

REPP Conference Speech

I am here today in place of Paul Bourke, and his absence from our company and our lives is a great sadness. Paul would approve of what I am going to try to do, but he would no doubt want to intervene before long and take issue with some of my remarks. It has been so for ten years, and I wish he were here to do it today.

As a historian who became a political scientist and later spent twenty years in the area of research and education policy I have an abiding interest in the question of research funding. I have been responsible for a lot of research spending in my time, and can claim some credit for increasing the quantum of research expenditure. Both activities brought me into close and sometimes daily contact with Ministers, politicians, public servants, businessmen and of course researchers. I was myself a decently successful researcher, have trained and examined a lot of aspirant researchers, and read more applications for research grants than is probably healthy for anyone. All that pushed me into the evaluation of research, and I started the first serious evaluations conducted by the Australian Research Council. Earlier still, when I was a senior officer at the ANU, the then Vice-Chancellor of the ANU, Peter Karmel, asked me to evaluate the work of the tenured staff of the Institute of Advanced Studies, and I have served over the years on a large number of panels of various kinds reviewing Special Research Centres, Key Centres, departments, faculties and even universities.

As a vice-chancellor I am continually interested in the tricky question of how we know whether or not someone is 'good' or 'excellent' in their field. The easiest way is to ask the field how it discriminates. 'Internationally refereed research' publications (which usually means publication in American journals — Americans, oddly, don't insist on publication in Australian journals!) make up only one measure. The most human test is unforced admiration: 'I wish I'd written/said/thought that ...' It is, of course, an essentially private response. Citation counts are a public surrogate, with obvious problems that need no recounting to an audience like this one. The central point is that we need to discriminate between the good and the less good, and in the domain of research and scholarship that is not an easy thing to do. In what remains of this address I want to make some comments about why discrimination has become more and more important, and suggest that we need to take a long view. I will do this by setting out a set of propositions about what has been the case. All of them are contestable, but I think my propositions will survive contestation.

1. Research is a human activity conducted with other people's money.

This is a truth often overlooked by researchers.

2. Publicly-funded research on any scale is a relatively recent activity.

There is a tendency on the part of academics to see the past in terms of the present, so that it is easy for some to slide into the belief that universities always

did research. They didn't. Serious research of any kind in universities is a 19th century (and largely a late 19th century) activity, and the scale of the endeavour at that time was tiny. For most developed nations scale becomes important only after about 1960 (1965 in Australia). For the University of Sydney, only five of a few hundred staff members in 1924, and seven in 1935, were active in research publication. How could it have been otherwise? Australia was cut off from the world of research until the arrival of the Boeing 707.

By the early 1990s university research had become a substantial proportion of Australia's whole endeavour, and there had emerged a class of 'research only' staff (15,500 of them in 1991). The cost of the whole endeavour inside universities was approaching \$2 billion.

3. Governments are now relatively short of discretionary money, and the shortage is growing.

The second world war was followed, counter to the prevailing expectation, by a long economic boom. That boom, coupled with steady but relatively inflation, produced 'fiscal increments' for governments, which were already used to planning and directing, a consequence of the war and the depression which had preceded it. A lot of new things were done, including the building of universities and the funding of research.

That boom came to an end in the early 1970s, largely because of the decision of the major oil-producing countries to greatly increase the price of oil. There followed two decades of 'stagflation' — high levels both of unemployment and inflation. These decades were not productive of further government initiatives; rather, governments tried to cut back on their expenditures and curtail their activities. 'Friedmanite', or 'rational', economics grew in importance at the expense of the 'Keynesian' orthodoxy which followed the war.

In the 1990s governments have learned how to manage (or at least have benefited from) a regime in which inflation is low and unemployment controlled, if still higher than the rates prevailing in the 1950s and 1960s. But the mood is one of individualism rather than the solidarity of the 1940s, and 'user-pays' practices have wide support. Increases in public revenue for public purposes do not. What is more, the ageing of our societies is producing pressure on the public purse for benefits and health care.

