

Glittering prizes for research support

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Could public support for research be cheapened, and made more productive, by following the eighteenth-century precedent of the British government's prize for a means of measuring longitude?

THE crisis in British universities, the shortages of research funds and the insistent demands that science should serve the economic needs of the nation which supports it, ought to require a fundamental rethinking of our approach to the financing of science. I see much evidence of panicky tinkering by both the scientific community and the government, but few indications of radical thought.

We should perhaps consider what may be the first example of parliamentary funding of research, which is also one of the most successful. Because it is such an excellent precedent, those in power might take it seriously. The hero is Mr John Harrison, born in 1693 the son of a poor Yorkshire carpenter, a self-educated clockmaker and the toast of Neil Armstrong at a twentieth century 10 Downing Street dinner.

Petition

In a petition to the British Parliament on 25 March 1714, Captains of Her Majesty's Ships, Merchants of London and Commanders of Merchant Men demanded a solution to the problem of measuring longitude at sea. Failure was causing innumerable navigational disasters with the loss of ships, men, goods and battles. The government, wisely, chose not to throw money at the problem by supporting research by experts who, by definition, had failed to solve it. Instead, they arrived at an astonishing proposal: a prize of £10,000 was offered for a method that would reliably measure longitude to within 1°; £15,000 for a method accurate to within 40'; and £20,000 for a method accurate to within 0.5°. In 1714 those sums were truly astronomical. Anyone winning the prize would become not merely comfortable but genuinely rich.

The result was an outburst of private research on longitude. The problem and the prize were known to everyone. They are mentioned in Swift's *Gulliver's Travels* and a Hogarth cartoon shows a lunatic obsessed with them. Theoretically, it was known that one way to solve the problem was to make a timepiece so accurate that it would tell the time at Greenwich even after many weeks at sea, much buffeting and drastic changes in temperature. Comparison of that time with local time would then allow an accurate calculation of longitude.

None of the experts believed that a timepiece could be so accurate. An array

of bizarre and complex proposals was put forward by the Fellows of the Royal Society and their friends, but all failed. Only poor, provincial, self-taught John Harrison believed that a clock could be made to the required standards. His story is well known: his production of increasingly reliable, robust and accurate chronometers, their objective success in Royal Navy trials, his denigration by the eminent gentlemen of the Royal Society, the attempts by the Society's experts to deny him the prize legitimately won, the anger of the King when he heard about these academic machinations and Harrison's final triumph with grant of the full £20,000 at the age of 80. But the lessons to be drawn from this remarkable story are as valid now as they were in the eighteenth century.

- The announcement of a cash award sufficiently large to make the winner enormously wealthy will generate a phenomenal outpouring of research directed to the solution of practical problems. Such research will be privately financed and will cost the donor nothing but the cost of the award.

- Such an award will lead to unprecedented cross-fertilization of fields of endeavour. If the prize is large enough, scientists whose speciality may appear remote from the problem will be stimulated to think about it.

- The definition of success must be crystal clear, practical and open to verification by non-experts. The solution need not be brilliant or sophisticated, nor need it meet with the intellectual approval of experts. The only condition is that it must work.

- There must be no limitation on the categories of people allowed to succeed. The challenge must be open to non-experts and to the sons of poor carpenters.

Prizes

The government should decide what problems it wants solving. In my field of medicine, obvious problems whose solutions would save a great deal of money include schizophrenia, eczema, multiple sclerosis and Alzheimer's disease. People from other fields should be able to produce long and comparable lists. Economists could then work out what each particular problem costs the nation, and a prize could be offered for a practical approach which would either eliminate the problem or reduce the cost of solution. The prize should

be the value of the savings made during one full year.

Some of the prizes might turn out to be very large indeed, tens or even hundreds of millions of pounds. Even greater amounts would be possible if, say, the European Community, the United States and other countries acted together, as they might well do if the scheme cost little or nothing. The prize should be given tax free to the individual, company or group producing an answer.

The advantages of the scheme would be many, but include:

- The cost of initiating the scheme would be trivial. After the first year, a successful solution would save millions.

- Researchers of all kinds would be encouraged but not forced to work on practical problems. They would also be encouraged to extend their range of expertise.

- There would be large inflows of private venture capital into research as individuals, companies and others chose to support research directed towards the solution of problems attracting the larger prizes.

- There would probably be rapid progress in basic science. Many now suffer from the illusion that basic discoveries always precede practical research. Historical evidence suggests that the traffic is just as frequently in the other direction. We have forgotten the story of Pasteur. Brilliant people devoting themselves to practical problems, such as the spoiling of wine or dying silkworms, may end up by discovering new fundamental principles.

- The enterprise would be cash-generating rather than cash-consuming. Governments would be delighted with science instead of looking at it with a jaundiced eye.

- Last, but by no means least, the huge cash prizes would put money into the hands of the most practically creative members of society. The money would certainly be spent in interesting ways; on further research, on enterprising investment or even on innovative techniques of personal consumption.

The idea would cost almost nothing to put into operation. It would rejuvenate science and transform attitudes to science. It should be given a try. □

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