FOR THE PUBLIC GOOD, SET SCIENCE FREE

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A Manifesto Club
THINKPIECE

In short …

Scientists are not ‘stamp collectors’, arranging dry facts about nature. The development of scientific understanding requires creative thinking and imaginative leaps, just as much as in the arts.

Science has become increasingly an instrument of government policy, with cuts, redundancies and financial controls. These were ostensibly to improve efficiency and results but the short-term aspirations and narrow goals of this policy tend to limit scientific research. It becomes not the ‘search after truth’ but the ‘search after funding’.

Key developments in agricultural science came not from government policy, but from scientists who were playing with ideas, pursuing pure academic research, or having informal conversations with their peers. Free scientific investigation yielded major industrial and agricultural benefits.

Scientific research should be funded as a ‘public good’, an end in itself, held at arms length from political institutions. It is when scientists are free to follow their noses that they make the breakthroughs that advance both human understanding and industrial practice.

Scientific and artistic creativity

When I was in the sixth-form 50 years ago, some of the pupils studying arts subjects affected a disdain for us scientists. I remember an article in a school magazine in which we were disparaged as ‘stamp-collectors’, somehow ‘less intellectual’ than they were. I was annoyed by this comment at the time, but I didn’t discover the counter-argument I should have used until about 10 years later, when a friend introduced me to Sir Peter Medawar’s collection of essays The Art of the Soluble.[1]

In his essay ‘Hypothesis and imagination’, Medawar emphasised the importance in science of the imaginative leap that reveals what might be. ‘Scientists are building explanatory structures’, he wrote, ‘*telling stories*, which are scrupulously tested to see if they are stories about real life.’ The stories and the testing were the essential elements in the process of ‘having an idea and testing it’, more formally known as the hypothetico-deductive process proposed by the philosopher Karl Popper.[2] Medawar’s essay was first published more than 40 years ago; while some scientists, perhaps those more attracted by the ideas of Thomas Kuhn,[3] would now question the role played by falsification in Popper’s ideas, I suspect few would reject the role of the imaginative leap in scientific discovery. Indeed, it is probably inherent in Kuhn’s ideas about the way the emergence of anomalies in the current scientific paradigm leads to what he terms a ‘paradigm shift’.

At the end of his essay, Medawar drew attention to one question left unanswered about the hypothetico-deductive process. How does the hypothesis or idea come into being? This question brings back a still-vivid memory from more than 30 years ago. A computer model can be a form of hypothesis – you write it and then test it to see whether it conforms to reality – and I can still remember writing my first computer model, and then wondering, ‘Where on earth did that come from?’. And I’m still wondering.

Medawar offered no definite response to his own question, so we are left to seek answers elsewhere. One comes from a former Bishop of Durham, Ian Ramsey, a mathematician with an interest in medicine as well as theology. One of the concepts he developed was that of the *disclosure*, which he saw as a situation where ‘the light dawns’ or ‘the penny drops’.[4] ‘You begin with empirical, verifiable, flatly descriptive facts; these facts however are such as to invoke an insight, a disclosure of meaning or of the existence or the givenness of something not appreciated previously.’ It seems axiomatic that a disclosure will come most readily to a prepared mind. The geneticist Sir Alec Jeffreys appeared on Radio Four’s *Desert Island Discs* recently, and I was amused to hear him use the expression ‘the penny dropped’ to describe the realisation that led to genetic fingerprinting. The bishop would have been delighted.

Ramsey said that disclosures could occur in either a theological or a scientific context. We could add that disclosures must surely occur in other contexts, in poetry, music or the visual arts, for example. There is as much of a creative element in good science as there is in many more obviously creative activities in the arts world. But, as Medawar pointed out, the creative elements in the two contexts are not identical. ‘No-one questions the inspirational character of musical or poetic invention’, he wrote, ‘because the delight and exultation that go with it somehow communicate themselves to others. Something travels: we're carried away. But science is not an art form in this sense; scientific discovery is a private event, and the delight that accompanies it, or the despair of finding it illusory, does not travel.'

