Policy note

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GROUP OF EIGHT

Where should governments invest their research funding?

Introduction

With an uncertain economic outlook and very tight budgetary conditions, it is not surprising that governments are looking closely at all areas of their spending: they need to know that the return on their investments makes the expenditure worthwhile and that the processes they use to target and manage their funding activities are both efficient and effective. Government funding of research is as subject to this increased scrutiny and accountability as are all other areas of spending.

At a macro level it is easy to justify government support for research. Numerous studies, whether at national, program, institutional or even project level, have clearly shown that research can provide returns significantly in excess of the cost of the research. This is not surprising. Anyone observing the world in which they live can see how the outputs of research improve our life, work and environment. From health to entertainment, from communications to travel, from agriculture and mining to the manufacturing and service industries – all depend to an increasing extent on technologies that are constantly improving and which have their origin in the enhanced understanding and knowledge that derive from the global research effort. Moreover, government requires research to support and improve the efficiency of its own activities and to help meet its own responsibilities, from policy development and evaluation to national security.

Will a more directed selection of the research that government supports provide an even higher return on government investment?

At this macro level of analysis, the easiest way to increase the return on the investment in research is to streamline and improve the efficiency of the processes used to provide, manage and evaluate the government funding. There is plenty of scope to do this and current debate around this issue is identifying and evaluating the many options that exist.

At a micro level the situation is more complex. Not all research projects are successful; only some research produces impact in the short-term; research performed in Australia may benefit companies located overseas; and some research may have no apparent or direct link to improving economic performance, health, lifestyle, social sustainability, environmental management or government decisions. This inevitably raises the question as to whether a more directed selection of the research that government supports can provide an even higher return on the government investment than it already achieves.

The question is an important one and it is necessary to pay it serious attention.

Not all research is the same

"Research" encompasses a very wide range of activities. The Organisation for Economic Co-operation and Development (OECD) defines a number of different types of research that flow from basic (fundamental, curiosity- or investigator-led) research to experimental development. While the divisions between the different categories are not sharp and can sometimes depend on the intention of the researchers (or of the parties funding them) as much as on the nature of the research process, it is important to recognise the range of different approaches that can exist. It is also necessary to appreciate that different parts of the innovation system focus on different parts of this research spectrum; and that some kinds of research are more worthy of government support than others.

Business funding and performance of research tends to concentrate on the experimental development end of the spectrum. Research in this sense can be very specific in terms of its intended outcomes – for example improving a particular piece of machinery, perhaps by using a newly invented material, so that it will perform faster and more accurately. The benefits of such research largely flow to the company performing the research and its customers and shareholders.

Applied research – such as trying to develop a material having particular characteristics (for example specified combinations of strength, density, heat resistance, or elasticity) – may have wider benefits. This is because the successful production of such a material may find many applications – including uses that those performing the research did not have in their mind. As with experimental development, the outcomes of this research are often not very surprising and the intent of the research is to exploit opportunities, technical or commercial, that already exist.

Basic research (usually performed in universities and funded by government) is more about inspiration and aspiration than specification. Such research might involve studies of atomic and molecular structure that provide a better understanding of what underlies the macroscopic, physical characteristics of materials. The knowledge that flows from such research can provide the basis for designing a whole range of new materials having applications across many industries and firms. As well as having the potential to create the possibility of developing new industries building on the characteristics of materials that at present we cannot even imagine, such basic research might lead to knowledge having applications in quite different areas – such as pharmaceuticals, soil science or electronics. (This is why another term for basic research is "research not yet applied".)

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One characteristic of the development and applied research performed in business is that it operates within boundaries set by the needs of the business. These include the technical, financial, managerial, market and other capabilities of the business that may limit its ability to use the research. Business research also aims to produce commercially useful results as soon as possible; it does not seek the dramatic breakthrough or the revolution in understanding that creates completely new opportunities, because these would be beyond the capacity of the business to use. Neither does business research encourage (or even allow) the exploration of new ideas and opportunities that arise during the course of the research – the research management process aims at a predetermined endpoint defined by an existing market opportunity and supported by a detailed business case. Research management processes within a business focus on this end point and will normally adopt a fast failure approach to stop or redirect any stream of research which for one reason or another will not contribute to this agreed destination. However, business does draw upon the fundamental research performed by universities and the understanding that this creates. This is shown by the extent to which patent applications for business inventions explicitly draw upon the publications through which university researchers made their findings available.

