The Vexed Question of Research Priorities: An Australian Example

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ABSTRACT *This paper discusses the nature of the research priorities debate in Australia, and traces the working out of that debate over recent years. The discussion is embedded in an account of how the institutional structure developed to allocate funds for research and how mechanisms were put in place to try to establish national research priorities. It is argued that the prioritising processes developed by the Australian Research Council (ARC) and the Australian Science and Technology Council (ASTEC) during the 1980s and early 1990s are adaptable enough for current and future use, but that by 1996–7, the possibility of a sustained effort to work out national research priorities appeared remote.*

Keywords: research priorities, science policy, research coordination, foresighting.

Introduction

When Vannevar Bush advised President Truman at the end of World War II that he should establish a body for the promotion and funding of research, a body that was to become the National Science Foundation, he argued powerfully, and in the event successfully, that the researchers so funded should be allowed to follow their own sense of what was important. If not the father of the linear model of R&D he was certainly a most important early propagator. New products and processes, he said, 'are founded on new principles and conceptions which in turn flow from basic scientific research. Basic scientific research is scientific capital'.[1] The notion that researchers should decide what was worth researching was not hard to accept, but that they should be funded to follow their own instincts was more novel. Understandably, it was a view that found ready acceptance in universities and research institutes. As higher education and research generally grew in importance in the postwar world the cost of supporting researchers in their enterprises grew more significant. In the 1960s, and more frequently in the 1970s and 1980s, governments began to ask whether in fact there was a better way of allocating research funds, and even to suggest that such allocation should be guided in some way by national needs or priorities.

Such propositions were always fiercely opposed by the research community, and not simply because they contradicted the most sacred doctrine of 'The Book of Research'. At one level any dispute over research priorities was a dispute over territory. At another level it was a dispute over autonomy or power. At a third level it was a dispute brought about by misapprehension: scientists (particularly) did not understand, or want to understand, what was being proposed, while many of those who were keen on the notion of priorities did not really understand, because they had never experienced it, the culture in which most researchers worked.

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What follows is an essay on the nature of the research priorities debate followed by an account of the working-out of that debate over several recent years in Australia. It is written by one of the participants, someone who in the mid 1980s wore three complementary hats—as Chairman of the Board of the Institute of Advanced Studies of the Australian National University (1986–8), as Chairman of the principal Australian research funding body (the Australian Research Grants Committee from 1986 to 1987, and from 1988 to 1990 the ARC), and as a member of the ASTEC, 1986–92. Something needs to be said about each of these institutions.

The Australian National University (ANU) was established after World War II to provide Australia with a world-class research institute which would train Australia's best researchers in fields thought to be important to the nation. By the mid-1980s most of that research endeavour was concentrated in the University's Institute of Advanced Studies, a collection of largely autonomous schools and units organised around disciplines (physics, chemistry) or fields (medical research, social sciences). The Institute had secured one Nobel prize, in medicine, in 1963 and would win another in 1996. Like all large research institutes, it had insufficient funds (the wave of enthusiasm for such an institution had long since passed) and no clear sense of how to allocate money other than historically (since almost by definition anyone appointed to it was 'excellent').

The Australian Research Grants Committee was established in 1964 and recommended its first grants in the following year. Formally, it advised the appropriate Minister (Science and Technology, or Science, in the early 1980s) and did so after extensive peer review of applications from academic researchers to be funded for particular projects. Its methods of operation and its situation have been described in an earlier article.[2] The ARC was its linear successor, and was set up in 1988 following a comprehensive report on the funding of research by ASTEC and a major change in the structure of government which made the establishment of a new body to fund and oversee academic research relatively easy. The ARC was matched in both resources and reach, in the field of medical research, by the National Health and Medical Research Council. To the side of both lay a congeries of research and development corporations with an interest in the science and technology relevant to the primary industries.

The Australian Science and Technology Council was established in 1978 as the principal *policy* body for research, and not just for that carried out in universities. It was in some sense a compromise. Organised science wanted a body that advanced the cause of basic research, a cause that had little power to attract the continuing interest of government. In time, as government began to see the connection between science and technology and national goals of one kind and another, sympathy increased for the creation of a national advisory council on science and technology that would report on outcomes and possible strategies. It was always chaired by an eminent scientist, but its members included businessmen, a primary producer, a trade union official and a couple of academics from the social sciences. It had its own secretariat, and played an important role in organising public debate about these issues, by publishing high-quality papers on one or other aspect of its domain.

This article does not attempt to be the whole story, and is written partly in the hope that it will prompt others to contribute their own accounts. It is probably important to know that my own background is in history and political science.

The Nature of the Priorities Debate

The holy writ of research, which I grew up with, states that no person shall tell another what he or she should work on. A person's research agenda is a uniquely personal attribute, and one builds one's reputation in research through one's success in forwarding that agenda. One demonstrates one's genius, first by knowing what a good question is, and second by providing an ingenious answer to it. One's peers, or more accurately (especially when one is young) one's elders and betters, are the judges whose decisions are given each time one publishes a paper or a book or seeks a research grant. Everyone is involved in the same game. It is a game against nature, and 'understanding' is the goal. Great prizes go to those who find elegant short-cuts and thereby allow thousands of others to travel more quickly to their own understanding. Research will presumably cease when we know everything (when we know 'the mind of God' as Hawking[3] puts it). That time is a long way off.