Medawar and Ramsey therefore suggested that science and the arts can be equally creative activities but are likely to be experienced differently. This, of course, is the answer to those budding intellectuals who perceived scientists as stamp collectors. Today, unfortunately, the argument is no longer about whether the scientist or the artist is the more 'intellectual'; it is about whether the intellectual approach has any validity at all.[5]

The creative element must surely be one of the key reasons why science and the arts were both viewed as a 'public good' when I began my research career more than 40 years ago. Both science and the arts were funded as an end in themselves, and were guided by their own discipline, not the demands of politics or politicians. Both have fallen from this happy position. Over the past decade or two, science has gradually become subject to more and more managerial goals. While these were brought through in the name of efficiency, they actually hamper the research process. The case for science as a public good needs to be made anew.

Scientific research: An end in itself or an instrument of government policy?

I joined Rothamsted Experimental station (now Rothamsted Research) in 1966, in the days of the Haldane, or 'arms-length', principle. Under this principle, the research councils were funded almost entirely from the science vote controlled by the then Department of Education and Science, and were autonomous in respect of their programmes. They decided what work to do without any formal direction from government. And the arms-length principle meant that there was little interaction with the departments with which they were most obviously associated, for example, the Agricultural Research Council with the Ministry of Agriculture, Fisheries and Food. Research was seen as an end in itself, a public good, and the idea of research as an instrument of government policy was still waiting to emerge.

Rothamsted fell under the Agricultural Research Council, which gave it an annual grant, administered by the director, to cover salaries and research expenses. The duty of the staff was to do research and, although the area of research was usually suggested by the head of department, they enjoyed considerable freedom to 'follow their noses' in what they actually studied. This was, looking back, some kind of 'Golden Age' for scientific research and, to be honest, we probably had it too easy. But change was soon on the way.

Harold Wilson, who was Labour prime minister when I came to Rothamsted, had recently delivered his speech about the 'white heat of the technological revolution', which sounded supportive of science, but he was defeated in the 1970 election by the Conservative Edward Heath. Two years later came the Rothschild Report, one of the greatest upheavals British science has experienced. This was felt particularly at Rothamsted because our popular and highly respected director, Sir Frederick Bawden, strongly opposed the report, fought it tooth and nail and died of a heart attack while doing so. His funeral was on a foul wet afternoon in February and my gloom was intensified by seeing ahead of me some smoothly dressed character in an overcoat with an astrakhan collar, pinching my umbrella as we went out into the rain.

What was it in the report that so incensed Sir Fred? The essence of the Rothschild Report was that applied research and development should be subject to the 'customer/contractor principle'. This meant that the customer (in this case the government) said what he wanted, the contractor did it, if he could, and the customer paid him. The principle doesn't sound too unreasonable on the surface, particularly for applied research, but it was the beginning of the idea of science as an instrument of government policy rather than an end in itself. This might have been what so troubled

Sir Fred, or maybe he saw further. The Royal Society's Biographical Memoir of Bawden, written by his close associate NW 'Bill' Pirie, noted that: 'He did not object to the changes on principle but on the eminently practical grounds that the proposed changes in the organisation of research would only make matters worse. The only people who know enough about the potentialities to plan research intelligently are those who are actually doing the work.'[6]

This was one obvious potential problem with the customer/contractor principle: 'Did the customer actually *know* what he wanted?'. This problem was addressed by the introduction of JCOs, Joint Consultative Organisations, comprising representatives of both the customer and the contractor, who between them sorted out what research needed to be done. The senior scientists who sat on the JCOs thus retained a fair measure of say in what research was done. And less senior scientists still retained a reasonable amount of freedom and still had scope for scientific creativity. This was perhaps the first peacetime intrusion of bureaucracy into science, but it was a very mild bureaucracy compared with what was to come.

In 1979, a faltering Labour government was again replaced by the Conservatives, this time led by a certain Margaret Thatcher, and nothing in British science was ever the same again. I remember coming back in the autumn of 1983 from a sabbatical at Cornell University USA to find that a friend, a reasonably well-known scientist, had been made redundant. He was the first of many. To be fair, one or two people who were cut were not pulling their weight, but most of them definitely were. The impact of cuts and redundancies on science led to the foundation of the 'Save British Science' (SBS) organisation, which I joined. I doubt whether any other country has had an organisation devoted to protecting its science against its government.

