Submission to the

Senate Economics References Committee
on
Australia's Innovation System

by

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Introduction

On 18 March 2014, the Senate referred the following matter to the Senate Economics References Committee for inquiry and report by the first sitting day of July 2015:

The challenges to Australian industries and jobs posed by increasing global competition in innovation, science, engineering, research and education, with particular reference to:

(a) the need to attract new investment in innovation to secure high skill, high wage jobs and industries in Australia, as well as the role of public policy in nurturing a culture of innovation and a healthy innovation ecosystem;

(b) the Australian Government's approach to innovation, especially with respect to the funding of education and research, the allocation of investment in industries, and the maintenance of capabilities across the economy;

(c) the importance of translating research output into social and economic benefits for Australians, and mechanisms by which it can be promoted;

(d) the relationship between advanced manufacturing and a dynamic innovation culture;

(e) current policies, funding and procedures of Australia's publicly-funded research agencies, universities, and other actors in the innovation system;

(f) potential governance and funding models for Australia's research infrastructure and agencies, and policy options to diversify science and research financing;

(g) the effectiveness of mechanisms within Australian universities and industry for developing research pathways, particularly in regards to early and mid-career researchers;

(h) policy actions to attract, train and retain a healthy research and innovation workforce;

(i) policy actions to ensure strategic international engagement in science, research and innovation; and

(j) policy options to create a seamless innovation pipeline, including support for emerging industries, with a view to identifying key areas of future competitive advantage.

In this submission, we will focus most of our attention on points b) and e), which relate to the mechanisms used to fund Australian scientific research. However, in the process of commenting on the Australian funding research system, we also cut across many of the other themes mentioned in the terms of reference.

We focus on the research funding issue for two reasons: first, we believe that scientific research is a fundamental building block for the Australian innovation system; and second, we believe that there is much that can be done to improve the current funding system. It's as important to think about *how* the research funds are allocated as it is to think about *how much* research funds are allocated. Part of the problem is that there hasn't been a systematic and rigorous appraisal of the mechanisms which characterise the Australian research funding system, so much of what we says rests on deductive logic and the small number of empirical studies that have been undertaken here. The 'science of science and innovation policy' is, however, an increasingly-important area of inquiry internationally and it would be desirable if a similar community of practice emerged in Australia. One way to stimulate this might be to provide greater access to unit-record data on funding applications and outcomes in Australia which could then be linked to researchers' outputs including graduate students, publications, patents, and the like.

Background

In recent years, there has been a barrage of criticism of the Australian research funding system including a number of serious attempts to try and reform it (e.g. the McKeon Strategic Review of Health and Medical Research in 2011). Despite all of this energy, it hasn't really lead to much meaningful change in the system: researchers are still frustrated at the inordinate amount of their time is taken up with preparing and reporting on competitive grants. Anecdotally, the majority of researchers see the system as arcane, overly-bureaucratic and wildly inefficient. And these concerns are being voiced by our leading researchers who have been successful in the existing system; they are not the sour grapes of those who have found it hard to win competitive grants. "Broken" might be too strong a description, but there seems little doubt that the system is ailing.

This submission provides some reflections on the system and some thoughts about how we might move forward. Much of this has been said before, but hopefully seeing it through a slightly different lens will add some value to the debate. It should also be noted that these reflections overlap: they are not discrete, mutually exclusive comments. Some are uniquely Australian issues while others are common to the funding schemes in most developed countries.

Let us start with the following position statement: if we are serious about improving Australian productivity – and this seems to be an issue which has bipartisan political support – we should recognise the enormous *potential* of our scientific research base and place it at the core of productivity policy. We say potential, because hitherto, Australian industry (both the for-profit and not for-profit sectors) has made poor use of our world-class research sector. Leading industrial economies, such as the US, Germany, Japan and more recently the UK, have extensive well-funded programs to promote the translation of new discoveries from science into industry and to engage the research community with real world problems.

Being serious about using research to promote productivity growth means we have to take a long, hard look at the way in which we fund scholarly research *and* engagement (by which we broadly mean the translation, extension and diffusion of new scientific knowledge). As a nation, we have done well when it comes to translating agricultural scientific research into farming practice, but we need to do more to embrace the importance of translation in all research domains. Of course, translation is more important in some domains than others – and some researchers are interested in basic rather than applied research – but this shouldn't detract from the importance of translation.

Specific Issues

In the following, we look at some of the biggest problems with the Australian research funding system. In doing so, we focus on improving the way we currently spend our research dollars, rather than whether or not we should increase the amount we spend on research. Pleading for the latter without understanding more about the former is misguided. Columbia University's Professor Ray Fisman recently stated that: "The way we spend those dollars will be *at least as important* as how much we spend" (Slate, January 10th 2010, emphasis added). We couldn't agree more.