There is no room in this vision for externally set research priorities, for it is the work of researchers, following their own sense of what is important, that provides the knowledge we presently have. And even the researcher herself cannot be sure of the final worth of her own discovery. The history of research is rich with instances of apparently small-scale or uninteresting discoveries proving to be of immense importance at another time or in another field. Penicillin and the transistor are two much-cited contemporary examples. The process of finding out something important when looking for something else which is unrelated is seen by some almost to be the primary characteristic of the best research, and it has a name, 'serendipity', drawn from a tale by Hugh Walpole about three princes from the land of Serendip (an old name for Sri Lanka) who were always making accidental discoveries. From this perspective, research findings cannot be predicted, and if they could be predicted there would be no point in doing the research, for we would already have the necessary understanding. The existence of research priorities presupposes that findings can be predicted. To be in favour of research priorities is *ipso facto* to display a lack of knowledge of research that should disqualify one from having anything to do with the allocation of research moneys. And so on.

I have tried not to caricature this position, which is passionately held to be true within much of the university system, especially among natural scientists. I myself once took it to be true, since it accorded with my own experience of research as a human process. But as I became more deeply involved in questions of research policy I grew to see that there were other important dimensions to the research endeavour of a country, and that these were necessarily bound up with questions of funding. I enjoyed my time on ASTEC especially because these questions were argued out there, and because they were inherently political and therefore familiar to me. By the time I was heading the ARC I was deeply convinced both that research priorities were necessary for Australia and that the Council should build much of its sense of mission around them.

The virtues of a self-conscious approach to national research priorities, I believed, could be argued as simply and as cogently as the opposite view set out above. A country like Australia produced about 2% of the world's research, and was only a small player in the game. It could not, and should not try to, do everything. Rather, it should seek to work out what areas of research its national interests demanded that it be good at, and to make sure those were of world class. If there were any money over, the next priority would be to enhance the work done in research fields clearly related to those of national importance, and so on. To say this was not to say that everyone should be doing applied research; rather, it would be to say that so-called 'pure' research would be deliberately encouraged, through specific funding, in areas where the national interest demanded it, and left to the universities, philanthropists or individual initiative where the national interest was not involved. Of course, granting bodies themselves should have a role in deciding what those national interests were, because slowly emerging opportunities can be as important to nations as immediately pressing needs, and researchers themselves can have a better sense of opportunity than people in government or industry. Granting bodies could also be used as instruments of national policy, to support research fields or questions thought to have a national importance. Granting bodies could thus serve two distinct but related purposes: directing research money to areas agreed to be of national importance, and allocating funds to excellent researchers, whatever their field. A perspective like this started not from the freedom of researchers but from the need to raise and justify public expenditure.

Research priorities, thus described, were thoroughly political, but that did not disturb me. The political was familiar and ordinary to me, and in fact anything that a society regards as important is bound to be political in some significant sense. But to a researcher from the ancient tradition, things political were an anathema in research. Excellence alone should rule. And since research had not been important to governments until the 1980s, Australian research, in the Commonwealth Scientific and Industrial Research Organisation (CSIRO), in the universities and even to a degree in industry, was run by the researchers themselves; the ancient tradition did rule.

The great weaknesses of the ancient tradition were that it was so easily demolished by questioning, and that its budgetary implications were open-ended. The approach that researchers have taken to governments, in most Western countries, has been not inaccurately pictured as 'Slip the money under the door and go away'! That was possible when research was an activity mysterious in character, apparently potent in effect and engaged in by only a few—the research that produced the atomic bomb, for example. But as research became a more familiar activity, as more and more politicians and public servants went to university, and as the scale of the research activity and its budgetary consequences both grew larger, those writing the cheque increasingly wanted to see what they were getting for their money.

An additional stimulus, after the early 1970s, was the failure of most Western economies to grow at the rate that had been true of the 1950s and 1960s. It was no use scientists' arguing that Western economies had been built on the wealth flowing from the discoveries produced by pure research. That wasn't so. To the extent that research had been instrumental in developing the European, North American and Antipodean economies in the 19th century (research whose products were steam locomotion, electricity, high-grade steel and the internal combustion engine), that research had overwhelmingly originated in industry, not in the universities. Nor could it be argued that pure research was leading the way in the late 20th century. The countries with the highest economic growth rates, like Japan, Korea, Taiwan and Singapore, were plainly choosing not to do pure research but to concentrate on engineering, product development and marketing. Britain, with the world's proudest tradition of high-quality pure research, had one of the Western world's most ailing economies. Australia, in some respects a colonial admirer of the British tradition, was no better off despite its quite respectable pure research endeavour. The linear model simply didn't seem to be true. If anything, the causal arrows seemed to point the other way: pure research was something countries did when they *became* wealthy; it was like investing in opera, ballet and other examples of high culture.