Scientific research and financial control

Cuts and redundancies were not the only problem under the Thatcher government. Mrs T was also the 'high priestess of bureaucracy'. She had by then embraced the cult of monetarism, and financial control was the order of the day. Whether it was cost-effective financial control was questionable, and 'Who's auditing the auditors?' was always a relevant question. At one time the word came down that we were to do 'near-market' research, research that was close to the requirements of industry, farming in our case. This was followed rapidly by another word from on high saying that we should not be doing near-market research, as it should be done by the industry.

There was little doubt overall that by this time most of our research was regarded not as an end in itself, but as an instrument of government policy. Scientific creativity was not stifled completely, but those who were in a position to exercise it were in a privileged position. One curious aspect of the bureaucracy was that it eventually numbered among its employees some of the scientists who had been made redundant from research posts. I remember wondering how they felt towards those of us still working in research.

Financial control usually implies giving out money in small closely controlled amounts. This was expressed in the short-term contract for research, usually three years, often employing a post-doctoral scientist, and summarised by one wag as 'one year learning the job, one year doing it and one year looking for the next job'. It gave no job security to the scientist in the post, and it was a problem for the person managing the project because the in-post scientist tended to 'jump' if he or she saw a more secure post elsewhere. This was not the only inefficiency in the system. Senior staff had to spend time applying for these projects, time that was wasted if the application was not successful. Scientific research had arguably changed from being the 'search after truth' to being the 'search after funding'.

This system also had little flexibility, virtually no allowance for experiments that go wrong or equipment that breaks down. To quote again from that Desert Island, this time when the castaway was the writer Tariq Ali, 'Creative freedom involves the right to fail'.

Or as my father used to say, ‘A man who never made a mistake never made anything’. But despite what happens in the real world, the system demanded that timetables for achieving objectives were laid down in advance and followed. You virtually needed to know what you were going to find before you started. But science doesn’t work like that! To quote again from Sir Alec Jeffreys, ‘Important discoveries are always unexpected’. In a lesser vein, my PhD turned on a result in the first experiment that was the opposite of what was expected.

The New Labour government 11 years ago brought some improvement in the position of science, but it did not greatly change the approach to funding scientific research. The weakness of the system in nourishing scientific creativity was recently illustrated in a group at Rothamsted with which I am associated in a retired capacity. A very bright young researcher who was recently the lead author of a paper proposing an important new idea about a key soil process has just left the group to take up a non-research post in a national library. She would have preferred to stay with the group, but she has a husband and family to support, and the library offered her an appreciably larger salary and, more importantly, job security. Scientific research simply cannot afford to lose creative people like her.

How do the ideas behind important discoveries emerge?

Sir Alec Jeffreys’ comment above that important discoveries are always unexpected implies that no generalisations can be made about how the ideas behind them originate, except perhaps that they come to the prepared mind. Probably the best I can do is to provide examples from Rothamsted’s research. Two of the examples – the development of the synthetic pyrethroid pesticides, and the elimination of the paracrinkle virus from King Edward potatoes – exemplify research that had significant financial benefits for the UK, while the other – the discovery of the effect of the metals in sewage sludge on soil microbes – was of great practical and environmental importance.

The development of the synthetic pyrethroid insecticides.

Pyrethrum extract is a natural insecticide with low toxicity for mammals, rapid knockdown of flying insects and virtually no problems of persistence in the environment. The team led by Michael Elliott that worked on the synthetic pyrethroids aimed to produce compounds that had greater insecticidal activity while retaining the other desirable properties.[7] They were enormously successful in this aim, the resulting compounds proving highly effective against pests causing both agricultural and health problems. They were also economically beneficial to the UK. By the middle of the 1980s, synthetic pyrethroids made up more than 20 per cent of the global insecticide market, with the compounds developed by Elliott’s team constituting two-thirds of the total and making a major contribution to the UK economy through patent rights.

