs. A lethargy has enveloped basic scientists in a hideous tautology of despair. As with polio, the recognition of HIV disease as a disease of lymphoid tissues in humans, and as such a typical lentivirus[5,6], has required years to achieve.

Before basic research becomes a tenet of politically correct ideological rectitude it would be wise to remember that science involves a complex equation, with modern science a détente between Aristotelian descriptive science and Baconian experimental science, where nature is 'tortured' by experiment to reveal her truth. The method of practising science is selected by individual scientists themselves according to their proclivities. Edmund Burke wrote that "men must have a certain fund of moderation to qualify them for freedom else it becomes noxious to themselves and a perfect nuisance to every body else"[7]. If science is to enjoy the support of society, a rationale may be a practical necessity. But basic research must happen under the dictates of circumstance or conscience and not as an exercise in subsidized self-indulgence. Moderation, an obligation of intellectual freedom, requires that science retains the tacit understanding that taxpaying society has agreed to support not only scientists, but creativity, for increasing knowledge and to deal with specific problems. Science is not fixed, rather it is a vital entity, basic or applied, and its lugubrious progress is more easily crippled by attempts to regularize it than not. And finally, the practice of scientific thinking with public support is a privilege, not a right. □

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