This sort of talk was unpleasant to those inhabiting the ancient tradition, and one response was to blame industry: Australia's *public* expenditure on research was, if not the highest in the world, then certainly not embarrassingly low. But its *private sector* research expenditure was close to the bottom of the world heap, and had fallen since the mid 1970s. In the mid 1980s Australia was devoting a little over one per cent of gross domestic product to R&D, and about three quarters of that one per cent was public expenditure. Some of the industrialised countries of Europe were spending at least double the Australian proportion of GDP on research and development, and the greater part of that was coming from the private sector. In the Australian case private sector spending had actually declined since the early 1970s, when tariff reductions decided on by the Whitlam Government made the financial condition of many Australian industrial companies precarious. In response to continued criticism governments looked for ways to encourage private firms to invest in R&D again, and had a degree of success in doing so.

But this did not greatly stimulate research in the universities, and it was not hard to find an explanation. Australian firms were not in industries where high levels of expenditure on R&D made a lot of sense. We had no large firms in aerospace, or pharmaceuticals, or electronics, the 'high-tech' industries where a close connection with the university research might reasonably be expected. Many of our largest firms were in finance and retailing—what sort of research should they be doing? Our motor vehicle industry was foreign-owned, and its R&D was done at home, not in foreign outposts like Australia. Where we had world-class industries, like agriculture or mining, they were among the world leaders in the extent to which they conducted and used R&D, but their levels of expenditure on R&D were not high (and for most mining companies 'R&D' meant exploration!). We had some very clever people in Australia, but if their fields were in physics or chemistry or the higher forms of engineering they were quite likely to be consultants for one or other foreign corporation, not for an Australian firm.

The uncomfortable truth was that Australia's research endeavour was what you'd expect it to be, all things considered, once you examined what the country did for its living. We did indeed have national research priorities of a kind, although we denied it. While we produced about 2% of the world's scientific research in all, we produced 4% of the world's biological research and around 1% of its physics research, proportions which make good sense given our great dependence on plants and animals, our exotic ecosystem and the fact that our defence industry was relatively small and devoted to providing local content (physics is usually strong where nations are, or seek, to be militarily powerful). And despite the talk that we had a scientific crisis on our hands, Australia led the world in the production of natural scientists as a proportion of all graduates. If governments wanted to get a better handle on the nation's research activity, therefore, it would have to start talking priorities.

My hindsight, like that of most other people, is much more accurate than my foresight. At the end of the 1970s I saw the research world very much in terms of the linear model: we researchers had good ideas, and then other people developed them into products, processes or policies. Yet even then I had a strong feeling that at least in the social sciences we ought to be doing research that was important to the society we lived in and fed us; and we needed to stay close to the decision-makers and offer to tackle the difficult questions. I explored that general theme in a presidential speech to the Australasian Political Studies Association in 1980, and kept returning to it in the years ahead. Towards the end of 1981 I encountered John Button, who was an irregular correspondent of mine and at the time the Opposition's Shadow Minister for Education and Science, at the Society restaurant in Melbourne, where we spent a couple of hours discussing where Australia was and why it was that way, a subject on which I gave lectures from time to time. His contemporary perspective gave point to my own rather more historical analysis, and the discussion pushed me into thinking that all publicly funded research ought to have some kind of plausible and defendable public pay-off. Why else would the ordinary taxpayer be providing the money?

In 1981 I was appointed as a member of the Australian Research Grants Committee

(ARGC) for a 5-year term, and a couple of years' experience of the ARGC process tended to reinforce these worries. Not only did a lot of the research we funded, in all disciplines, seem to be pretty humdrum stuff, but no-one outside the university world seemed interested in even the really good research which we also funded, not to mention the humdrum. Moreover, it was plain that there were problems of considerable moment in the world outside the university which academic researchers were not interested in tackling. Indeed, we seemed almost to delight in *rejecting* the problems of the world. They were defined as 'applied' and we were 'pure'; or something called 'industry' should be funding these real-world problems if they were truly interesting; or they were 'Australian' and we had to be careful not to be parochial, because international referees were not interested in anything parochial. And at the same time we were proud that we had at least one researcher in almost every known field of human endeavour. We had real depth in hardly anything, no critical mass. Every new discovery overseas prompted a cry that we must be good in this branch of knowledge too. Since there was no accompanying proposal that any existing activity be wound down, the Australian research endeavour was very wide, but fearfully thin. In this respect, as in others, the Institute of Advanced Studies at the ANU, where I was Chairman of the Board, was a microcosm of the nation.

We had inconclusive discussions about these matters at ARGC meetings. They were inconclusive partly because the holy writ said that something called 'excellence' must rule and partly because we did not have any priorities of our own. If there were to be priorities for the ARGC they would have to come from outside, and who would set them, or have the authority to set them? I could see priorities that made sense to me within my own history and politics bailiwick, but I could not get my own colleagues to agree. Nonetheless, my conviction grew that without a sense of purpose it was hard to justify any given level of funding for our enterprise, let alone for an increase in that funding. And as soon as I was exposed to the kind of arguments that went on at every ASTEC meeting, after I joined that body in 1986, I began to see that 'research priorities', in some form or other, must be a necessary ingredient of future research policy.

