



Submission to Science System Advisory Group (SSAG)

To: Science System Advisory Group (SSAG)

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Submitter details

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Introduction

The comments that follow relate very largely to public good environmental research since that is within the ambit of my statutory responsibilities. The Advisory Group should be aware that I have published *A review of the funding and prioritisation of environmental research in New Zealand*. Though published in December 2020, nothing has changed in the interim to cause me to significantly change my conclusions. The material painstakingly gathered for the review on a bespoke basis contains the sort of information that should be routinely in the possession of a ministry for research – which New Zealand has now lacked for over a decade.

The Advisory Group needs to familiarise itself with the report which can be found [here](#). The Group needs to be aware that this report followed from a review of New Zealand's environmental reporting system and was followed by a review of the extent to which public expenditure under the Public Finance Act is informed both by the insights of research and monitoring data. This trilogy of reports led to the publication of an overview synthesis entitled [Environmental reporting, research and investment: Do we know if we're making a difference?](#)

While I welcome the appointment of the Advisory Group, I am concerned that we should avoid wheel-reinvention. The Advisory Group should resist the temptation, too often indulged in New Zealand, to see structural or bureaucratic reform as the starting point for seeing how we can do things better. There do need to be changes but they need to make a real difference to the way research is funded and commissioned and its outputs used to inform and enrich society. In particular I believe that we should identify what core scientific capacity New Zealand wants to maintain over the next couple of decades and provide core (ie non contestable) funding to achieve that.



Question Set 1

I would make two general observations about the framing of these questions.

Firstly, they talk about a ‘science, innovation and technology system’ in the singular. I don’t have a problem with framing the current suite of science and innovation investments as being part of *a system*. It is banal to observe that knowledge and innovations can spill over in any direction and that it is artificial to seek to think about this ecosystem in a fragmented or siloed way. That said, the Advisory Group must resist the temptation to see the delivery, and in particular the funding, of research as homogeneous. It isn’t. There may be ‘a’ system, but it has to embrace completely different types of science that make different claims on skill and resources, and proceed over radically different timeframes. The current system’s attempt to reduce the metrics for funding to ‘excellence’ and ‘impact’ is an artefact of a mistaken view about the homogeneity (or fungibility) of research.

Secondly, the framing is very much around particular outcomes – ‘positive sustainable growth and prosperity’, ‘a knowledge-based, diversified economy’, and ‘innovative solutions to emerging challenges’. No one could object to any of these ambitions. But they run the risk of coalescing around a species of social and economic meliorism. Ours is a civilisation that assumes that there are always solutions available to seemingly intractable problems. This in itself is a culturally loaded phenomenon.

It needs to be acknowledged that our science and innovation system must also be able to grapple with challenges for which there are no happy endings in sight. There is a defensive or risk management element of research that is devoted to trying to understand systemic break-down and either how to prevent it or how to learn to live with the consequences. There is a hint at this in the reference to ‘climate change, biodiversity loss, and societal health’. But it could easily be lost sight of in the contest for resources.

If the Advisory Group is going to deliver advice on a single system, it has to be very clear about the quite different outcomes that different fields of research are addressing. In supporting research investments that underpin economic productivity and social cohesion, governments are in the business of making choices under the watchful gaze of interest groups. They can choose to back particular industries – space, advanced manufacturing, pharmaceuticals – but they can’t back them all. Similarly, they can choose from a myriad of social pathologies to fund research that is intended to improve the lives of people. In both cases, lobbyists will be there to help steer the flow of dollars.

By contrast, environmental research is often defensive risk management research in that we have no choice about the biophysical context in which we find ourselves. Of course, choices have to be made about the relative importance of different domains. But the cards we *have been* dealt in terms of land, water and biodiversity and the cards we *are being* dealt by the global community in the form of pollutants at a planetary level are cards we cannot wish away.



Therefore, if the Advisory group insists on having criteria that allow for a homogenous view of making investments across the research system, I strongly suggest that **risks need to be given equal weight to opportunities**. This will give far greater weight to environmental concerns. Significantly, of the [World Economic Forum's top ten long term global risks](#), five are environmental.

In my view giving risk mitigation equal weighting with opportunities is more than defensible. The defining feature of our age is that the opportunities to increase our current standard of living are increasingly being matched by very real risks that it may fall. Behavioural economics suggests we should value these risks even more highly than opportunities because losing what we have negatively impacts our wellbeing twice as much as gaining something new.

