# SCIENCE AND THE SUBURBAN SPIRIT

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Although Walter Murdoch's essays ranged widely, he rarely mentioned science. The word does not, for example, appear in the index of his Collected Essays. Yet his spirited campaign against the suburban spirit, "the everlasting enemy" as he called it, is as pertinent for science today as it was for other influences on our lives in Murdoch's time, and I am mindful of his injunction not to acquiesce, but to growl!

Walter Murdoch was, however, too wily to be trapped into defining just what he meant by "the suburban spirit." One of the older (1668) usages of suburban in the Oxford Dictionary refers to "inferior, debased and especially licentious habits of life", but that isn't what worried Murdoch. His everlasting enemy was, rather, all "crabb'd and confined" imagination, mediocrity of spirit, submission to authority and lack of adventurousness. It was adventures of the mind, not of the body, that concerned him, for as he said "I never yet harpooned a whale", although he bagged many other targets. Mine this evening is our management of science.

My particular concern is that as science grows and spreads its influence ever more widely in our lives, it too is being suburbanized to the point where it deters the original, protects the mediocre, tempts its practitioners to find false contexts for their research, and leads its managers to believe they have solved a problem when they have identified it. The front garden of science is being made increasingly orderly and productive, filled with Francis Bacon's "experiments of fruit". But what of his "experiments of light", do they still thrive in a chaotic back garden, out of sight?

## The suburbanization of science

When the Royal Society was established in the seventeenth century, its founders were very conscious of Francis Bacon's grand design for the natural sciences, and pursued both theoretical and practical advances. Some early apologists, like Thomas Sprat, emphasized the uses of science, but it was the combination of intellectual adventure and usefulness that particularly appealed to many. During the 19th century, however, following the industrial revolution, there emerged increasingly a class distinction in British science between the aristocratically pure and the vulgarly applied.

Indeed, as Medawar (1967) points out, the notion of purity was somehow superimposed on basic science, implying a self-righteous disengagement from the pressures of necessity and use. Science was agreed to be both good and useful, but the good science was not considered useful, while the useful was not thought to be good.

Over a longer time scale, however, pure science was also found to be useful. But in its growing requirements for equipment it became more expensive, more reliant on government funding, and began to lose its autonomy along with its purity. As Peter Kapitsa put it: "in the year that Rutherford died there disappeared forever the happy days of free scientific work . . . Science has lost her freedom. Science has become a productive force. She has become rich but she has become enslaved, part of her veiled in secrecy."

World War II enhanced the enthusiasm for the usefulness of good science: if it could win the war, might it not win the peace? J. D. Bernal thought so, in his book "The Social Function of Science", and argued that science should be planned to serve social needs. While Polanyi and others dissented from this view, the growing dependence of science on government funding was almost inevitably coupled with attempts to manage it more closely. New genera of science evolved to fill the new niches in administrative thinking and financial availability. Besides basic (or pure or fundamental) and applied science, there appeared oriented fundamental, mission and non-mission, strategic and tactical, curiosity-oriented and product-oriented, indeed a whole spectrum of sciences. At least this made the point that science was a continuum, but it also made it easier to justify the shifting of resources away from the basic end of the spectrum. When it was 'pure' it was, like Caesar's wife, beyond reproach, as well as having an aristocratic éclat. Now it became, in the words of Werner von Braun: "What I'm doing when I don't know what I'm doing". So there grew the temptation to suburbanize it, to replace the tyranny of the pure by the tyranny of the relevant, to tell science what it should do.

## The view from the government bench

Quite apart from its many uses, science has contributed profoundly to our culture and to our understanding of ourselves and our environment. It reflects our restless determination to penetrate the laws of nature, and in doing so provides a progressive element in society through its ceaseless questioning of dogmas, traditions and assumptions. Beyond all that, the great conceptual advances of science excite, stretch and inspire us like all the finest cultural achievements of mankind.

Most governments and peoples accept, therefore, the desirability of supporting their most outstanding scientists for the cultural value of their research, for their contribution to national identity, and for their enlivening effect on the educational system.