In 1986 I was simply one of the recently converted, and was to become a passionate convert later in the year after I went to an international meeting on science policy at Ditchley Park in England and immersed myself in the international literature. In 1987 I met John Ziman, the British physicist who was inventing for himself a new career in science policy. Ziman greatly impressed me, as he did most people with whom he came into contact who were interested in these wider questions. His principal contribution was to argue that the very success of postwar science was strangling it: each advance provided the opportunity for a new advance, but the skilled people and above all the national budget could not sustain a helter-skelter research attack.[4] There had to be a more moderate pace, and there had to be priorities. There would be priorities, whatever we said. The important thing was to make sure that they were understood and explicit, rather than vague and implicit.

Of course 'priorities' were not a new concept The word had never been absent from the research policy debate in the 1980s. ASTEC had conducted a 'National Objectives and Research Priorities Workshop' under UNESCO auspices in 1981, and Barry Jones had established a 'National Technology Strategy' in 1984. Neither was successful, for the critical mood within the research community was strong. But ASTEC kept at it, and the set of papers it produced in the middle and later 1980s kept saying, about one thing after another, and from one perspective and another, that Australia had to be able to make explicit choices in the domain of research. These papers began to have an effect. CSIRO was restructured to make it less an example of the ancient tradition, following an ASTEC report which proposed such a change; the establishment of an Australian Research

Council was argued for in another report, and its priority-advising function was always to the fore in any discussions about its role and purpose.

Ideas take time to be ingested and acted upon. By the end of the decade there were enough people in enough organisations for the question of research priorities to become for a little while a central political question. It was not before time: Australia spent more than AU$3 billion a year on research, and most of that came on one path or another from the pocket of the taxpayer. Two major perspectives were apparently in conflict: that researchers had a kind of right to determine their own agenda and be funded for it, and that the spending of public money in a democracy can only be justified if there is a clear and defendable public benefit. Those of us who were charged with managing the expenditure of that money had to find a way of reconciling these two perspectives.

The Australian Research Council Tries Its Hand

The Council was established by the *Employment, Education and Training Act* of 1988 as one of four constituent Councils of a National Board of Employment, Education and Training reporting to a Minister with the same portfolio responsibilities. The Council was required by the *Act* 'to inquire into ... any matter referred to the Council by the Minister or the Board, being ... a matter relating to national research priorities or the co-ordination of research priorities' (S. 27 (1) (b)). There were four matters mentioned 'in particular'—the support needed for fundamental research and for research which would contribute directly to Australia's social or economic development; measures to concentrate research to best effect; measures to improve research training; and measures to bring industry, the government research sector and higher education closer together. The Council could also initiate any such enquiry at its own volition, provided that this did not prejudice its response to Ministerial or Board references. These were very wide responsibilities, and extended far beyond the boundaries of the higher education system. For that reason alone, they made one or two Council members rather nervous.

From the very beginning, months before the Act appeared, I emphasised as its interim Chairman that the new Council would advise the Government on where new research funds ought to be spent, and why that would be a good thing. At a Conference on the new Minister's Green Paper at the University of New England, in January 1988, I set out why it was that we had to move past notions of excellence determined by peer review as the sole guide for allocating money.

> ... peer review is marvellous once you know how much money there is. But peer review is absolutely useless if you want to ... spend more on biological science and less on physics, more on engineering and less on chemistry, or more on all of those and less on history, etc. Decisions about going this way rather than that way are always made in some kind of political fashion, whether in universities, colleges, government or anywhere else.

Coming through strongly here was the effect of 3 years' endeavour in trying to get past notions of 'excellence' and 'peer review' in developing a strategy for the Institute of Advanced Studies. Research priorities were in my early 1988 strategy paper, and were discussed by the interim Council after its establishment in April. Yet much as I might have wished, I could not pursue all the new policy issues by myself. I had to have colleagues who shared the vision. While most of the academic members of the Council were somewhat leary of research priorities, at least in the beginning, one who was not was Adrienne Clarke, a plant cell biologist at the University of Melbourne and a member of the CSIRO Board of Management (she became its Chairman in 1991). The fact that she was also a Director of a Special Research Centre, and thus one of the stars of the research system in the Australian universities, gave her views on research priorities a special significance. The Council appointed her as chair of a committee into research priorities that soon looked like becoming a taskforce. And as chair also of the Biological Sciences Committee she was able, with two no less talented colleagues, Nancy Millis and Brian Gunning, to design a funding strategy for the biological sciences that was from the beginning built around the notion that some questions were more important for Australian biology than others.

But not everyone in Australian biology agreed that this was so, while others who did agree then disagreed on which questions they were. Involving the professional associations in biology and the leading researchers, which Adrienne and her colleagues did through a widely circulated questionnaire, only complicated matters further. While a few praised her resolve as well as her proposals, others gave the whole endeavour an emphatic thumbs-down. 'Priorities favour funding of second- and third-rate research', thundered one collective opinion. 'If the work were first rate it would be funded irrespective of the existence of priorities'. The view that excellence must rule, whatever national needs were, was a widespread one, and it handicapped the furthering of biological science priorities.

