Comments on "Public Support for Science and Innovation: Productivity Commission Issues Paper"

Professor James Trevelyan

The University of Western Australia, Mechatronics Discipline Chair, School of Mechanical Engineering

I can offer comments on several aspects based on my experience in both industry and University research and development over the last 30 years.

I advocate the following:

1. Industry-based funding similar to the rural research trust funds to support long-term industry research projects. This has been demonstrably successful in the past.

2. The review team could use these comments (and an attached paper) to examine the emerging "Network R&D" being adopted by companies in the USA and Europe.

3. Increased levels of base funding to support new curiosity-driven research in universities because peer-review assessment depends on establishing a narrowly based track record before research can be supported.

4. A trial of multi-discipline priority-based assessment panels for each of the national priority research areas.

Impact of Research

The project which both established my reputation and absorbed my interest for 15 years was the development of robots for shearing sheep. So far no robot has been used for commercial shearing yet the project has been an outstanding success for the wool industry. It took some time for me to realise why this project was such an outstanding success even though there has been no commercialisation of the technology.

The success was strategic. By demonstrating the availability of a viable alternative to manual shearing, wool growers have enjoyed the luxury of a strengthened bargaining position with respect to shearing costs. Our research required an investment of approximately $10 million over 15 years. Given that shearing costs average typically $500 million per year, the research cost was about 0.13% of shearing costs. Industry sources were confident in the late 1980s that the existence of an alternative technology strengthened their negotiating position lowering shearing costs by 2% or more, giving a return of about 15 times the original cost of the research. So far there has been no repetition of the strikes that paralysed the shearing industry in the early 1980s and the existence of alternative shearing methods also contributes to a calmer industrial environment.

This project also established Australia's reputation as a leader in advanced robotics and this has inspired a new generation of researchers such as Prof. Hugh Durrant-Whyte who leads the Australian Centre for Field Robotics at Sydney University and Prof. Alex Zelinsky who leads CSIRO research in robotics and automation. Their research is leading towards advances such as automation of underground mining, cargo handling in our ports, automotive collision avoidance, and driver fatigue monitoring.

The robot sheep shearing project was only possible because of a long-term commitment to the future viability of the wool industry made possible by the Wool Research Trust Fund. The wool industry was anxious to maintain its profitability 20 - 50 years down the track. They encouraged both basic and applied research with long-term projects made possible by guaranteed long-term funding. The international fine wool revolution of the 1980s and 1990s was made possible largely by Australian research and development.

The wool industry crisis of the early 1990s led to the collapse of this long-term research programme. This program has only recently been restarted but the memory of that early research has been lost. As a result money is now being wasted repeating research performed in the 1960s and 1970s, for example on chain shearing.

It will take some time to rebuild the research base that supported the wool industry success of the 1980s. What is needed is not just funding but a long-term vision of the future.

Industries often run into problems that require short-term answers. An example in the wool industry was the failure of transfer dye printing onto wool fabrics. This process had revolutionised the production of small quantities of designer fabrics used in high-fashion circles. The process worked on cotton and synthetic materials but not wool. CSIRO scientists managed to solve the problem quickly but only because they had been working on wool textile dye problems for a generation before that problem arose.

What has happened since?

The research capability of the wool industry was decimated by the economic crisis of the early 1990s. Other industries went through a more voluntary process, dismantling their corporate research and development laboratories deliberately. Telstra is a good example. BHP and other companies followed suit. They dispersed their research assets and try to align them more closely with short-term business priorities. The result was a loss of focus. Admittedly it was difficult to maintain research productivity in laboratories with a focus on long-term research. It was also difficult to align scientists in these laboratories with long-term business interests without unnecessarily constraining their creativity and curiosity that stimulates new research.

In a recent paper, Alice Lam (2005) traces this evolution in corporate research strategy (copy attached). In Europe and the USA large organisations are pinning their hopes on what is called "Network R&D". This strategy relies on maintaining a large and mostly informal network of small research and development suppliers including universities and small independent research companies. The difficulty is one of maintaining both a long-term focus to guarantee organisational success in the future while at the same time meeting short-term business priorities.