Getting the incentives right. Innovative research is arduous, risky and long-term. It almost never occurs as a simple progression from an idea to a proven, published result in 5 years. But the

incentives to do genuinely innovative (and therefore risky) research just aren't in the system. Instead we reward short-termism and incrementalism. Although the rules encourage "novel" research, it also has to be "achievable". The well-known game here is to have the research half done when you are writing an application, so that you can achieve these incongruous goals. But this is clearly the tail wagging the dog. Surely, we should be aiming to design a system in which the best ideas rise to the top, even if they are not yet proven to be correct (i.e. they really are ***ideas***). Of course, in a perfectly functioning 'market for ideas', good ideas lead to top publications. But this is not a perfectly competitive market: it is riven with uncertainty and imperfect information. Our system needs to recognise and provide incentives for researchers to do the best possible work they are capable of.

In a provocative recent paper, Azoulay, Graff Zivin and Manso (2011) showed that incentives in grant systems matter. They compared outcomes from two very different funding schemes in the U.S. – the NIH and the Howard Hughes Medical Institute – and showed that the type of outputs vary markedly. The NIH requires long grant applications, is focused on the applicant's CV and wants to reward projects that are highly likely to succeed. On the other hand, the Howard Hughes Medical Institute embraces failure, focuses on the person not the project and provides the researcher freedom to choose how to allocate their time and effort. The paper showed that the Howard Hughes Medical Institute investigators produced high-impact articles at a much higher rate than a control group of similarly accomplished NIH-funded scientists. This indicates that incentives matter – long–term funding arrangements are more likely to produce big ideas which have a longer-lasting effect on the profession. In Australia, we spend about $9 billion on research but have not even attempted to investigate the effects that incentives play in researchers' decisions about what they will work on. Much of the $9 billion could be spent inefficiently (we simply don't know). A better way to allocate grants will enhance our ability to attract and retain the world's best researchers.

Recognising research lags are getting longer. There is also ample evidence that the age at which great invention occurs is rising over time. The most convincing evidence for this is provided by Jones (2011), who demonstrates that the age of great invention – as demonstrated by the age at which a scientist did the work which led to a Nobel prize – has been steadily increasing over time. This implies that it is getting harder and harder to get to the scientific frontier. But this change in the fundamental demographics of scientific research is not reflected in our funding system. Apart from the emergence of new schemes like the Discovery Early Career Research awards and the Future Fellowships there is no recognition of the fact that the research process is being drawn out over a longer period of time. In particular, there is virtually no increase in the length of research grants: apart from a few prestigious fellowships, the overwhelming majority of grant awarded are short-term (say 3 years).

On top of this, researchers are under increasing pressure to publish their research in order to get tenure, promotion, improve their institution's ERA ranking, etc. The upside of this is obvious: researchers work harder in to order to demonstrate their suitability to be part of the profession. The downside of this is also pretty obvious: researchers tackle simpler, bite-sized research projects in order to get some papers published as quickly as possible. This effect is particularly pernicious for early-career researchers who are typically given 5 years after their PhD to prove that they are worthy of a tenured research position. It isn't until much later in your career – when the pressure to publish has diminished somewhat – that you have the luxury of tackling blue sky projects.

Keeping sight of the objective. What matters is the *creation* and *transmission* of new knowledge, but we get obsessed with intermediate outcomes like the number of publications and the quality of the journal. If you reward publications, you may just get publications. Focusing on these metrics just leads to bean counting behaviour, not true scholarship. Current incentives in Australia reward academics for slicing their papers into the smallest publishable unit. This is the way to get tenure, promotion, bonuses, etc. But it is not what we really want: what we want is a system that promotes and rewards the discovery of real solutions to real problems; findings that are robust and reproducible. This may or may not be reflected in the number of A* publications a scholar produces in a 5-year window. Academics in the UK are judged after senior assessors read each paper carefully and judge it on its merits (not just where it is published, which is a noisy indicator of the quality of the research).

To highlight the idiocy of the bean counting approach, recent Nobel Laureate in Physics Peter Higgs said he became "…an embarrassment to the department when they did research assessment exercises….Today I wouldn't get an academic job. It's as simple as that. I don't think I would be regarded as productive enough" (Higgs, quoted in The Guardian 7 December 2013). So, great scholars are potentially being left out in the cold because of an undue focus on short-term intermediate outcomes rather than long-term quality of scholarship.