The Clarke experiment taught us some useful lessons. The Council was not in a position to ignore the Act and pretend that priorities were unimportant. But it had to recognise that no-one much in the research community wanted them. It seemed that we would have to try to educate our clients about something they didn't want, and we decided to do that by involving as many as possible of them in the business of deciding what the priorities should be. At the same time, we ourselves would need to have a clear idea of what we thought was important. In 1989 and 1990, therefore, when we carried out two huge consultative exercises that allowed everyone to have their say, we took particular notice of the views of our own disciplinary panels, the learned, academies and stakeholders like the Australian Vice-Chancellors Committee. The result was a set of field priorities which made good sense in the Australian context, were broad enough to have a decent level of support within the research community and were limited enough in number to attract useful money. They were: materials science, cognitive science, scientific instruments and instrumentation, and the use of molecular biology in managing the Australian environment. 'Australian and Asia', strongly supported in 1989, was added in the second year.

The first round was done quickly, in order to demonstrate that we could do the job at all. But I was the first to admit that the exercise had lacked a coherent intellectual rationale. Yes, some things were more important than others, and deserved to be identified for funding. But how would we know what they were? Did this large consultative exercise amount to little more than a vote? If it did, it was not very different to the process that would go on in any organisation, and hardly needed a national council to manage it. Since I was the one who was most insistent that we take the matter seriously, it was my responsibility to find the way forward.

I found it through a culinary metaphor. We needed a 'sieve' or filter of some kind which would allow us to collect a relatively small number of candidates for priority funding, rather than choose among hundreds from a great pot. I developed the sieve notion into a paper, 'The Matter of Research Priorities' which not only became the basis for the Council's priority funding ventures, but allowed us to present all our programs as serving national purposes of one kind or another. As always, there was the need to reassure the nervous research community that the autonomy of the individual researcher was not threatened: people could still choose the research questions they thought were interesting ones. But I could also be a lot bolder about the responsibilities of the research community than I had been in the past.

> Publicly funded researchers have an obligation to respond to the issues and problems facing society. Government and public support for research depends on the perception that research supports national goals. In general, all the programs funded through the Council can be seen as serving seven broad aspects of the public interest:

- the well-being and harmony of Australian society;
- the understanding and advancement of human culture and values;
- the advancement of Australia's geopolitical interests;
- understanding and managing the environment;
- furthering the contribution of research and education to society;
- enhancing the nature and potential of the primary industries; and
- developing the science and technology underlying industrial development.

This was not just a grab-bag of Australian concerns. We were not proclaiming a responsibility for health, for example, or housing, or energy, or defence; those important matters could be left to others. But it was a broadly comprehensive list, and everything we funded could be seen to serve one or other of the goals; some programs or projects, of course, were furthering more than one of them.

Given objectives like these, I argued, the business of setting research priorities became 'a way of encouraging research in a particular field', and we would do that in order to encourage good researchers to move into that field, to encourage an expansion of the field itself, to break down disciplinary barriers, and to get researchers and the users of research together. I was anxious to make it clear that priority research was not by definition short-term research:

> There are issues facing society which are long-term concerns; obvious current examples are the managing of the environment and bringing about structural adjustment. How we respond to such issues will have major consequences for our nation throughout the foreseeable future.

Recognising that every group in society 'will see its own immediate concerns as being of national importance and therefore fully warranting the establishment of a national research priority' (and we had indeed had scores of examples in our first round, with 300 submissions to hand for 1991), the Council needed to have three questions addressed.

Does the national need have a significantly long time-scale? It takes time to develop a research effort in a given direction, and it is not sensible to do so if the 'national need' is a thing of the moment, a passing fashion which may well be replaced by another in a year or two, or with a change of government. The national need should sit comfortably in one of the seven broad aspects of the public interest set out above.

Does the national need possess an obvious and important research and development or research training dimension? There are many important problems which do not lend themselves readily or immediately to a sustained research effort. The Council has to decide not only whether a given national need does have such a dimension, but also which funding program is the most effective vehicle for establishing a given research priority, if one is to be established.

Does that research and development dimension fit the Council's essential role? That is, if the national need does require a substantial research effort, that should be located either towards the basic end of the research and development continuum, in support of other mission-oriented research agencies or private industry, or represent high-quality applied research in an area where there is not another obvious sponsor.

This approach to priorities, adopted by the Council in September 1989, largely defused the priorities dispute within the Research Council and the higher education system. We now had a rationale for what we did, and a shield to protect us against the importunate barrow-pushers who flourish in the research world. We also had given our clients a defence for what they did that could be brought into play if the research endeavour of universities was attacked for partisan reasons, as had happened in 1987 when the Federal Opposition's 'Wastewatch' committee criticised the allocation of grant moneys to apparently irrelevant projects. It was clear to me, nonetheless, that some researchers had little pleasure in the thought that what they did was actually useful!

The Alliance with ASTEC

It had been clear to me for some years that priorities could not simply be left to agencies like the ARC. Higher education was an important locus of the Australian research endeavour, but it was by no means the only important site.