For all these reasons the public funding of research is not enjoying the kind of support that it received in the 1960s.

4. Governments are less in awe than they used to be of science, research, and those who carry out these activities.

Vannevar Bush, an adviser to President Truman, is generally credited with the public funding initiative institutionalised in the US National Science Foundation and its counterparts in Britain, Canada and Australia. The European countries were probably more affected by the example of the German Deutsche

Forschungs Gemeinschaft (DFG) which was founded in 1922. Bush's other legacy was the principle that scientists should be left to follow their own noses in conducting research. The linear model that he proposed (pure research leading to applications, development and commercialisation) found great favour with researchers. Governments, which knew little history (Pasteur and others moved into the 'pure' field as a result of the discoveries they made in trying to solve real contemporary problems) and less science, did what persuasive scientists advised them to do. Scientists, after all, had won the war, and Bush proposed that they should win the peace too. Professor Oliphant was able to persuade the Commonwealth Government to spend £500,000 on a single experimental machine, at a time when the entire budget of the CSIRO was around £3 million.

This rosy state of affairs did not last for long, for several reasons. One was that a great truth was learned quickly: that successful and unsuccessful research alike always lead to the need for more research. In short, the Bush principle led to the open cheque, and it was not long before budget advisers were calling out in alarm. Second, it is probably true that, defence aside, no single endeavour can sustain political attention indefinitely. Universities and research all had their runs, and in time lost the political and financial limelight. Third, the late 1960s were the last period when Presidents and Prime Ministers believed that all human problems were solvable given requisite amounts of money, will and knowledge. Today's governments are much more modest in their desire to accomplish. Fourth, science (particularly) and research were probably oversold (or, more gently, governments did not realise the long lead-times necessary for discoveries to be utilised).

Fifth, the emphasis on research in the universities was very often at the expense of teaching, in principle, in practice and in the attitude of academics. By the 1980s there were enough university-trained politicians and public servants who had not been impressed by their own experiences at university to have a powerful effect on policy formation. In the mid 1980s, as a senior member of the Australian Research Grants Committee, I went to see the Secretary of the Department of Finance, Mike Keating, whom I knew reasonably well, to find out why we weren't getting more money for research. I found out that he wondered why we got any: what was the public benefit? As Chairman of the ARGC I had to field a parliamentary question on the number of patents that had been issued as a consequence of ARGC grants. In more than twenty years of funding no patents had been issued and one had been applied for but not proceeded with.

This information did not help the cause, but more importantly, questions like this were not asked in the 1960s. For a high proportion of those in a position to shape policy in the field, academic life in the 1980s was easily seen as something of a wank, with academics avoiding teaching to indulge themselves in pursuing, with public money, questions of no interest to those who were providing the funds. Public comments by academic leaders often carried, whether or not this was intended, an attitude of implied superiority. It may be true that if we knew the answers we wouldn't need to do the research (so let us not have priorities), but the inference that academics knew best did not help the cause either.

5. A time of transition?

What I have been talking about provides the context for what has happened in the past decade. The binary system, which was dead anyway, has gone, but the Commonwealth provided very little extra money to equip the new universities, and the newer parts of old ones, with the infrastructure needed to carry out research in the style of the 1960s. There has been a substantial increase in the amount of research money allocated competitively, but not in the funds allocated in block form to universities. It is much harder to get funding to pursue one's own interests. It is, I am assured, much easier to find funding for research tasks which industry and government want done (and that seems to be the case for my own University). Academics work much harder than they did ten years ago, and there are fewer of them, while there are more students — a lot more. Teaching has a much higher status, and is taken much more seriously.