The importance of the environment is particularly salient given the importance of the biological or land-based sector to New Zealand's economy. While we may want innovation to spur new industries that reduce this dependence, we can't be dependent on that bet. It also makes sense to invest in protecting our existing industries. These "defensive" environmental investments underwrite the majority of the country's existing economic comparative advantage.

To the extent that economic and social wellbeing relies ultimately on the provision of ecosystem services, we put off understanding what is happening to those services at our peril. And unlike research into artificial intelligence or quantum computing, the **duration of research into many environmental problems needs to unfold over decades**. As I commented in *A review of the funding and prioritisation of environmental research in New Zealand* –

The patient interpretation and understanding of environmental change unfolding over decades does not need to be a slave to novelty or innovation (although those qualities may well be in evidence). It is often through meticulous, continuous work that real, perennially applicable discoveries are made. These serve to overturn long-held assumptions about ecosystem function and very often spawn new ideas and directions.

Environmental research and innovation is pursued for different reasons, in different ways and over different timeframes than other domains within 'the science, innovation and technology system'. Given the large degree of uniqueness that characterises New Zealand's environment including our high levels of endemism, it is likely that much environmental research undertaken in New Zealand will need to be funded by New Zealand. We cannot rely on others to do it for us. It is largely public good research undertaken for reasons that are prescribed by our biophysical context rather than short-term political preferences about wellbeing (despite the fact - as I point out above – that the environment is central to wellbeing in the long term).

The Advisory Group should **avoid applying a common frame to the very different fields of enquiry encompassed by the science system**.



Beyond these two observations I would offer some even more **general comments about the framing of the Advisory Group’s questions.**

The third question in the first set asks what principles “should underpin the design of a science, innovation and technology system”. Comments are then prompted by a strange list of matters few of which have much to do with principles but seem to have quite a bit to do with institutions and incentives. I offer comments under some different headings:

- It should be clearly understood that public good research will rarely be undertaken for private gain. For this reason, a decision not to undertake much environmental research is a guarantee that it will not happen. The same cannot be said for public investment designed to de-risk private research investments.
- Politicians are entitled to take responsibility for the high-level priorities that govern the expenditure of taxpayers’ money on research, but they should be held firmly at arms’ length from the allocation of money to specific projects. The allocation of resources should be in the hands of individuals with an understanding of the broad field of research in question. In the case of environmental research that means environmental experts drawn from a range of fields – and to some extent different countries.
- Question 3 (e) asks “what are some important factors for the government to consider as criteria when prioritising investment in research appropriate to New Zealand’s size and characteristics?” Question 7 (i) in question set 4 broaches a very similar question (How should the balance of research investment extend across from the humanities, social sciences, health sciences, life sciences, physical sciences and earth sciences?)

The question seems to start from the premise that any prioritisation exercise applies to research as a whole. As I noted above, that framing risks once again treating research as some homogeneous ‘thing’ that governments prioritise. The relative weight of research investments in different sectors is certainly a matter of interest in terms of the skill sets available to the society as a whole, but the framing suggests that there is a homogeneous research cake of a certain size that needs to be divided according to some criteria. And indeed, that is what has happened to some extent with the current system focused as it is on excellence and impact.

I would start from a different place. The reasons the Crown invests in research are plural and will relate to the particularities of our social, economic and environmental setting. The criteria that will be useful to determine what environmental research is conducted will relate to the particularities of our climatic, biodiversity and land and water challenges as well as the environmentally mediated risks facing our population and biological industries. These are all matters on which the Ministry for the Environment, working with other central



government agencies, should be able to advise. As I have said above, there is fundamental biophysical research that we cannot avoid. The debate will be about the amount we can afford but that should be judged not in terms of what is an appropriate claim on the total research budget but the nature of the risks and the costs and benefits of having the research base to manage those environmental risks.

It is not my function to comment on the criteria that should apply to other research domains, but I should have thought the same reasoning applies: social science research should address the particularities of our society, its human capital and its social pathologies. This is what core social ministries should be able to advise on. The most problematic sort of public sector research investment to justify is going to be research that is effectively designed to generate growth in industrial, commercial and service sector productivity. This is a completely different universe about which I have no expertise. But I am quite clear that whatever criteria do apply, they are unlikely to have much relevance to investment in public good environmental outcomes. (Of course, the case may well be made for investing in technologies developed in the course of public good environmental research but that is not the rationale for undertaking that research.)

- Questions 3d and 3e both ask about prioritisation and leverage in the context of a small economy such as ours. Notwithstanding my comments above, if and when decisions about prioritisation are made it is worth bearing the following in mind.