For the rest of the scientific enterprise, however, governments have a quite understandable wish to domesticate it, on the grounds that science is now too important to be left to scientists. After all, were not many masterpieces of music, painting and sculpture commissioned, so why not those of science? However, although the artist can't be sure that he will produce a masterpiece, he can be reasonably certain of producing a finished work on time, whereas the scientist knows neither when nor whence his solution will come. Adaptive research within existing paradigms can, of course, be commissioned, but not the major and unpredictable advances.

Until recently, the scale of scientific research has tended to grow exponentially in many countries, including Australia, a phenomenon which has been referred to as 'the sigmoid fraud'. But as the demand for funds has outrun their availability, choices have had to be made and governments have had to develop policies to guide those choices. One seductive guideline is the belief that research can and should be guided into more useful channels rather than leaving it to the processes of peer evaluation which scientists favour. Lord Rothschild put the matter with characteristic bluntness in his Green Paper: "the country's needs are not so trivial as to be left to the mercies of a form of scientific roulette". Once governments believe that science can be so shaped, they naturally want to use science to realize national policies. But attempts to use science for policy in this way are doubly dangerous. Firstly, governments may come to believe that once they have specified a problem, science can find a solution for it. Secondly, the failure of science to do so may all too easily be interpreted as due to obstructionism or lack of social conscience among the scientists.

Yet another siren song, particularly for governments of smaller or developing countries, is the argument that it is the scientifically strongest countries which gain most from any advances in basic research, so that it is better for the smaller countries to concentrate their research in specified areas of more immediate need. The temptation to do this is almost irresistible in developing countries, yet by doing so they reduce their capacity to be aware of and to exploit advances in world research. The urge to specify favoured areas of research can be quite strong even in countries like Australia. The advertisement for research centres of excellence mentions the possibility of their being "directed to a national research objective" — a rather broad hint — and ASTEC has recently published a report entitled "Basic research and national objectives". So we need to examine how scientists react to this domestication process, and whether it is likely to be effective.

### The view from the laboratory bench

When scientists resist the pressures of suburbanization, as they often do, it is all too easy for both government and the public to dismiss this as revealing them in their true colours as irresponsible individualists concerned only with their own scientific careers. That may be true of some, but more often it reflects a longer term, more international view, and a greater awareness of how science works and of the driving forces in scientific discovery. Most scientists have wit enough to recognize that, in the long term, support for their research and for their institutes will depend on the public backing that comes from demonstrated value.

However, they tend to resist attempts to focus their research on specified national objectives, except under the exigencies of war, for two reasons that deserve better than simply to be labelled as self-seeking. The first is their credo that science can't be programmed because one really can't predict when or whence a solution will come. In most cases it is unlikely to come from the direct approach to a specified problem, and to press for that is tantamount to putting blinkers on the scientific workhorse. It is the lateral thinking, the bringing together of disparate elements, the serendipitous recognition of unexpected opportunities that has made science so useful. T. S. Eliot's comment in 'The use of poetry and the use of criticism' that "What happens is something negative: that is to say not 'inspiration' as we commonly think of it, but the breaking down of strong habitual barriers — this disturbance of our quotidian character" applies to science as much as to poetry.

Many examples of scientific serendipity have been given, and I shall explore later the origins of selective herbicides in this context. Powerful current examples from medical research include interferon, antibody production by hybridomas, and recombinant DNA technology, none of which grew from research oriented towards specified practical objectives. But it is not only the great steps forward that come this way. One research technique, the electrophoretic separation of proteins, now rather unexpectedly provides the quickest and surest way of checking the identity of wheat, or even of meat when necessary.

Secondly, scientists are so acutely aware of the internationalism of science, of it advancing by the free exchange and testing of techniques, results and concepts from many countries, that they feel uncomfortable with attempts by their own country to monopolize the benefit of their work. They know it grew out of the great informal international collaboration of science, and they hesitate to see any one nation take advantage of it. As Anton Chekhov put it: "There is no national science just as there is no national multiplication table; what is national is no longer science."

There are also problems of motivation and recognition, which I come to later. Every politician knows that politics is the art of the possible, so I am surprised by their lack of empathy for Medawar's view that science is the art of the soluble. No scientist wishes to spend his life butting his head against a suburban brick wall.