Intellectual Property and Innovation

Commercial entities have become much more aware of intellectual property issues. However, only explicit written knowledge can be protected effectively by intellectual property instruments. Recently I started a project aimed at understanding engineering work and innovation in great detail. Part of this requires a detailed understanding of what technical knowledge actually is and how it is used. One of the interesting results of this research is the provisional finding that much (if not most) of the technical knowledge used in day-to-day engineering practice is implicit knowledge: it is unwritten and/or tacit knowledge. Engineers and technicians are seldom consciously aware of the knowledge that they use.

At the conclusion of the robot sheep shearing project, the Australian Wool Corporation insisted on the removal and storage of all documents, records, computer disks, photographs and papers associated with the project in order to protect their intellectual property. What they removed was the knowledge of how we would not build a sheep shearing robot again. We knew in our heads how to make the necessary improvements for a future generation of robots. That remains with us to this day.

Our research is throwing new light on the problems of technical knowledge transfer and acquisition in the innovation process. Many researchers in the 1980s and 1990s have attempted to explain the relative success of Japanese companies in developing new electronic technologies. Organisational differences between Japan, USA, Europe and Britain have been studied extensively by management researchers. By studying the work of individual engineers and technicians we can now understand and explain these results in terms of the unwritten and tacit knowledge on which all engineering depends. We can now begin to define this knowledge and how it is used. We are confident that in the long term this will lead to big improvements in engineering education and training at all levels of skill. It will help to avoid recurrences of the current "skills shortages" in Australian industries.

It has been possible for us to make this advance because of a fortunate coincidence of engineering technical knowledge and experience coupled with the ability to use recently developed research methods developed for the social sciences, particularly anthropology.

Missed Opportunities in Business Decision-Making

Our recent research on engineering maintenance and asset management provides a useful example of how business can easily overlook major opportunities for performance improvement. It is surprising to learn that typical process industries in industrialised countries typically lose 30% of turnover because of efficiency losses and operator and maintenance mistakes. When the failures are large ones there is a brief focus of attention on this issue. For example, last year Woodside Energy had several large failures that cost several hundred million dollars in lost production. At the moment there is a focus on maintenance systems in the company but great frustration at the engineering level because they have found it very difficult to change the way that maintenance work is done. This problem reflects several failures. There is a failure in accounting practices: we have found fundamental missing elements in accounting education at the most basic level. There is a failure in engineering practice: maintenance is considered a low status occupation among engineers so good engineers avoid maintenance work and even bad ones move on quickly. Maintenance simply receives very little serious attention. Computerised maintenance management systems have been implemented in most large enterprises in the last decade but these have only compounded the problems that were evident earlier. Computer databases cannot represent unwritten and tacit knowledge that is a vital component of effective engineering. Interestingly there are many engineering research projects looking at design, but almost none with a serious focus on maintenance work. Only one systematic account of actual engineering maintenance work seems to have been published in the last 40 years.

The high-profile engineering failures such as the Longford gas explosion and BHP's $3 billion hot briquetted iron plant write-off at Port Hedland represent just the tip of the iceberg. In any company or engineering organisation in Australia you will find similar failures at all levels.

This problem is not unique to Australia: other countries are experiencing similar problems. In developing countries the situation is far worse as evidenced by the astonishingly high end-user costs of essential commodities such as drinking water and electricity. We have recently calculated that end-users costs of drinking water are up to 25 times higher in Pakistan than in Australia (in equal currencies). A similar situation exists in India and many other developing countries.

It is worth asking why, in an age of unprecedented education and communication, such failures can continue to occur.

I would submit that we have not yet mastered the link between human behaviour and successful engineering. This is not at all surprising given that we rely almost entirely on a cadre of technical specialists for industry research and development. These technical specialists have only an informal understanding of human behaviour and are trained to work entirely with explicit knowledge even though they depend on unwritten and tacit knowledge for the success of their research. There are almost no engineering or science academics or researchers in Australia (or other industrialised countries) with the necessary research techniques to understand human behaviour. Likewise, management and social science researchers seldom have the technical background needed to understand engineering and technical work. We have little if any research and development capability that can link human behaviour and technology.