In any case, the nation itself had come to ask strategic questions, for the projects and possibilities that were too large for us, or for CSIRO, would have to dealt with by Cabinet. While we had as yet no mechanisms to help Ministers, there was no doubt that what Cabinet or Ministers said had a profound effect on researchers. A national statement that for the next few years we would, for example, be deeply interested in the Antarctic but not so concerned with space-flight would be an important signal to all the institutions in which the research was actually carried out The national government did not set priorities for organisations or institutions at the lower level, but it was important that it established a framework in which all those who carried out research and development with public money, including the departments and agencies of government, considered what they were doing and why that was worthwhile.

In short, the climate seemed right for a national priority-setting mechanism, the sort of thing done admirably in Sweden and Japan. In May 1989 the Government had responded to sustained criticism about its science policy, especially from scientists working in the CSIRO, by issuing a 'Science Statement' with the authority of the Prime Minister himself. There was to be not only some more money for science, some of it going to, or through, the ARC, but there was to be a Prime Minister's Science Council, comprising senior Ministers, senior figures from industry and a sprinkling of scientists, a new position of Chief Scientist, to be filled by Professor Ralph Slatyer, a former Chairman of ASTEC, and also a departmental 'Co-ordinating Committee on Science and Technology' which was to be filled by civil servants at deputy secretary level, and on which I had a place. The bits and pieces were there, and Ralph Slatyer I knew to be sympathetic to the need for priority-setting. Could all this be mobilised to produce a national priority-setting mechanism for research? If it could, then I could see a means by which extra funding for research could be rationally argued for and rationally considered.

While the Research Council had been given a very wide remit in the matter of research policy, as has been shown, an endeavour of the kind I was considering needed allies, and allies especially in the central co-ordinating departments. And the obvious primary ally was ASTEC itself, the more so because by 1989 I was an experienced member of that body, in my second term. So in May 1989 I proposed that ASTEC make 'research priorities' one of its own priorities for the following year. I said that if it were to do so it would find the ARC ready to be an enthusiastic partner. It was a timely proposal, and was agreed to with some speed. For several members of Council it was the inevitable final step in the direction ASTEC had been travelling for some years; for others it seemed very much in the centre of contemporary policy concerns, and was therefore supportable for that reason. Ray Martin, Ralph Slatyer's successor as Chairman, was keenly interested, and agreed to head the working party. Unusually for an ASTEC working party, this one was to be small and consist only of members of the two Councils: Ray Martin and myself, Ron Johnston, who was the doyen of Australia's science policy thinkers, Jim McLeod, from Sydney University's Faculty of Medicine, and from the ARC, Michael Pitman, at that time the Chief Science Adviser to John Button, Minister for Industry, Technology and Commerce, and deputy to Ralph Slatyer in the new science co-ordination structure set up in the May Statement.

My own Council was enthusiastic about the proposal, if only because we could thus discharge our responsibility to advise the Government on research priorities without doing the job ourselves—indeed, we would be the junior partner, with an appropriately small financial contribution. ASTEC had never done a report in conjunction with another body, and was anxious that its own independence not be compromised. That would never have been a problem, since the ARC was nervous about straying outside the higher education pastures that it knew well, and was happy to leave the initiative to the older body. The alliance with ASTEC therefore suited everybody, and was a most harmonious one.

Setting Directions for Australian Research

The members of the working party knew the area of concern well and for the most part were friends or acquaintances of long standing. What was perhaps even more important, we shared a dislike for the ways things had traditionally been done in Australia, especially when we compared them to the practices that ruled in other countries. The universal antipathy towards priorities in universities was matched in some parts of the wider society by an almost pathological fear of planning, as though it were some kind of Bolshevist plot. In consequence, it was hard to discern more than a basic rationality in what the nation did in the fields of science and technology. These are fields which, like defence, water conservation and electricity generation, demand a long time-scale, consistency and patience. Such a demand was rarely met in Australian science policy. A plausible scientific salesman would persuade a Minister (even better, the Prime Minister) that the country needed, to take an inoffensive example, a thingatron. Cabinet would agree, often without understanding what thingatronics was or what would be needed to sustain the endeavour in later years. A Grand Thingatron would be planned, some initial funds would be appropriated, some appointments made. Because issues pass quickly in politics, thingatronics would before long lose its place in prime ministerial favour to another concern. The outcome would very likely be a small thingatron, slowly built, some years behind world standards when it was finally opened (by another Prime Minister) and run by a group of staff too small to have much impact on the world scene, and increasingly disgruntled. Excellent programs, like the Commonwealth PostGraduate Awards Scheme, to take an actual example, over the years lost their place in the funding queue, with the effect that postgraduate students could not eventually live on the stipend offered, especially if they were married. Fulltime students thus became part-time students, a change not obviously good for the quality of their research, let alone for their morale. Any of us could suggest several similar examples of the consequences of the Australian way. Surely the nation could do it better.