## The roots of technological advance

Just as scientists are aware of the unpredictability of scientific advances, so are technologists aware of the need to specify objectives if science is to be applied effectively. Each group can call up anecdotal evidence in support of its view, and both government and public find it hard to discern where the truth lies.

It was in this context that the U.S. Department of Defence established "Project Hindsight", to examine retrospectively the relative contributions of science and technology to the development of recent weapon systems. Reporting on this extensive study, Sherwin and Isenson (1966) draw attention to the large number of "events" (i.e. advances) in science and technology that are needed to make each new weapon system possible, to the synergistic effects between many of these, and to the creativity required to mesh them together. For the systems they examined, the requisite 'events' were overwhelmingly (92%) technological, and most of the science events came from applied research oriented to defence needs. They concluded, therefore, that there were great advantages in such work being funded and managed by the Department of Defence. Given the nature of the projects examined, and the relatively short time scale for the inclusion of "events", these findings are not altogether surprising. Moreover, no allowance was made for the relative importance of the many contributing "events".

A more wide-ranging study was published in 1968 by the Illinois Institute of Technology Research, under the acronym of TRACES, standing for Technology in Retrospect and Critical Events in Science. What fun Walter Murdoch would have had with that! TRACES dealt with magnetic ferrites, video tape recorders, oral contraceptives, electron microscopes and matrix isolation, and examined 'events' over a longer time period than that in Project Hindsight, with very different results. In this case, 70% of the advances came from non-mission research, 20% from mission-oriented research, and 10% from development and application.

One reason for the difference from Project Hindsight conclusions was the much longer time lag between 'event' and application for non-oriented compared with oriented research, with a mode of 20-30 years for the former compared with less than 10 years for the latter.

The TRACES study was extended to several other retrospective cases by the Battelle Columbus Laboratories, which concluded that 34% of the significant events came from non-oriented research, 38% from oriented research, and 26% from technological development (Mosteller, 1981).

One of the most satisfactory retrospective studies, in the length of its time profile and the objectivity of its evaluations, is that by Comroe and Dripps (1976) on the sources of ten major clinical advances in cardiovascular and pulmonary medicine and surgery. Like Project Hindsight, their study emphasized the many and diverse elements that contribute to each innovation. But their highly objective analysis of 529 key advances indicated that 41% came from research which was not clinically oriented at the time it was done, and that such research eventually provided a handsome though unpredictable yield of useful discoveries.

Clearly, both curiosity-oriented and mission-oriented research are needed to achieve major innovations. Indeed, many kinds of science, from many disciplines, must usually be brought together in each such innovation. Both conceptual and technical advances in science may open up wholly new opportunities, but these usually require a great deal of adaptive research to turn opportunity into reality. Moreover, while the innovation process often proceeds from basic research towards its application, these applications may also open up new opportunities for basic research. There is, indeed, a synergistic and reciprocating relation between all kinds of science in the process of technological innovation, as a brief look at some of Darwin's research will show.

## How agriculture helped Darwin

Charles Darwin and Walter Murdoch were utterly different in many ways, but both were gentle, humane, questioning men with a puckish sense of humour. Both are remembered as old: Murdoch as the growling sage, Darwin as the white-bearded thinker. When Murdoch was born, 107 years ago today, Darwin had completed his last revision of the 'Origin', was working on insectivorous plants at Down, and was soon to begin his autobiography. The Transmutation Notebooks he had begun 37 years before, on his return from the voyage of the Beagle, had long since ceased to be quarried by Darwin for his later books. Yet it is those four notebooks (B-E), now edited and published, that reveal so well how science advances. In them one sees the zig-zag uncertain route towards a major concept, the great range of unlikely and chance material that contributes to it, the voracious synthesizing power of the mind, the hesitations along the way, and the fresh perceptions that didn't hold up or weren't followed up. Of the latter, let me give just one example which needed almost 150 more years to reach triumphant fruition in the concept of plate tectonics: "Speculate on land being grouped towards centre near Equator at former periods and then splitting off" (B 72).