Efficient peer review. It maybe the best system available, but we have made things so bureaucratic (and costly) to administer that the costs seemingly outweigh the benefits. The bottom line is that the current system is noisy and expensive. By noisy, we mean that there is randomness in the decision-making process. Graves, Barnett and Clarke (2011) demonstrate using NH&MRC grant applications that the cost per proposal is about $18k. About 4 in 10 applications are *fundable*: that is, looking at the probability distribution function of possible total scores based on the actual set of scores each application received from 4 assessors. However, only 2 in 10 was actually funded.

There has been a lot of interesting proposals put forward recently which attempt to improve the efficiency of the grant system, while retaining the integrity of peer review. For example, Graves, Barnett and Clarke (2011) advocate a system that is noisy and cheap (which is much better, after all, than one that is noisy and expensive): simply undertake some triage on all applications to determine the top and bottom x% (which is relatively easy to do) and then randomly allocate funds to the remainder. Of course, there are obvious objections to this 'anti-meritocratic' approach of handing out funds randomly. But if you take their results above at face value (and assume that it occurs in the ARC system as well as the NH&MRC system), this is roughly what happens now. And the proposed approach would be much cheaper to administer than asking experts to referee the proposals and then congregating the College of Experts to consider their final verdict.

Another provocative proposal put forward is to allocate a certain amount of money to all researchers with an obligation to pass on a portion (say 50%) to others (e.g. Bollen 2014). This would effectively enable researchers to allocate money to other researchers who they thought were doing good work. This makes some sense: research dollars will naturally flow (on balance) to good scholars and it will provide an incentive to academics to do more to promote the work they are doing. And it accommodates the fact that researchers can continue to work on new ideas as they bubble up during the course of the research (which is the way science actually happens) rather than be forced to try and *ex ante* write out exactly the way their research will pan out in order to attract more funding to continue their work.

Again, this proposal is not without its problems – it has overtones of nepotism (or at least cronyism) and it's not clear how money flows would be administered – but these could potentially be overcome (e.g. 'donations' to other scholars could be anonymous). The bigger issue to note is that these proposals reflect the growing concern about the current system amongst the research community and we should not shy away from difficult conversations that we need to have in order to improve the efficiency of the current funding system.

Opportunity costs. One of the big problems with the current system is the amount of resources required to apply for grants, review/assess grant applications and report on completed grants. All-told, this is an enormous burden on the system and one which takes researchers away from their main function: to do research! Recent survey estimates indicates that this is extremely expensive in terms of both time (550 years for NHMRC Project Grants in 2012) and money (AUD$66 million in salary costs of the investigators, not including assessors) (Herbert, Barnett and Graves 2012). The opportunity cost of this is extraordinarily high since these researchers have spent 10-20 years of their career (in expensive formal and informal training) only to then spent the most productive time of their career away from 'the bench' (i.e. not actually doing research but filling out paperwork).

Understanding why collaboration matters. Much alarm has been spread through the system about the low level of collaboration between university and industry in Australia. In fact, in a recent speech, Minister McFarlane recently said: "I was shocked to see statistics the Chief Scientist provided about Australia's business collaboration with higher education or public research agencies. We ranked 33rd. We need to do better for our economy to continue to grow" (Science meets Parliament, 14th March 2014). To be clear, this does seem to be an issue for Australia and further emphasis should be put on enhancing university-industry collaboration. But it is equally alarming that there isn't more discussion about *why* such collaboration matters. Collaboration is not important, in and of itself: it is a means to an end.

One reason collaboration may matter is because it speeds up the process of diffusing knowledge (once created) to industry. But whether this is true or not is an empirical question that we don't know the answer to, despite the numerous reviews that have been conducted on the CRCs and the like. There have been no large scale statistical studies – using a control group to construct a counterfactual – of the impact of CRCs on firm performance. This general point about the lack of awareness about the systematic effects of government intervention on innovation policy was made by Ben Bernanke when he noted: "Unfortunately, economists know less about how best to channel public support for research and development than we would like…" (May 16, 2011 "Promoting Research and Development: The Government's Role", Conference on "New Building Blocks for Jobs and Economic Growth," Washington, D.C.).

So, more needs to be done to understand how collaboration with universities affects knowledge diffusion and firm/industry performance (in terms of productivity, sales growth, and/or exports). Much the same could be said for other dimensions of collaboration including cross-institution, cross-disciplinary, cross-country and cross-cultural collaboration. These are probably good things to promote, but let's do it in a more reasoned and rigorous fashion. Let's move on beyond anecdotes to reproducible evidence.