While this problem exists in every industrialised country, there is an opportunity for Australia to make an investment in research capacity that could give us a substantial advantage in decades to come.

It is difficult if not impossible to obtain significant funding for this kind of multi-disciplinary research in Australia. It will only be possible with a change to the present discipline-based system for allocating research funding.

Removing Barriers to Change

For several years we have applied for ARC research funding to support our cross-disciplinary research on engineering work. All these attempts have been unsuccessful and it is worth understanding the reasons for this.

The first attempt involved a collaborative application between myself and a colleague in the Graduate School of Management. I have an extensive track record in engineering research and he has an extensive track record in organisational behaviour. However, our research application was referred to the management panel of the ARC who discounted my track record as having no relevance to management. My colleague's track record was also attacked on the basis that he had no engineering expertise. The application was misunderstood perhaps because the reviewers had little understanding of technical issues and because we were equally inexperienced in writing a cross disciplinary research application. My colleague decided that we would be safer if he were not part of the application so it would be reviewed by an engineering panel instead. We followed his advice. Unfortunately it has not been possible for the engineering panel to obtain any reviews because engineering academics do not have the research background needed to understand the methods that are required to this research. Also, it is not possible to demonstrate a track record in a new research area that does not yet exist for engineering. (We thoroughly searched the literature and found only a tiny handful of earlier reports of similar research.)

I am fortunate that I have access to independent research funding and strong support from my colleagues and the University. Otherwise it would not have been possible to start this research which has now attracted three academic colleagues and about 20 research students.

We have been successful in obtaining some funds from a CRC recently established to investigate engineering asset management. However, while there was strong support from the commercial partners who seem to appreciate the value of this research, it was not possible for a large group of engineering academics who run the centre to understand this.

An experienced colleague once told me that under the ARC system one has to apply for funding on the basis of research that has already been completed. Only then does one have the publication record and detailed arguments to explain why the research was necessary in the first place and to establish the required track record needed to win a grant. Given that the discipline-based assessor panel relies on the detailed knowledge of specialised reviewers to assess individual research grants, it is difficult to see any easy way of overcoming this fundamental difficulty in peer review processes.

It is also worth noting that the same difficulty applies equally to ARC Linkage grants. Of course, the support of companies and business organisations is essential. However, the proposal has to withstand peer-review and because of the novelty of our research it has not been possible for assessor panel is to obtain any appropriate reviewers.

Therefore, we have had to approach companies to cover the full cost of research without being able to offer them the same leverage that is available through Linkage and CRC funding mechanisms. This has not been easy.

There are two possible ways to overcome this barrier.

One is to increase the base level of research funding provided to institutions, recognising that we need to encourage a modest level of curiosity-driven research to allow new ideas to develop to the stage where larger peer-review research funding mechanisms can work to provide further support.

Another is to recognize that discipline-based assessment of research will lead only to discipline specific funding. Given that national research priorities have been established, why not attempt “problem-based assessment” of research? Each major priority area for research could be assessed by a panel representing a cross section of research disciplines (drawn from humanities, social and behavioural sciences, engineering, management, biological sciences, medicine and physical sciences). This might lead to more integrated research programmes to tackle national priority areas, and would provide the opportunity to create linkages between complementary research teams that might not otherwise be aware of each others’ interests. This would also help to break down the kind of difficulties facing cross-discipline research applications that I have described above. Also it would be easier to discriminate between applications that cite unfounded reasons for their relevance to a national research priority area.

For example, I am currently reviewing an ARC application that attempts to justify a kind of robotics research by claiming that it contributes to “Frontier Technologies for Building and Transforming Australian Industries”. Little credible evidence is presented to support this claim. However, specialized robotics researchers that would review this application under the current discipline-specific regime are unlikely to have the background necessary to assess the evidence presented to this assertion. A broadly based priorty-focused panel would be more likely to see through these claims.