But my particular purpose in mentioning these Notebooks is because they make clear the many ways in which Darwin used the accumulated experience and insights of plant and animal breeders in the conception, development and testing of his great theory. As he says in the introduction to 'The Origin': "At the commencement of my observations it seemed to me probable that a careful study of domesticated animals and of cultivated plants would offer the best chance of making out this obscure problem. Nor have I been disappointed: in this and in all other perplexing cases I have invariably found that our knowledge, imperfect though it be, of variation under domestication, afforded the best and safest clue. I may venture to express my conviction of the high value of such studies, although they have been very commonly neglected by naturalists."

The four Transmutation notebooks fully support this comment made 20 years later, from the discussion in the first notebook of the differences between European and Indian cattle (e.g. B 132), to the entry in the last which reads: "It is a beautiful part of my theory, that domesticated races of organics are made by precisely same means as species — but latter far more perfectly and infinitely slower" (E 71).

The first major step towards his theory was recognition of "a constant principle of change", and Darwin's profound knowledge of the domestication of animals and plants certainly contributed many insights to this recognition. Indeed, Darwin's own copies of the pamphlets by Sebright (1809) on "The Art of Improving the Breeds of Domestic Animals" and by Wilkinson (1820) on "Remarks on the Improvement of Cattle", now in the Cambridge University Library, are both annotated with this phrase.

Darwin's next major step, his recognition of the power of selection in changing organisms, was entirely due to what he learned from plant and animal breeding. In the early notebook entries, Darwin often refers to the selection or "picking" by breeders of what he called 'monstrosities', such as showy flowers or fancy birds, but it was difficult to relate these 'perversions' to changes in nature. About March 1838, however, his reading of the Sebright and Wilkinson pamphlets led to his triumphant recognition, on a par with that following his reading of Malthus six months later, of the power of sustained gradual selection as practised by animal breeders to effect quite profound changes. As Wilkinson wrote: "by such procedure, animals have at length been produced, so different from the generality of the stock from whence they were originally taken, that none but such as are well acquainted with these matters, could have any idea, that there existed between them the least affinity . . . The longer also these perfections have been continued, the more stability they will have acquired, and the more they will partake of nature itself." This last sentence was heavily marked by Darwin, and many entries in the Notebooks indicate how crucial this agricultural analogue was to his recognition of the mechanism of change in nature.

Thirdly, whereas, unlike Wallace, Darwin had originally thought there was little variation in nature, he changed his mind "from studying, lately, domestic variation, and seeing what an enormous field of undesigned variability there is ready for natural selection to appropriate for any purpose useful to each creature" as he subsequently wrote in a letter to Asa Gray (June 5, 1861).

I have examined elsewhere (Evans, 1982) the ways in which Darwin went on to use his knowledge of domesticated organisms to solve many of the problems posed by his theory and exposed in his Sketch of 1842 and his Essay of 1844. They can be seen in their full glory in his 'Big Species Book' (Stauffer, 1975), but only in muted form in 'The Origin', possibly in deference to Wallace's belief "that no inferences as to varieties in a state of nature can be deduced from those occurring among domestic animals".

## How Darwin helped agriculture

Darwin, against the currents of his time, clearly relied a great deal on mankind's experience with domesticated organisms to shape his theory about species in nature. Several of the insights he gained from plant and animal breeders were crucial, and their experience offered the only approach to experimental demonstration available to him.

By the same token, Darwin made a profound contribution to agriculture. Although he did not solve the mechanism of inheritance, as he had hoped to do, the enormous effort he made to understand it resulted in his being able to write, in both 'The Origin' and 'Variation', succinct and workable accounts of the behaviour of inherited characteristics that were valued and widely used by plant and animal breeders for many years.

William Farrer, for example, gained from 'Variation' the stimulus and ideas which led to his work on the breeding of Australian wheats (Evans, 1980).

Beyond that Darwin gave to plant and animal breeding a broad evolutionary perspective which was extremely useful in spite of the continuing confusion over heredity. Moreover, as a result of his great work on domestication, natural scientists began to take more interest in agriculture and horticulture.

In a letter to J. S. Henslow of April 1, 1848, Darwin wrote that the "instinct for truth, or knowledge or discovery" which he recognized within himself should be "reason enough for scientific researches without any practical results ever ensuing from them". Nevertheless, he also mentioned by way of example the great, but wholly unexpected, practical value of the discovery of chloroform. He would, I am sure, have been delighted with his own equivalent of the chloroform example, namely the selective herbicides derived from experiments he carried out at Down 100 years ago.