Andrew Farnham, who wrote Elliott’s obituary in the *Guardian*, records that Elliott was convinced that the pyrethroids acted by a ‘lock and key’ mechanism and spent many hours manipulating molecular models to determine which chemicals should be synthesised to probe the shape of the lock. Even some of Elliott’s colleagues felt that he was merely ‘playing’, so how would today’s bureaucrats have reacted? But by 1967 the team had synthesised pyrethroids such as resmethrin and bioresmethrin that were more active than the natural products while retaining their desirable characteristics, and even better compounds were produced in 1974. Creative play is regarded as important in children’s development, and it seems to have a useful role in science too (which may be no coincidence). But it would never be allowed under a system of political or bureaucratic management.

The elimination of the paracrinkle virus from King Edward potatoes.

Paracrinkle was the name given in 1930 to the severe disease that developed when Arran Victory potato plants were grafted with scions from the apparently normal King

Edward variety. The disease was attributed to a virus carried without symptoms by the King Edward plants. Basil Kassanis, a member of the Plant Pathology Department at Rothamsted, was interested in this virus, and eventually managed in about 1957 to produce virus-free King Edward plants by meristem-tip culture (the meristem is the growing point at the top of the plant). This enabled virus-free clones of these plants to be propagated extensively and used to replace existing stocks of this cultivar, with an important but totally unexpected result. Although the virus had not caused any apparent symptoms in the King Edward plants, it was now clear that it had been decreasing yields by 10 percent. King Edward was a very popular variety and eliminating this loss had brought financial benefits of several million pounds per year then, and a lot more at today's values.

As the authors of the Royal Society's Biographical Memoir of Kassanis[8] noted, work begun for purely academic reasons had proved to be of considerable value to British farmers. No-one could have foreseen these benefits when Kassanis started his work. The authors of the memoir also imply an element of serendipity in their comment that, '… Kassanis, now *by good fortune* working on carnation viruses, discovered a virus that was latent in this species, … and had particles resembling those of paracrinkle virus'. Serendipity has a long and honourable history in research, but there is little room for it when scientists are mere contractors for government.

The effect of 'heavy metals' on microbes in the soil.
Rothamsted has since 1942 run the Woburn Market Garden experiment, which assesses sewage sludge as a soil conditioner and supplier of plant nutrients and compares it with farmyard manure and other materials. This sludge contains appreciable concentrations of the so-called 'heavy metals', copper, nickel and zinc. ('Heavy' is a misnomer because, in terms of their atomic weights, they are not particularly heavy.) My former boss Edward Johnston had this experiment among his responsibilities for many years, and from his observations had formed the hypothesis that the soil in plots with sewage sludge contained more microbial biomass than soil in plots without it. (Microbial biomass is a measure of the sum of the living microbes in the soil.) He put this idea to Phil Brookes, an expert on microbial biomass, during a chat in the corridor, and Phil did a few preliminary experiments that suggested that the sludge plots in fact contained *less* microbial biomass than the others.

Johnston's hypothesis was wrong but the question he asked turned out to be immensely important. Further experiments on microbial biomass by Brookes and Steve McGrath showed unequivocally that the plots with sewage sludge contained less microbial biomass than those without it, and also less adenosine tri-phosphate (ATP), an independent measure of microbial biomass.[9] The toxic effect of the metals seemed to make the microbial biomass in the sludge-affected soils respire at a much higher rate than that in the soils given farmyard manure. These are serious problems, and this was an important discovery. An immense amount of sewage sludge is produced per year – we all contribute – and land disposal is the commonest way of getting rid of it.

Casual conversations between scientists over coffee or in corridors have perhaps had more impact on scientific progress than all the bureaucrats of science put together. Perhaps only the input from 'unexpected results' has been more influential. But neither type of occurrence is allowed for when science becomes an instrument of government policy.

