The value of science

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The key issue for today's meeting is that of the perceived value of public funding of science to both society and the taxpayer. Addressing this is a perennial challenge, and one in which the science community has to do better in advancing the case. For, in the end, policy formation is strongly influenced by political dynamics and public opinion, and investing more in science must be seen to have benefits that politicians can own. In many other countries the science community has done a much better job in enjoining other parts of the community to be advocates for science. Too often, and not without justification, the public perception of science advocacy by scientists is one of self-serving promotion of a narrow piece of research, or of a fabrication based on naïve economic arguments. If science is genuinely for the public good, scientists need to engage the public in the arguments for it. The case is made more problematic when scientists confuse knowledge brokerage with advocacy – both are proper roles, but public trust in science requires clarity as to what role individuals are playing. Such confusion has raised cynicism in policy circles and in the media, yet the issue of the value of science to the taxpayer is both recognisably logical on one hand and has a deep values-based component on the other.

It may seem self-evident that science has cultural, environmental, social, defensive, societal, diplomatic and policy benefits as well as both indirect and direct economic benefits. However, this listing represents a much broader taxonomy describing the utility of science than is generally acknowledged, and some of these components have been largely ignored for too long.

This all leads me to ask the question of why is it that New Zealand has taken a different path in its public science compared to successful western nations for at least three decades, including times when our economy was in a very robust state. Over that time most countries have committed to major increases in public investment in science. This has been particularly the case for the small advanced economies such as Denmark, Israel, and Singapore. In contrast, until recently we have essentially only tinkered with a system established years ago.

Paradoxically, there are cogent reasons why small countries may have to spend disproportionately more, rather than less, if they want to compete in a world moving towards knowledge as the commodity of exchange.

There are three beliefs still held by some in our policy circles that need to be addressed. Firstly, some policy makers accept that the correlation between R&D and economic growth is causal, but not in the way most of us and much of the policy community overseas thinks, where it is accepted that science is a key driver of economic growth. Contrary to this, a prevalent view in some circles is that rich countries have a high investment in R&D because they can afford it. Secondly, because the return on public investment in R&D extends well beyond an electoral life cycle and because there is not a linear relationship between individual research effort and return, this has become reason to assume that there is no urgency to invest. This has negative implications for the human infrastructure of science, both its development and its retention. Thirdly, there still remains the view of some within the policy community that there is a zero-sum game between investment in discovery research and research to benefit economic growth. This ignores the many other critical benefits of public science and indeed is not sustainable – an innovation ecosystem needs a constant flow of frontier knowledge. Besides, since the 1990s most of our effort has gone into promoting private or very targeted public R&D at the expense of sustaining both the quality and depth of discovery research.

A simple look at other small countries shows that, if there is not sufficient volume of basic research, innovation suffers. It would appear from comparator countries that, at about a public sector spend of 0.8% GDP on R&D, there is an inflection in private sector spending. Up to that point, private sector spending is below or equal to public sector spending, but after that point private sector spending takes off. There are many

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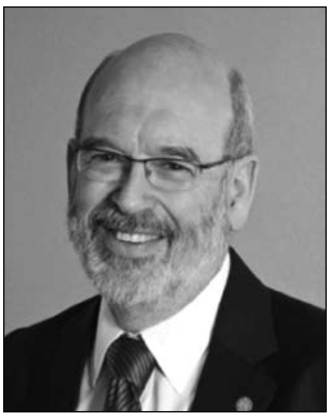

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reasons why that is the case, but I would argue that, because of New Zealand's history and geographical position and lack of multinational companies, we might have to go even higher before we see that return.

We keep beating ourselves up because we have a low private sector spend on R&D and saying that is a failure of our science and enterprise system. That may be unfair. Indeed the recent statistics suggest that private sector R&D is growing faster than the OECD average, albeit from a lower base. The problem is complex because actually our SMEs are relatively research-intensive by global standards – indeed corrected for company size our private sector spend is surprisingly good. The problem is that, without the involvement of the multinationals in our innovation ecosystem, it is difficult to create the environment found in our peer countries.

There are deeper policy issues that will not go away. These include: What is the value proposition that would bring research-intensive multinationals to undertake R&D here? What innovative models will help our enterprises make their way globally but retain their ownership here? Israel in some ways is equally as isolated as New Zealand, but its high-value knowledge-based exports are many times ours because they have developed strategies to both incentivise and retain activity – an important element is a shift from private sector research grants to loans.

Let me segue into a second set of points. I have just finished chairing the first stage of the National Science Challenge process, namely identifying those challenges to recommend to Cabinet – the report was forwarded formally yesterday. During that process we not only incorporated the panel's own views but those of hundreds of academic and public submissions. Through all of this, several points became very clear.