In his later years, after the turmoil of the evolutionary debate, Darwin turned increasingly to plant physiological experimentation, with the result that he was finally elected to the Institut de France in the botanical sciences section after having been turned down earlier by the zoology section. As his son Frank said, "He was guilty of evolution, but with extenuating botanical circumstances."

It was with Frank that he conducted his ingenious experiments with hooded and painted seedlings of cereals and grasses, from which he concluded that "some influence is transmitted from the upper to the lower part, causing the latter to bend". This highly original deduction was strongly opposed at first, but was confirmed 20 years later by botanists in several countries. Then, 50 years after Darwin's experiments, this transmissible "influence" was isolated into agar by F. W. Went, in 1926, and chemically identified by Kögl, Erxleben and Haagen-Smit in 1933. They identified a third active substance, indole-acetic acid, the following year, and this was synthesized by Went and Thimann in the same year and shown to influence growth in the same way as did the natural plant hormone. Several related compounds were therefore synthesized in order to determine the relations between chemical structure and functional activity.

Curiosity was the motivation behind all this work, which revealed that naphthoxy acetic acid and several phenoxy acetic acids were highly active (Peterson, 1967). By the end of the 1930s, these compounds were known to have a number of potentially useful applications, such as the promotion of rooting in cuttings and of fruit setting and ripening in various horticultural crops.

Then in 1941 Pokorny reported, in a mere 27 lines of purely chemical description, the synthesis of 2,4-dichlorophenoxy acetic acid and 2,4,5-trichlorophenoxy acetic acid. This was unpretentious science with no hint of either conceptual advance or practical application. Yet, in view of the work already done by Koepfli, Thimann and Went on the relation between the structure and physiological activity of related compounds, it led Zimmerman and Hitchcock at the Boyce Thompson Institute to apply them to several plant systems. The two compounds were found to be highly active, but Zimmerman and Hitchcock made no mention of their possible use as herbicides in their 1942 paper, even though they had both caused some distortions of plant growth. All discussions of the possible uses of such 'hormones' up until that time were within the framework of positive effects, initiating roots, setting, holding or ripening fruit etc.

It required E. J. Kraus to view these compounds in a different light, to envisage them as potential herbicides, and to begin the research that led others, such as Hamner and Tukey (1944), to establish this possibility. 2,4-D was introduced as a commercial weed-killer in the U.S.A. almost immediately, while 2,4,5-T was released three years later. The huge agrichemical industry was founded on 2,4-D and DDT, yet at almost every step along the way those who want science to be 'relevant' would have joined Senator Proxmire in making his annual 'Golden Fleece' awards:

(1) 1880 award; to Darwin, for the soft-headedness evident in his suggestion that plants produce transmissible influences;
(2) 1926; to Went, for the ultimate in uselessness, putting coleoptile tips lopsidedly on agar blocks;
(3) 1933; to Kögl *et al,* for identifying so-called plant hormones from the urine of Gouda cheese-eating Dutchmen;
(4) 1937; to Koepfli *et al,* for the hopelessly academic exercise of inventing rules for plant hormone structure;
(5) 1941; to Pokorny, for some chemical cookery with neither use nor conceptual value.

Yet without these apparently useless pieces of research to build on, Kraus, Hamner and Tukey would have had only copper and arsenic salts, chlorates and other such dangerous, unselective and unsatisfactory contact herbicides on which to base their experiments. Darwin's curiosity, and that of his successors, has led, indirectly but inexorably, to one of the most powerful and pervasive innovations in the whole of agriculture. 2,4-D and its successors have made possible the selective and effective control of weeds, raising crop yields, simplifying crop rotations, reducing energy use in crop production, and allowing the development of minimum tillage techniques. These latter are having an enormous impact on cropping, through the increased flexibility and timeliness of cultivation which they permit, as well as through the reduction in soil erosion. This makes possible the cropping of previously uncultivable land, as well as an increase in the intensity of cropping, thereby increasing world food production through greater yields, faster crop turn-around and increased areas under cultivation.