The different forms of research complement each other, operate on different timeframes and are subject to different degrees of market failure and potential spill-over effects. Investment in basic research is equivalent to long-term, patient, capital investment that creates new infrastructure and will provide a sustained flow of opportunities well into the future, many of which we cannot yet envisage. Moreover, while the intended users of experimental development and even applied research are usually known to the researchers performing such research (and are small in number), the eventual users of the knowledge and understanding that result from basic research are many and unknowable in advance. While it is true that the narrative of cause and effect from basic research outputs to the changes they make in the world can be complex and perhaps not very transparent (in part because of the time lags involved and the requirement for advances in other areas of research to unleash the full range of potential impacts), we should not discount it for this reason.

As already emphasised, the different forms of research also take place in different sectors and use different research management techniques. Business accounts for most of the national effort in experimental development and performs hardly any basic research. Universities account for most of the nation's basic research. Cultural differences between the business, government and university sectors are important in creating the appropriate environment for these different types of research, so that it is not easy to transplant the research intent of one sector to another.

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Problems in investing for impact

As there are always fewer resources than needed to support all research opportunities, government might want to argue that the funding it provides should support those research activities that have a definite (and short-term) end in mind. Support for business research already does this and, using mechanisms such as taxation incentives, removes the role of government in determining business research priorities. Setting priorities is easier for public sector research and the competitive grant schemes provide one means of doing this. However, setting priorities for research that will lead to particular outcomes, especially when the research is longer term, can present problems.

Optimistic forecasts

One problem is that, by definition, research is the process of discovering something we do not already know. The more definite we can be about the research outcome when we start the research, the more trivial the research and the weaker the arguments for government support. Moreover, it is not the role of government to fund or perform research that business needs for itself and which does not involve a significant risk. University research should complement, not duplicate or supplement business research. There is even a danger that if universities move too far along the applied research and development spectrum they will crowd out business investment in research, making Australia's international ranking in business performance even worse than it is already. (An exception here of course is where business is paying the university to perform the research.)

The inherent uncertainty of research means that although everyone might be clear about the impact the research should accomplish and even be confident about achieving it, the research might not be successful, even in the sense that it might not produce the technical output it is seeking to realise.[1] We celebrate Isaac Newton for his basic research exploring the laws of motion or his studies of light or for the development of calculus, not for his applied endeavours in trying to turn lead into gold. (And it is the results of subsequent studies in basic science that have shown us why his work in this area was bound to fail.)

1 Business research might also fail in commercial or market terms, even if it is successful technically.

In the 1950s researchers were promising that within 20 years nuclear fusion would be producing electricity that would be too cheap to meter. After billions of dollars of investment and more than 50 years later, the commercial production of fusion energy is now 30 to 50 years away and nobody is suggesting it will be free. In 1971 Richard Nixon, the then President of the United States, started a 'war on cancer' but the biggest returns on the vast number of dollars spent on cancer research have come from fundamental research exploring basic biological, genetic and biochemical processes. Similarly, research on carbon capture and storage has consumed huge amounts of money and has a very clear aim but commercial outcomes are still somewhere in the future; and any examination of forecasts of the potential impacts of genetic engineering made in the early 1980s will show that while this research has produced many benefits, it has still to realise many of the applied outcomes seen as imminent even 30 years ago.

Researchers are often over-optimistic about what and when their research or particular pathways of research can produce. This does not mean we should ignore such predictions but it does mean that it makes sense to continue to invest in other areas of research that apparently have less or uncertain promise, which aim at similar objectives while using different approaches, or which aim at other outcomes. The development of lasers has probably had as much beneficial impact on eye surgery as has medical research directly addressing eyesight problems (and lasers were originally seen as a solution in search of a problem). Moreover, as research progresses it identifies issues not at first apparent, so that achieving any particular impact will normally require contributions from research across many disciplines. Focussing research effort on narrow areas of activity can be counterproductive if it leads to the loss of capabilities later found to be necessary.