The first is that the siloed nature of the New Zealand science system has not been helpful. There is no doubt that greater incorporation of the social sciences (which have been largely ignored in our contestable systems) into both biological and physical sciences is needed. It also highlights the need for greater multidisciplinarity. Science communities need to appreciate how their work is perceived and incorporated by the community into values-based decisions. Secondly, there is no doubt that those amongst the public who responded to the challenges understood and saw the critical importance of science to New Zealand in social, societal, environmental, health and cultural terms. Thirdly, there is an enormous challenge for New Zealand to up-skill its population in terms of STEM education, science literacy, and the public understanding of science. Fourthly, many of the public submissions focused on better application of current knowledge to solve problems as well as wanting to see policy makers and government make far better use of science and knowledge in their decision making. This last point is something I feel particularly strongly about. I have just released a discussion paper entitled *Interpreting science - implications for public understanding, advocacy and policy formation*[1]; this paper highlights some of my concerns in this regard.

One of the challenges we face in a climate of greater utilitarian attitudes to public expenditure on R&D is how to measure **impact**. Impact is not easy, even in the case of very applied industrial research, because of the long lag times and the multiple inputs into any innovative steps. It gets much harder when one looks at impacts ranging from workforce development through to diplomatic impact and from biosecurity to research that might have impact on public policy. Simple metrics based on publication rates, impact factors, and patents granted simply don't do it. One work programme of an initiative that New Zealand is facilitating, in which we are having very valuable discussions with five other small advanced nations, is to look at a number of innovative ways this might be addressed and see if some consensual approaches can be identified.

To turn to the more practical aspects of the issues we face, a topic that is also one of the foci of the small advanced nations initiative, is the simple reality that small countries cannot do everything in science – they have to identify where they can advance their own interests through science. So how do they choose, how do they prioritise?

To put this in context, globally the public science system is undergoing considerable change. In part this reflects society's wish and expectations to see an ever-greater utilitarian role for science, following the enormous transformations in health, communications, and technology that we have seen since the Second World War. And in part these changes reflect the massive increase in scientific enterprise, partially driven by the massive expansion of tertiary education. Just because the tertiary sector is larger, does that shape the research agenda? Further, the costs of much research have risen rapidly as a result of technological advances. Interdisciplinary and multidisciplinary research is increasingly needed to address many issues – yet its very nature creates significant challenges for the science system. These issues are much more acute in a small country which by definition is more limited in absolute terms in its research spend; yet in small countries the capacities for change are higher and the consequence of bad decisions more severe. At the same time the pressures on the funding system are more intense because there remains the desire, if not the need, to have a broad range of research endeavours across most domains of intellectual enquiry.

So what processes underpin prioritisation? The question occurs even in the big economies, even if it is dealt with indirectly via allocation systems to different funding agencies. In smaller countries the need for more explicit prioritisation comes into play, and some have embarked on formal exercises to decide on national science areas – but even here the exception is always made for exceptional science.

However, domain specificity is only one part of a science priority-setting exercise. There is another axis that is just as important, and that is the relative emphasis to be placed on individuals versus ideas. How this is framed can lead to very different outcomes. There should be clarity as to the extent to which the key points of assessment are about the individual and his/her track record and potential or the project idea *per se*. In New Zealand we have tended to focus on the project, particularly in the major contestable pools, but science systems around the world are beginning to give much greater focus to the quality of the applicant(s) and their teams – recognising that the most 'intellectually entrepreneurial' scientists need to be fostered, thereby allowing teams to be built around them.

[1] See, http://www.pmcsa.org.nz/wp-content/uploads/Interpreting-Science-April-2013.pdf

After all, science is a creative human endeavour. For example, in some countries their equivalents of Centres of Research Excellence are built around individuals. Indeed, experience here as elsewhere suggests that the most academically strong scientists also tend to be the ones whose work has the greatest impact in industry or for the public – it is not, as some would like to claim, either good basic science or second class applied science. While such an approach is not egalitarian (and grant funding cannot be), track record put in perspective remains a better predictor of performance than anything else. However, this will depend on what is meant by 'track record' in the context of the intent to support innovative and impactful research. This focus has to be accompanied by an overt way of identifying and seeding emerging talent.

It is important to note that it is rare for a single project to lead to an innovative breakthrough. Rather, there is a need for a team to compile and build its research over time, and the assessment process therefore needs to look not at projects in isolation but at the trajectory of the research and the performance of those doing it

The research environment has attributes that mean that some people will be more productive than others. Careers cannot be sustained if such individuals and their teams cannot have a realistic expectation of grant renewal, as long as the progress and potential or actual impact of the research justifies it.