It was always a nonsense that every academic had to be, and in some sense must have been, at the cutting edge of his or her discipline. They weren't and aren't. While I am prepared to accept that virtually anyone could be a first-rate researcher, given adequate preparation, motivation and encouragement, in practice some people are a lot better than others. In the academic world we are not very discriminating about 'excellence', partly because it is hard to make judgments across disciplines, partly because disciplines (correctly, in my view) will not accept that they are situated in a hierarchy of disciplines, and partly because academic egos are at once large and fragile. After ten years of ARGC and ARC I was pretty confident that Australia had about 200 absolutely first-rate researchers in the fields covered by these organisations, and that from a performance perspective, giving them virtually all the money was as sensible an outcome as any. In fact, we funded (from memory) about 1500 out of the 2500 who applied. I proposed a radical shift like that in the mid 1980s, but it terrified some of my colleagues on the ARGC, so we didn't do it. When I reviewed the work of the tenured staff of the IAS at ANU I felt that there were about 30 who on their performance over the years could have held chairs in any university in the world; that too was a bit more than 10 per cent of the whole. In my own University there are 11 outstanding performers in 300 academic staff, all of them ostensibly engaged in research. That is less than half the IAS-ANU proportion at another time, but then UC was never funded for research.

I think we are in a time of transition, from a somewhat romantically remembered past to a very different future. Since I used four pointers to provide context, I'll suggest four important elements of a possible future.

First, I think we are moving towards research funding being split into two distinct domains. One of them is for the elite in pure research (I'll use this old term for convenience), and they will be funded to pursue their own sense of what the important questions are. But a lot of them will be pulled and pushed towards dealing with the important questions of today and tomorrow — what to do and how to do it best — because the next 25 years or so are likely to be 'challenging'. Our Government has made it plain that it wants the talents of the

best researchers to be applied to solving real rather than disciplinary problems. It does not believe that understanding necessarily comes first, and it is now confident enough to almost say so. The other domain, which will be very much larger, is what we used to call 'applied research'. There will be a third domain, which will be self-funded to some degree. People may be in all three domains at the same time, or move from one to the other. It will not be an offence to be an academic who is not currently engaged on research.

Second, for those who evaluate research, outcomes are going to struggle with outputs. Measuring papers, patents and the like will, I think, give way to some degree to an interest in measuring what happened as a result. To give an example (one I have used many times before) one of the major outcomes of the all the humanities and social science research carried out and published in the 1960s and 1970s — the heyday of that activity — was a much sharper sense of the Australian story, a growing national self-confidence, a clearer identity, a much lessened colonial cringe. This is an extraordinarily interesting story, but no-one has written about it comprehensively. It is time that someone did. It provides an answer, but not necessarily a compelling answer, to those who ask why we spend all that money on humanities and social science, when we could be spending it on medical research, or physics or engineering (supply your own hobby horse). We need lots of output studies of this kind.

Third, I think we will see a move towards researchers in some fields setting up their own enterprises, and taking intellectual property out of the university into the community. This has begun to occur, and expect the trend to increase. There seems to be more venture capital available than was the case in the 1980s, and more understanding of what is involved in backing R&D. Research is too important to be confined to the university anyway, and it is only academics who think that somehow the university is the natural home of serious inquiry. As I have argued already, 'research in universities' is a pretty recent phenomenon. What has happened is that over the last thirty years the universities have educated scores of thousands of people who understand what research is, have what I call a 'research attitude' and find it interesting and fun. Most of them are out in the community. This trend will accentuate the other two I have mentioned already.

Fourth and finally, these trends all point to a different kind of university. It will have less tenure, sharper divisions of labour, shorter appointment times, closer relations with industry and the community, and greater independence from government. The higher education system will exhibit greater diversity, a much larger and more varied basket of resources, strong links with the rest of the world (I think that today's universities play an extraordinarily important role in projecting Australian values and virtues to the rest of the world, a role that is hardly recognised). For those who remember the 1960s and 1970s as the Golden Age, the early 21st century university may seem dismal in comparison. But we will get the universities we need, just as we did thirty years ago.