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Pessimistic forecasts

In the same way that researchers can be overly optimistic, identifying research outcomes and impacts beyond those that they can achieve, the reverse situation is also common. This is when the most senior and respected scientists (often those controlling the purse strings) argue that an intended outcome is not possible. Interesting examples (from the many possible) include:

> *Radio has no future. Heavier-than-air flying machines are impossible. X-rays will prove to be a hoax.* (Lord Kelvin 1899)
>
> *There is not the slightest indication that [nuclear energy] will ever be obtainable. It would mean that the atom would have to be shattered at will.* (Albert Einstein 1932)
>
> *Anyone who expects a source of power from the transformation of [the nuclei of] atoms is talking moonshine.* (Lord Rutherford 1933)
>
> *Space travel is utter bilge.* (the Astronomer Royal 1965)
>
> *The abdomen, the chest and the brain will forever be shut from the intrusion of the wise and humane surgeon.* (Surgeon Extraordinary to Queen Victoria 1873)
>
> *I think there is a world market for maybe five computers.* (IBM Chairman & CEO Thomas Watson 1943)

Focussing research on areas experts claim to be those most likely to produce significant 'real world' impacts and ignoring those even experts claim to be of no value or potential would mean missing out on important and pervasive benefits that in at least some cases have the power to transform our lives and livelihood. Following particular or popular pathways to achieve an impact might miss the most practical, if unexpected, way to achieve the desired outcome.

Unforseen outcomes

Apart from over optimism and pessimism and perhaps even more important, is that some of the most significant impacts of research were not the result of planning and even the most respected scientists found them surprising. They appeared to come from nowhere, from research that was not mainstream or which was even antagonistic to mainstream views. Fleming's accidental discovery of penicillin is one kind of example, illustrating the importance of serendipity. A different kind of Australian example is the discovery of *Helicobacter pylori* as the cause of gastro-duodenal ulcers. At the time Robin Warren and Barry Marshall performed their research, most informed researchers and medical practitioners were totally antagonistic to this explanation. More generally, scientific progress and the technology that flow from this are the result of debate and challenge, not the development of a comfortable consensus. An effective science funding system has to provide support for the maverick, as well as for those going along with the herd.

Radioastronomy exemplifies a different way in which capabilities developed through one area of research can have profound implications for society as a whole. The study of quasars, pulsars and the cosmic microwave background provides information about the origin of the universe, the big bang and cosmological developments over the past 13.8 billion years but in itself has no practical use, exciting as it is intellectually. However, the capabilities developed through this esoteric research have enabled Australia to develop a world standard microwave aircraft landing system (Interscan) and the Wi-Lan technology now found in literally millions of phones and computers.

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A more general demonstration of the difficulty of predicting (and therefore planning) practical outcomes from research comes from the many technology foresighting exercises which, whether carried out by governments or other bodies, always miss some of the most important developments that take place over the future periods they consider. A 1937 report of the US National Resources Committee entitled *Technological Trends and National Policy*, forecasting technological developments over the next 25 years, missed not just nuclear technologies and jet engines but antibiotics, DNA and most of the important biological and medical discoveries over the period they were examining. Had the report led to the funding of only that research deemed likely to have impact, the world we live in would be a very different place.

It is an important and profound truth that the people who performed the research that led to the knowledge and understanding that underpin much modern technology could never have even imagined how their findings would have impact. Early experimenters on electricity, atomic structure and magnetism were not trying to produce the lasers, computers, smart phones and countless other devices that their findings made possible. Researchers interested in the magnetic properties of atomic nuclei were not trying to develop medical imaging equipment – but MRI scanners depend on the work they performed. Mathematicians studying prime numbers were not trying to support safe internet banking, but their work has done just that. The route between basic research and impact is often long, complex and surprising. It is not easy to predict or to manage but it is fundamental to technological and economic progress.[2] Moreover, the impacts that research achieves depend in most cases on the actions of people from outside the research system and such people often envisage potential that the researchers themselves had not considered, recognised or planned.