The importance of knowledge diffusion. Recall that the objective for research academics is the *creation* and *diffusion* of knowledge. Broadly speaking, there are two dimensions of diffusion: within the academic community and outside the academic community. For some researchers – for

example, those working in very abstract, esoteric fields – only the former matters. And we do a pretty good job at providing resources for academics to communicate with each other via conferences, workshops, journal publications, etc. For other researchers – e.g. most of those people (though not all) working in social and applied sciences – translation of their research into practice matters. For these researchers, it isn't enough to simply come up with brilliant research: they typically want to see that once a problem they have been working on has been solved, the solution is implemented in practice (either by industry or by government).

The problem is that aside from a few schemes – like the NH&MRC Development Grants and the ARC Linkage and ITRP Grants – there is insufficient emphasis of the importance of external knowledge diffusion in the existing funding system. The academics who do external knowledge translation work do it on the smell of an oily rag because they are passionate about it, not because it pays to do so. Some enlightened institutions support this type of work via promoting academics (partly) on the basis of their engagement/impact activities – we are lucky enough to work at one such institution – but it is a day-by-day proposition which isn't financially supported. This needs to be looked at if we are to provide the right incentives. Not all researchers are interested in doing translation work – and nor should they be – but it does appear important to provide incentives for those who are interested in doing this important work. Otherwise, we can be sure to continue to under-invest in putting our new discoveries into practice.

Acknowledging the full cost of research. Most research grants don't pay the cost of the chief investigator's time, which is a huge problem for scholars in research-only environments. In other words, the funders rely on the fact that chief investigators are working in teaching positions which can then be used to cross-subsidise their involvement in research. When they do pay for personnel (research staff, support/technical staff), they don't pay the *full-cost* of employment, rather they just pay the salary component plus on-costs. But this means that the universities have to find some way to cover the other overhead costs (administration, marketing, office space, computers, etc.). Again, this relies on the university being able to cross-subsidise its research activities (normally from teaching). If stand-alone research institutes are going to flourish in our university system – and there are definitely good grounds to argue that we should have specialised research-only institutes in more than just the medical sciences – we need to recognise their needs and address this shortcoming in the funding rules.

Specialisation in research. There are many domains where equity is an important consideration in the allocation of resources, but research isn't one of them. The notion that every university should get its fair share of the research pie seems to run counter-productive to the very notion of research excellence. In the US and the UK, they don't share the research resources around; they are highly concentrated in a small number of outstanding institutions (say 6 or 20 respectively). Concentration is desirable for three reasons:

i) frontier research is extremely difficult and only a small proportion of researchers are capable of doing it, and we should give them adequate resources;
ii) frontier research relies on both economies of scale and scope, which means that there are efficiency gains from having large groups of world-class researchers co-located (in other words, spillovers are large); and
iii) frontier research often requires access to expensive and specialised equipment. Australia may only need one type of each piece of equipment.

This is not to argue that the existing large research-intensive universities should necessarily get larger and more research-intensive. There is a compelling argument to making some smaller, regional universities (e.g. in Far North Queensland, Tasmania) centres of research excellence in fields like marine science or mining. However, the current notion that every university should aspire to being excellent in every research fields seems misguided.

Some Concluding Remarks

Trying to pull these issues together into a coherent set of policy implications is difficult. There is obviously a lot of critique in there, but not much in the way of concrete solutions. The reason for this is simple: we are still really quite ignorant about what works – and what doesn't work – when it comes to effective research funding mechanisms. We need to sort this out before we can make any convincing arguments to modify (and improve) the existing system. On this issue, there is a lot we can do to improve the situation.

One major obstacle to moving forward on this is the lack of a community of practice in Australia around what has become known as the 'science of science and innovation policy' (where 'science' means both the social and physical disciplines). The US has seen the emergence of a nascent 'science of science and innovation policy' program in recent years. This project, spearheaded by Julia Lane is supported by National Science Foundation (NSF) funds and is starting to generate interesting new insights (see Bertuzzi and Lane 2011; Weinberg et al. 2014). But in Australia, this has been slow to take seed. We should do more to promote the evaluation of mechanisms designed to improve our national innovation system, starting with a commitment to build the type of data infrastructure required for social scientists to evaluate the effectiveness of different policy interventions.

Although it comes as somewhat of a surprise to most people, it is true that academics have deep understanding of their own experiences with the research funding system, but not much of an understanding of the overall workings of the system. That is, they have anecdotal evidence but not systematic evidence of 'what works'. We need to move beyond the idea of throwing 3% of GDP at the research system and waiting 25 years for a miracle to occur, which is a crude (but fairly accurate) summation of the current system.

We would be happy to discuss these issues with the Senate Economics References Committee at any stage.

Regards,

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