Thus, Darwin's apparently useless work on movement in plants, amplified by progress in several interlocking areas of science, led eventually, in wholly unpredictable ways, to a technological revolution in agriculture. As Dorothea Brooke's loquacious uncle says in George Eliot's 'Middlemarch', a book admired by both Charles Darwin and Walter Murdoch, "I went into science a great deal myself at one time, but I saw it would not do. It leads to everything; you can let nothing alone."

## Rothschild and the scientific estate

How, then, should we manage our scientific estate, like a national park, by letting it run wild, or like a national gallery, buying only what we want?

There will never be agreement on this question, but we can at least discern parts of the answer from an experiment launched ten years ago by Lord Rothschild's Green Paper of November 1971 entitled "A framework for Government research and development". This 'brisk, brusque, iconoclastic' report, as The Times editorial of Nov. 26, 1971 referred to it, was the work of one man, Lord Rothschild, but it was published together with the very different ("bland, conciliatory, evolutionary") report of the Dainton committee. As Lord Kennet said in the subsequent House of Lords debate, one of the more illuminating of the debates which followed the publication of the Green Paper, to turn from Rothschild to Dainton is "like turning from Sapper to the later Wordsworth". But it is the former we should discuss because the subsequent Government White Paper of July 1972 adopted Rothschild's recommendations almost without change, in spite of the extensive and acrid debate they had generated. As a result, however, we are now at least in a position to see whether the dire consequences forecast by the scientific community and its allies have come to pass. For Rothschild's vigorous but rough and ready and sometimes cryptic English expression, and his not always well-argued assertions, at least succeeded in bringing science back into the arena of public discussion, as witness four editorials and more than forty letters in The Times. They also acted as a catalyst to bring the various parts of the scientific and technological community together although, as 'Chemistry in Britain' (*8*, 367/8, 1972) put it, other catalysts might have acted "more selectively, more completely, and at lower temperatures and pressures".

What generated all the fuss? Above all else, it was Rothschild's principle that all research undertaken by the Research Councils of Britain "with practical application as its objective, must be done on a customer-contractor basis. The customer says what he wants; the contractor does it (if he can); and the customer pays". This might have been acceptable as a general guideline to be used where appropriate, but it was Rothschild's recommendation that it be applied to three quarters of the work of the Agricultural Research Council (ARC), to half that of the Natural Environment Research Council (NERC), and to one third of that of the Medical Research Council (MRC), combined with his recommendations that "the Research Councils should not have the right to reject such contracts without good reasons agreed with the sponsoring executive department", and that if their requirements are not met, these departments "can and, doubtless, will go elsewhere with their money", that generated such fierce opposition. His gratuitous references to gratifying the desires of the research workers, and to 'the monopolistic position' of the Research Councils, in his accompanying memorandum to the Commons, added fuel to the fires, and provoked vigorous responses in The Times, in Nature and in the House of Lords debate, as well as in a comprehensive paper by the Royal Society. The crude commercial orientation of the customer-contractor principle, the monopoly it would give to government departments, the illogicality of excluding the Science Research Council, the inefficiency of conducting long term work like plant breeding on a short term basis, the effects on the morale of the research workers and many other defects were forcefully pointed out.

The likelihood that the designated government departments would be inadequate representatives of the ultimate beneficiaries of such research, whether in agriculture, medicine or the environment, was seen as the Achilles heel of Rothschild's principle. The 'customer' for agricultural research, for example, must be a complex blend, indeed a compromise, between farmer, food processor, consumer and policy maker. Moreover, how were the customer departments going to recruit and retain, let alone listen to, the requisitely perceptive and balanced high level scientific advice? Only good scientists could sense the new opportunities being created by research, or whether a practical problem was ripe for solution, if indeed even they could. No customer would have thought to ask for most of the major benefits that have come from research, so how could it be wrong, as Rothschild said it was, that no customer commissioned or approved it.

Part of Rothschild's objective was, of course, to strengthen the representation and role of scientists at the highest levels in the various government departments. Indeed, his customer-contractor principle was a Trojan horse with that objective. But, as many respondents pointed out, it would be difficult to attract good scientists to such positions, and even if they were attracted, the non-scientific mandarins of the civil service might not altogether welcome them and their advice. And so it has proved to be.