Thus arises the second question: How should small advanced nations operate their science funding systems? In view of the fundamentals of scale, a science system in a small country cannot simply be a scaled-down version of what operates in a large country. It is fair to say that this general question is well overdue for consideration, yet the conservative nature of academia and indeed the scientific community has largely precluded it.

So how should funding be allocated? There is a range of processes that can be used to make research funding decisions; equally, there is a range of funding scales, from a travel grant of a few thousand dollars to a research platform of tens of millions. It has generally been accepted that peer review is a core element of funding allocation in science and that the so-called 'Haldane principle' whereby scientists should assess science excellence must operate. In New Zealand we have evolved a system that started as a stripped down version of a big-country system and then has had a 25 year history of *ad hoc* decisions which have led to a system that we cannot honestly say is fit for purpose.

The greater the competition, the more these issues come into focus. In New Zealand, our three major contestable systems all struggle with stiflingly low success rates. Because the most innovative research tends to involve intellectual risk and thus can invite criticism, it is generally accepted that the processes of contestation can bias grant award decisions towards conservatism. This is in contradiction to the need of the nation for science to contribute to cultural (in the academic sense), social, environmental, and economic goals. As my recently appointed Irish counterpart said to me, such a system is designed to produce spectacular mediocrity. Not surprisingly, their own system is now under radical and constructive redesign.

Internationally, peer review is coming under increased focus. The literature suggests considerable discomfort with the peer review system as it generally operates. It is very expensive – indeed the associated costs can be as high as 80% of the total fund costs. I have a discussion paper[2] on my website about these and related issues. The reality is that random effects are far more likely to determine outcome than any attempt at objectivity. Solutions have been suggested and indeed tried elsewhere.

One of the most problematic issues arises because of the chance effects arising from the general and most common model whereby the awarding committee starts with the nominated in-depth reviewer(s) presenting his/her views and then effectively steering discussion, consciously or not, in a certain direction.

Where success rates are low, it is most probably because the impact of bias, marginal negativity, or controversy can be a very strong factor in influencing the success of a proposal. The result is that the more innovative and edgy ideas often get disparate scores and are therefore unlikely to be funded. This would appear to be more likely when the panel is inexperienced and therefore cautious, while conservative research with less impact becomes the norm – even within the lottery of the process. Experience and seniority have real value in such evaluations, particularly where there is a multidisciplinary dimension.

In small countries the potential for conscious or unconscious positive or negative bias by the lead reviewer, other members of the panel, or referees are greater. Peer review relies on the avoidance of conflicts of interest. In a small science community, in any one field there are relatively few experts and they are most likely to be either working collaboratively or actually effectively competing for the same pool of funds. Beyond that, in a small science community, personality and extraneous information can well influence a reviewer or referee, often unconsciously. This problem of potential bias has led countries like Israel, Sweden, and Ireland to use exclusively extra-jurisdictional panels for scientific assessment.

Further issues emerge when factors such as impact and other elements of relevance are inserted into the science review process. Given that the research is publically funded, the taxpayer has the right through the policy/political process to expect applied relevance. However, depending on the type of research, how that is assessed again requires care. Problems emerge when the assessment of science and impact are combined in a single panel. The influence of one on the other means that the criteria used for assessing excellence are often lost or obscured. Depending on the skill sets of those assessing impact, short-term goals will be assessed differently from longer-term research. In general, assessment of impact/relevance of the proposal and appraisal of the quality of research/researcher require separate skill sets and perspectives, and the industry sector does not always get it right – more so as technology development is becoming more disruptive.

So how should the two criteria of research quality and impact/relevance be combined? The consensus has emerged elsewhere that it should follow the pattern of first assessing for excellence and then filtering for fit with priorities or relevance. This makes it more likely that the best research is funded and more likely that the most innovative research, even if it does not fit a priority particularly well, will still be considered.

[2] *Which science to fund: time to review peer review?* See, http://www.pmcsa.org.nz/wp-content/uploads/Which-science-to-fund-time-to-review-peer-review.pdf

So how should a small country address such challenges? The contestable funding system remains the one part of our science and innovation system that has not been looked at in recent years. First, the policy and scientific communities need to agree on what they want to achieve in the grant allocation process – that requires the policy community to work more closely with the science community. Secondly, it must be decided where and when in the allocation system are people or projects the core determinants of the outcomes being sought. Thirdly, we also need to decide at what level of granularity priorities are set, and how? The smaller the pots of money, the greater the problem of granularity and the greater the risk of innovative research not being funded.

There is good evidence to suggest that peer review processes as we undertake them are high in burden and less than ideal in outcome. No perfect process exists, although new systems which appear to be more objective are emerging. It is timely to have a more objective look at the processes underpinning the contestable system, as this is the most important element in matching our research community to the changing shape of our innovation system; when all is said and done, funding decisions determine both careers and what and how science will contribute to our nation.