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2 In the USA, Representative Jim Cooper (D-Tenn) and two of his congressional colleagues have worked with other institutions to establish the Golden Goose Awards to honor federally funded research: "whose work may once have been viewed as unusual, odd, or obscure, but has produced important discoveries benefiting society in significant ways." See: http://www.goldengooseaward.org/

Problem identification

Undirected research seeking knowledge rather than impact can have many unexpected outcomes and lead to major advances in technology and wellbeing. Such research is also important in identifying problems of which we might not otherwise be aware. One obvious example is the work that has identified the extent of climate change, its possible consequences and possible causes. Another example is the identification of the hole in the ozone layer which, had scientists not identified the problem and possible ways of ameliorating it, would have caused significant increases in skin cancers and disruptions to natural ecosystems. Similarly, work in environmental science that has identified the importance of natural processes and the significant value they add through water purification, pollination and other environmental services has led to improved decision making at all levels. Better decisions have led to the saving of billions of dollars that would otherwise need to have been spent to replace the environmental services that would have been lost.

Selection of disciplines

In seeking to emphasise the short term and non-academic benefits of research, some commentators suggest that certain disciplines are more useful than others. In particular, there is a view that research in science, technology, engineering and mathematics is more likely to improve national wellbeing than research into the humanities, arts or social sciences. This is not the case. Indeed, it was G H Hardy, the British mathematician, who initiated the Mathematical Society of England's toast "Pure mathematics; may it never be of use to any man", a forlorn cry given the many practical applications that draw upon the findings of once 'pure' mathematics.

Research is making it increasingly apparent that achieving non-academic impact is not just a technical feat but very much a social one. Innovation takes place through people so that understanding the factors that encourage particular behaviours and social practices is essential to successful innovation. Gaining the potential economic benefits of genetic engineering or nanotechnology requires support from the social and cultural sciences as it has to build on an understanding of the concerns of the public and specific communities about the use of new technologies and building appropriate responses to these.

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Work in the humanities, arts and social sciences has many applications not least in supporting the development of government policies, understanding issues such as land rights and mining, or in comprehending research and the pathways through which it contributes to national welfare. Much social science can have quicker, more direct and more profound impacts on society than can a great deal of the work in the biological and physical sciences, in part because it is more accessible to the public and to policy advisers and government decision makers and easier to understand. It is worth noting that forty three per cent of the companies founded by Massachusetts Institute of Technology (MIT) graduates in the 15 years before 1997 were established by graduates in the social sciences and humanities.[3]

More directly, an improved understanding of global and regional history, politics, culture and social structures underlies much government activity and policy development. Research in the social sciences and humanities is necessary to understand and develop sensible and acceptable responses to current social and economic trends, to develop better business practices and environments and to support national security. Responding to today's uncertain and fragile world requires support from linguistics, comparative theology, the study of different cultures, an understanding of the factors that impact on people's beliefs, attitudes and behaviours, and a deep knowledge of ethological, political and theological issues in different countries and cultures. Understanding the causes of conflict and how to address them is central to 'capturing hearts and minds' and critical to the gathering and analysis of high quality intelligence, just as the findings of anthropological

3 http://web.mit.edu/newsoffice/founders/Founders2.pdf

research can feed into business practice or help understand organisational structures and dynamics.

Research in the creative arts can similarly have direct economic benefits. While the development of platform technologies is a necessary precondition for the development of the multibillion dollar gaming industry, its economic success depends on the narrative structures, the characterisation and the visual and sound impact of the content of the games, and their ability to draw in players by providing a holistic and increasingly personal experience. Research in the arts can contribute to this as to many other areas of economic endeavour. One interesting example is the way theatre directors now help design robots to help make them respond in appropriate ways to the people working with them.

Research produces more than publications and commercial outputs

In trying to measure the effectiveness and impact of investments in research, especially basic research, it is easy to focus on peer reviewed publications, patents and commercialisation activity as the substantive outputs of the research. However, research is a process and it is cumulative, building on the knowledge and skills sets that already exist and further developing them. This is one reason why research in universities is critical in exposing the very best students to current learning and in creating an awareness of the need to challenge and test current views and knowledge.