We had no doubt about what was needed: some kind of planning mechanism that subjected all proposals for large pieces of scientific equipment, such as oceanic research vessels and telescopes, new laboratories or major funding programs for personnel, to some kind of regular comparative scrutiny. Once funded, its progress should be regularly monitored, and if the need for it passed, it should be closed, merged with something else, or wound down. For a proposal to survive the first round it would of course have to satisfy some test of national need. Why else should public money be spent on it? We knew that this would be an unpopular suggestion, but we also thought that the time was ripe for such an innovation, and that we had, in the structures set up in the May Statement, the means of persuading Commonwealth departments that there were better procedures available than having one's own proposal for a laboratory or a new research program attacked by rival departments in the co-ordinating comments that went to Cabinet. Why not see all such proposals as part of a common endeavour, and criticised (if criticisms were appropriate) at an earlier stage in the decision process? The new Co-ordinating Committee on Science and Technology heard the plea, nodded in agreement, and waited to see what we would come up with.

But exactly what should such a mechanism be like? The working party assembled everything it could find on what was done in other countries, and our able secretary, Elizabeth Smith, who had considerable experience in this field, began to distil the elements of national priority-setting mechanisms across the world. We concentrated on Japan, Sweden, Norway, Canada and France, and soon discovered, as we were to write in our report, that 'styles of priority-setting tell us as much about the national cultures of these countries as they do about what makes for the successful setting of priorities'.

> The message is plain; for something to work well in Australia it must be congruent with the political, economic and social reality of our country. It must be built on a sound basis of existing practice here, so that the process is likely to be fruitful from the beginning.

The Japanese priority-setting system was complex and many-layered; the Swedish example was, by contrast, closely knit and consensual. The Canadians devoted great energy to consultation, especially between the national and the provincial governments. Each country had a different device to set the priority-setting mechanism going.

We proposed a familiar Australian device, the White Paper, which we saw appearing in the Parliamentary calendar every 4 years. We were no doubt guided in part by the fact that the Government had used the White Paper mechanism recently and powerfully in the fields both of science and technology and of higher education. Those in Government, politicians and public servants, understood the White Paper and how it worked; it was the best procedure available to us. In the first year of the 4-year cycle, we proposed, ASTEC should take charge of a consultation process which would result in an 'issues' paper to be discussed at a national conference. The issues paper would digest the multitude of submissions that would come from a national consultative process on Australia's needs and opportunities in research and development. The conference over, ASTEC would revise the issues paper and pass it on to the Co-ordinating Committee on Science and Technology, which would draft the White Paper for presentation to the Prime Minister, who would introduce it to Parliament once it had been endorsed by his Science Council. So far, some 18 months of the 4 years would have elapsed. The Budgets of the remaining period would be framed, where they bore on matters of research and development, with the White Paper in mind. The implementation phase of the cycle would also be a monitoring period, where those responsible for R&D at the national level not only reviewed the progress of new developments agreed to in the White Paper but also remained alert for new issues which had not been considered during consultation or had suddenly become much more important. Towards the end of the fourth year ASTEC would prepare a new issues paper and invite the widest possible consultation. The process would go into its second round, and continue indefinitely.

We chose a 4-year cycle in order to distinguish what we were seeking from the 3-year electoral cycle that so stamps the business of Australian government. We recognised that electoral politics was inescapable, but we also wanted to insist that these broad issues of research and development had time-scales that were very much longer than the periods between elections, and should not be subordinated to electoral politics. Following an argument of mine with which I had been wearying my ASTEC colleagues for some years, we talked of 'research and development', not of 'science and technology'. While many scientists saw no distinction between 'science' and 'research', it seemed to me that there was a most important distinction. It was true that while most of the *money* that was spent on R&D went in some sense to science or engineering, the country's actual *needs* for research spanned the whole range of human knowledge and a great deal of our need lay in the domain of the social sciences and the humanities; our terminology ought to be appropriate to our purposes.

Our report, *Setting Directions for Australian Research*, was approved by the Councils of both sponsoring bodies, and went to the Prime Minister in June 1990. He liked it and, without endorsing every element of our proposal, made it clear that we should start on the first of the 4-year cycles, by preparing the issues paper which would begin the process. There was indeed a sense of pleasant expectation abroad. The Co-ordinating Committee was still on side, the Chief Scientist approved, the learned academies were relaxed, if not exactly enthusiastic. When the two Councils conducted a seminar in October to gauge the reactions of the research community, the most discordant note was the suggestion that we had not been sufficiently prescriptive!

The Slow Rise and Sudden Fall of Priority-Setting

Since we had our own blueprint to follow, and since we knew that the Prime Minister wanted to make his Science Statement in May 1992, 3 years after his first such presentation, it took little time to get under way. The Australian Research Council was no longer involved—indeed, after the end of 1990 I had become a Vice-Chancellor and was no longer its Chairman—and the operation was wholly under the direction of ASTEC. We had believed that without the most extensive consultation no process of the kind we were undertaking was likely to succeed; that was one common lesson from overseas. So in preparing what became called the 'issues and options' paper we not only invited the entire nation to take part. We also set out on the most elaborate process of meetings and interviews across the nation that I had ever been involved in, discussing the issues in session after session with hundreds of those who had a stake in research and development in Australia. ASTEC committed most of its resources for a year to ensuring that the issues paper was as good as it could be, and that no-one could complain about the level of consultation.