Science must be seen as a public good

Has science as an instrument of government policy been more effective than science funded as a public good? Considering three examples of important developments from Rothamsted research, we see that one arose from Elliott's conviction about the mode of action of the pyrethrins and his almost obsessive 'playing' with molecular models; another sprang from Kassanis' purely academic study on King Edward potatoes; while the third resulted from Johnston's faulty but vital hypothesis about the effect of metals on biomass and his chat in the corridor with Brookes. Government policy played no part in any of them. It is questionable whether playing with molecular models or curiosity-led academic study would be approved by the present scientific bureaucracy.

Of course, we cannot argue on the basis of just these three examples that science is more productive when treated as a public good than as an instrument of government policy. But what does seem clear is that the public good approach provides a particularly good breeding ground for scientific creativity. Both Elliott and Kassanis achieved what they did because the system allowed them to pursue their academic interests or, in Elliott's case, to play. In both cases, the 'system' was amply rewarded financially. It is no coincidence that some commercial companies find that it pays to give their research scientists plenty of scope to follow their noses and to learn from their mistakes.

Both present and previous governments have been very keen on financial controls on science, but perhaps it is now time to ask what they really achieve. We might find that some of the financial controls on science are not as cost-effective as we are led to believe, and that there is a good case for restoring the arms-length principle.

There is also a philosophical case to be made. The Manifesto Club is committed to the values of the Enlightenment, and we recall that the first enlightenment led to a flowering of science and the arts. The blossoming of science led to many practical and economic benefits, in health and food supply for example. As we work towards a new enlightenment, we must once again treat science as an end in itself.

Our scientists need to be managed in a way that allows them to follow their noses, make mistakes, and be creative. And we need to make research exciting again, the sort of excitement you sense in *The Double Helix*, James D Watson's account of the discovery of the structure of DNA. It is when scientific research is freed from the demands of political bureaucracy that it can yield benefits to society – and be a joy for the scientists pursuing it.

Endnotes

1 Medawar, P.B. *The Art of the Soluble*. London: Methuen,1967

2 Popper, K.R. *The Logic of Scientific Discovery*. London: Routledge,1959

3 Kuhn, T.S. *The Structure of Scientific Revolutions*. Chicago: The University of Chicago Press, Third Edition, 1996

4 Ramsey, I.T. *Religion and Science: Conflict and Synthesis*. London, SPCK, 1964

5 Furedi, F. *Where Have all the Intellectuals Gone?* London, Continuum. 2004

6 *Biographical Memoirs of the Fellows of the Royal Society*, 1972 Volume 19, p.33

7 Farnham, A. Michael Elliott. *Chemist whose research led to modern insecticides*. Guardian, 5 December 2007: www.guardian.co.uk/print/0,,331455974-103684,00.html

8 *Biographical Memoirs of the Fellows of the Royal Society*, 1988, Volume 34, pp.347-371

9 Brookes, P.C. and McGrath S.P. Effects of metal toxicity on the size of the soil microbial biomass. *Journal of Soil Science* 35: 341-346, 1984.

About the Author

Tom Addiscott graduated in chemistry from Oxford in 1963 and spent the following year in the university's Soil Science Laboratory, which stimulated a lifetime interest in soil science. After a year as a volunteer studying soil phosphate in Tanzania and another in the fertilizer industry, he joined the agricultural research institute Rothamsted Experimental Station in 1966, and worked there until he retired in 2002. He remains in contact with the institute as a Lawes Trust Senior Fellow and through the pavilion bar, the setting for several discussions pertinent to this Thinkpiece. (But the views expressed here are his own and not necessarily those of Rothamsted.) He received the Gold Medal of the Royal Agricultural Society of England in 1991, was made a Visiting Professor at the University of East London in 1997, and was awarded a DSc by Oxford in 1999. Much of his career was spent developing computer models for the processes controlling losses of nitrate from the soil. He also became involved in the philosophical issues surrounding modelling and the problems of non-linearity in models.

He has written two books on nitrate issues and has recently taken a particular interest in the role of nitrate in human health, leading him to write articles and give talks with the aim of dispelling 'the myth of toxic nitrate'. He also targets the 'toxic mythology' that aims to ban DDT and thereby free malarial mosquitoes to kill Africans.

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