One of the most discerning responses to Rothschild was that of Harvey Brooks (1972). Given the heat generated by 'the Great Debate', Brooks' statement that "I find myself struck by the mildness of Lord Rothschild's proposals when compared with American reality", which had operated on the customer-contractor principle since World War II, was a refreshing change. It also added point to his comment on Rothschild's apparent obliviousness to the weaknesses and dangers of the American system, which he listed as the instability of funding, the lack of concern for the integrity and viability of scientfic institutions, the wasteful competition, the confusion of technological virtuosity with scientific achievement, the increasing obsession with narrowly conceived 'social relevance', and the exacerbation of grantsmanship and political manoeuvring. Brooks also indicated the dangers of relying too much on the needs as perceived by customer departments: "It seems doubtful that weather satellites would ever have been developed by the U.S. Weather Bureau, or nuclear reactors by the Federal Power Commission, or radioactive tracers by the Department of Health". So the success of the customer-contractor relationship hinges, he suggested, on how it is interpreted and handled. Success is unlikely if the customer tries to override the scientific judgement of the contractor.

In the event, while the worst fears of the scheme's opponents, such as "a Lysenko in our Ministry of Agriculture", have not been realized, neither have its objectives. Despite a greater load of committee work and additional bureaucracy, there has been no conspicuous re-direction of research. In the ARC, the Joint Consultative Organization and its special committees, which have at least provided a useful forum for discussions between research and extension workers and the industries they serve, have been streamlined to a small Board. Two of the customer departments of NERC, the Department of the Environment and the Department of Industry, no longer want to fund research which is not directly relevant to their work. As for the MRC, it has now been agreed that its customer-contractor arrangements with the Department of

Health will be completely abandoned as 'impracticable', given that Department's continuing difficulties in the recruitment of the high level scientific advisors so crucial to the success of the customer-contractor principle.

## Useful science and responsive scientists

"Between the idea and the reality falls the shadow" wrote T. S. Eliot. The shadow in Rothschild's and similar excursions into the suburbanization of science is projected by the unpredictability of the end uses of research on the one hand, and by the shared understanding among scientists about the most promising directions for further development on the other (Gibbons, 1980). Solutions to problems cannot be commissioned, and just as science is a communal activity in the sense that it is built upon repeatable facts and supportable constructs, so also does peer assessment play a key role in where and how it develops. As the U.K. Science Research Council (SRC) noted rather sheepishly in its 1974-5 report, the stimulation of research in areas selected by the SRC for special support in the national interest is good when the scientific community is already active there, but fails when it is not. Administrators who are tempted, in what they perceive to be the national interest, to break the power of peer assessment in science should recognize that, were they to succeed, the very procedures and motivations which make science so useful to mankind could also be destroyed.

I am not saying that *scientists* know the best way forward, nor that they should be free of the restrictions that others must labour under, but that *science* will most effectively serve the community when its opportunities and priorities are not distorted by ever greater demands for relevance to currently perceived needs.

Instead of blinkering science in this way, I would prefer to see us drop our pigeon-holing of science and join Louis Pasteur in recognizing that "there is no pure science and applied science, only science and the application of science". We have seen the strength that Charles Darwin both derived from and gave to science by ranging freely across it, regardless of the barriers of discipline and intellectual snobbery, and there is a lesson here for universities in their teaching as well as for bureaucrats in their management of science.

Given the limitations to our individual capacities, however, it is also important that we find ways of extending the interactions between scholars in all branches of learning, between scientists and technologists, between academia, industry, government and the public. Surely this is the best way to ensure that scientists recognize how their research can serve the community, by enhancing rather than restricting their perceptions. Here again universities have an important role to play, as the intellectual crucibles in which each scientist's interests and concerns are developed and amalgamated, and I believe Walter Murdoch would be pleased with the way their interactions are encouraged on this campus.

In this lecture I have concentrated on the relations between science and government, and particularly on the dangers of trying to domesticate science, to break its independent spirit, prescribe its course, and give it a more utilitarian direction. But these pressures are also being applied to universities, where they could be just as destructive, and just as harmful to the community in the long run. Walter Murdoch would have resisted them by poking fun at them, by exhorting us "to practise the noble, the dignified, the philosophic art of sitting about", and by growling. I hope I have at least fulfilled the sacred Murdoch duty of growling.

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