As I had learned already in the Research Council, it was not enough to listen. For these sessions to be most useful, those conducting them had to have a clear sense of what they themselves wanted, and be prepared to argue along these lines. Of course, one had to expect to modify one's vision as the debate continued. But without an initial vision the outcome was likely to be an opaque one. My earlier experience also prepared me for the discovery that few people could see the nation's problems as a whole. Once again, the generality of submissions represented more or less elegantly argued pleas for the enhancement of this or that branch of knowledge.

Nonetheless, the paper that we prepared, *Research and Technology: Future Directions*, was a strong statement of what might be done. We stuck firmly to our inclusive language. 'Research ... embraces all human knowledge and the application of knowledge and inquiry to the problems and opportunities which confront the community'. We even managed to define 'science' to be 'an ordered body of knowledge ... [that] includes the natural sciences, engineering, the social sciences and the humanities'. No more comprehensive approach to research and development has, to my knowledge, been made in our history.

Yet our second paper was far less well received than the first. There seemed to be little likelihood that the Government would widen its conception of 'science' in the way we had suggested: the apparatus of high-level science policy was the preserve of the natural scientists, and they had no wish to share it. As for the spokespeople from the social sciences and the humanities, some were almost outraged that their domain was included at all, and objected to the language used about the social sciences and humanities and their apparent indifference to national needs (language which was all my own doing). Many people again expected a highly prescriptive paper which would smile on one proposal or field of research and frown on others—despite the plain fact that ours was a discussion paper rather than a policy paper. The Co-ordinating Committee was not sure what it had expected, but doubted that this was it. And in any case it lost the task of developing the White Paper to the Chief Scientist and his office.

Then, as so often occurs in matters of high policy, came the intrusion of the larger world into the smaller. On 19 December 1991, the Hawke Government came to an end after a vote on the leadership in the Labor caucus. The new Prime Minister, Paul Keating, was much less interested in the field of research and development than was his predecessor and, again unlike his predecessor, he did not share a common experience at school and university with the Chief Scientist. A May Science Statement appeared in 1992, but was widely regarded as a squib. Well before the end of the year Ralph Slatyer announced his departure, and it was many months later before Michael Pitman was appointed as his successor.

It can take years to develop a climate of thinking and the conditions necessary for policy to be set out, adopted and endorsed. The process can be unravelled, however, in weeks. So the White Paper process for establishing national research priorities died quickly, and the same fate soon seemed to be being prepared for ASTEC. When early 1992 brought the end of my own term and that also of Bob Gregory, an economist good at the innocent but penetrating question, we two social scientists were not replaced. A committee of review, while it reported that ASTEC was both relevant and useful, clearly found our broader papers less useful than those which had a narrower focus. It is hard to read the Report of the Review Committee without feeling that its members felt that ASTEC had somehow gone off the rails in pursuing wider questions like priority-setting. The Report did not suggest that the membership of the Council include social scientists or people from the humanities with an interest in research in its widest sense. If ASTEC were to survive, it seemed plain, it would be a much less ambitious and wide-ranging body than had been the case in the past. ASTEC was allowed to decline in resources and reach, and although it continued with a major 'foresight' exercise it no longer had the status, the staff or the ear of government to make its report a matter of importance. By 1996, after a change of government, the Chief Scientist no longer advised the Prime

Minister but one of his colleagues, and operated on a part-time basis, with one of his responsibilities the chairing of ASTEC meetings. The Australian Research Council, while it continued to prosper, maintained a relatively low profile in matters of national priority-setting. The possibility of a sustained push towards national research priorities now seemed a long way off indeed.

Faced with outcomes like these, people who are engaged in national politics need to take heart from such soothers as 'Rome wasn't built in a day' and 'Many a mickle makes a muckle'. Sooner or later Australia will return to the business of trying to establish a workable procedure for deciding on whether Australia needs a very large optical telescope, or a large oceanographic vessel, or a gravitational wave observatory, or some other large and expensive facility. When it does, it will find the process developed by ARC and ASTEC in the early 1990s adaptable to the new circumstances.

If Australia does indeed succeed in moving forward, that will be so partly because the research community itself and its leaders will have at last come to understand their contemporary situation. In the early 1990s the research community, like much else in Australia, was still locked into an old view of the world and of itself, in which the country ought to be wealthy enough to support an indifferentiated research endeavour in which something called 'excellence' was the only criterion for funding, and winning Nobel prizes and their equivalents was the only important game. In fact, the nation needed much high-quality research for other important reasons, but it has not been able to find a way of reconciling the researchers' wish for autonomy and the Government's need to make every dollar spent on research achieve, if not an identifiable result, then at least a step along the path to a desired outcome. The declining preparedness of the Government to fund research or higher education itself at past levels may finally produce a change of perspective in the research community. And that could be the occasion for a shift to a system of priorities, at first to defend what is being done, but then to allow an expansion in what is being done. Australia needs that no less than any other country.

Notes and References

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