One of the most important outcomes of public sector research investment is the development of a set of capabilities that the nation can draw on as necessary. These capabilities include expert knowledge, the ability to use specialised facilities, and the domestic and international networks that provide access to overseas expertise and facilities. Basic research provides a window on overseas research and an ability to identify and provide advanced warning of technological and other trends across many fields that might have impact on Australia, its industries and society. This broad capability provides a kind of national technological insurance, a safeguard providing the assurance that we have access to any expertise that we need. Moreover, excellence in basic research provides a necessary precondition for developing links with overseas researchers. Such links not only facilitate the exchange of information and ideas, they also form an intangible component of our national capability that we can draw upon in time of need.

Just as important is having first-rate researchers publishing excellent, exciting and breakthrough research (as assessed by their peers) in the international literature. Adding to Australia's international reputation, this provides the credibility that derives from contributing to the international understanding of global problems and enables Australia to take a place at the international table. The resulting international research collaborations can then provide a secure foundation for establishing and strengthening other relationships and activities.

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Despite its relatively small population and geographical isolation, Australia's contribution to global research enables the country to play a respected role in discussions relating to matters as varied as climate change, biosecurity, food and drug regulation, the global economic crisis, the management of marine resources, pandemics and many more. This is not a trivial benefit of research, even though most impact measures are unable to capture it.

Conclusion

Any nation needs a portfolio of research programs and activities that span the full range of the research spectrum and which complement each other. Different parts of the innovation system will specialise in different kinds of research, depending on their particular roles, needs, responsibilities and capabilities. The government's investment in research (and the mechanisms it uses to make them) need to reflect and respect these differences and acknowledge the role of business in planning, performing and funding the more tactical, short term research intended to realise already identified market and other opportunities.

Government funding of research has three main purposes. One is to meet the government's own need for research to support the activities for which it is responsible (such as social and national security, health services, education, and national infrastructure). Some of this may be short-term, applied and even developmental research funded through individual government portfolios. The second purpose of government funding is to redress the market failures which tend to result in an underinvestment in research such that the national research effort is below that which would be optimal in promoting national wellbeing – including, but not exclusively, economic growth. Such market failures exist especially in relation to the long-term basic research that is strategic rather than tactical in intent and which has the potential to generate completely new prospects beyond the immediate horizon. The third reason for the government supporting research is to ensure a vibrant and effective education system able to train new researchers and ensure that graduates moving into any part of the national workforce have had exposure to leading edge knowledge and a learning experience that promotes critical thinking and innovation.

To meet these objectives the government should support high quality and original research that sustains and develops genuine talent, creativity and engagement, which matters, and which will receive international recognition as the best in its class. This means backing the most gifted and brilliant people we have so that they can follow the most inspired research paths they can establish; and ensuring universities have the internal resources necessary to support non-mainstream researchers whose work might eventually overturn existing paradigms.

If Australia is to capture the full benefits of the creative, original and imaginative efforts of its researchers, it will always need a means to support the ideas and challenges coming from individuals and small groups, even when these ideas fall outside formal priority setting mechanisms. Top-down approaches to the setting of research strategies are of considerable value and work well in certain parts of the research system because it is possible to link them to research management processes that focus on a predefined outcome. However, universities and the broader role they play within the innovation system need support for bottom-up approaches that allow tapping into the unfettered genius of our most creative people – those who can move from the immediate confines of the present and will go on to create the future.

Selecting projects for funding according to the excellence of the research proposal and the track record of the researcher is just as much a priority setting process as is allocating funding on the basis of the intended outcomes of the research – and should in any case underlie these other kinds of priority setting processes. There are many studies that demonstrate quite explicitly that research judged as excellent through peer review and by the use of citation measures is the research that is most likely to produce significant benefits beyond the research system, even though the researchers concerned did not set out to achieve these benefits directly.

At the same time, most researchers are not born excellent and in some disciplines excellence can take time to develop. For this reason universities need the internal capacity to support researchers with potential but who might not yet be competitive; and to support research which, while not mainstream and even unconventional, might have the capacity to overturn existing paradigms. Today's eccentric can become tomorrow's Nobel Prize winner.