Given the size of New Zealand any publicly funded research in New Zealand needs to take account of the global context in which it is being undertaken. That context is currently changing and for the worse. The open geo-political horizons that characterised the post-Cold War world are rapidly shrinking. The capacity of small nations in particular to access what they need and when they need it is increasingly at risk and will become even more threatened if the security environment deteriorates further. Supply chains are being re-wired as countries move to the more open adoption of industrial policies. Whatever New Zealand may think about the desirability of free trade and open markets, we have to be prepared to live in a world that may not share those ideals.

In addition to these challenges, we are starting to see, locally, the consequences of planetary level environmental breakdown. Climatic disruption, declining biodiversity and pollution of the global commons comes with a variety of regional impacts from which no economy will be immune. These will tend to reinforce the draw back in cooperation noted in the geo-political sphere. For example, onshoring of productive capability could well be matched by trade policies seeking to look beyond borders to assess process and production methods. Border carbon adjustments are just one example.



These trends will have knock-on consequences for research – who we co-operate with and on what. The effects will be plausibly less severe for pure public good research. But even here, the scale of cross-border environmental problems will make it more important than ever to build research alliances abroad with like-minded and like-affected countries. The Advisory Group should think about what these forces mean for environmental research.

In particular, it should consider the extent to which we should be building centres of excellence and expertise in close cooperation with Australia. We cannot afford to do everything – a degree of trans-Tasman specialisation that sees us contribute to some priorities and they to ours could potentially add more critical mass to our joint efforts. (I observe in passing that the same could well apply to other critical areas such as vaccine development and biosecurity-related research.)

Any discussion about prioritisation should bear in mind the [Productivity Commission's work on frontier firms](#). Discussions about “picking winners” are notoriously difficult – I won't rehearse those arguments here. What I would say is that economic advantage can't be created from nothing, there has to be an existing advantage there to build on. It is all very well talking about the opportunities presented by new technologies but we must also ask where New Zealand has the existing capacity to do world leading research.

In my view the logical order would be as follows. First, we would identify the risks and opportunities we want our research system to cover. Then we need to overlay that with a capability layer. Areas where New Zealand has world class capacity (or close to it) could be an area where we might invest more deeply (perhaps in concert with Australia as I mention above). As the Productivity Commission's work suggests, if we are talking about gaining an *economic* advantage from our research then it has to be world class. There are no points for second best in the world of innovation. As the Productivity Commission pointed out, such areas must be carefully chosen and can't be the subject of fads.

While we will no doubt be world class in some areas of environmental research (given our biological economy) I am also very interested in how we deal with the risks and opportunities that fall *outside* these areas of world class capacity. In areas of risk and opportunity where we don't have that existing world class capacity, we need to invest in ensuring we have the capacity to absorb ideas from overseas and implement them.

Biosecurity is a case in point. If an organism is found in the country, we need to have the capacity ready to identify it and work out the appropriate response as quickly as possible. This “absorptive capacity” needs a high degree of core funding to ensure viability. In my view, the researchers who comprise this ‘absorptive capacity’ should have a good degree of freedom over their scientific inquiries. No government is sufficiently omniscient to be able to make all purchasing decisions appropriately. We must on occasion be prepared to trust our experts.



- Question 3(g) asks “what future are we envisaging for the science, innovation and technology system”. This is quite nebulous – who is we? Again, there seems to be a prior assumption that there is a system about which we can envisage a future. I can only answer that from the perspective of environmental science. At the risk of repeating observations made above, I would suggest that the future of environmental research is likely to be grappling with the same long-run challenges we face today although their manifestations and severity will obviously change. Our environmental science investment will need to accommodate changed ways of doing things and new skill sets, but there have to be profound continuities to enable us to make sense of the biophysical realm we find ourselves in. No one else is going to tackle our problems or understand our bit of the planet’s surface for the same reasons we will.
- Obviously, a key strand of environmentally related research will be engagement with Māori scientists, knowledge and resources. My 2020 investigation recommended a fully developed mātauranga Māori work programme as part of the environmental research agenda.

Question Set 2 (and 5)

This question set focuses on public research organisations. Universities are not apparently regarded as ‘public research organisations’ even though they are major contributors to research. I assume that this because (a) they also perform an important teaching function and (b) are not traditionally directed in deference to an ancient tradition of academic freedom. The fact that they receive significant public funding to carry our research which contributes to society at large and very often of a public good nature means they should not be excluded from consideration – especially in terms of how they come to be funded. After all they contribute significantly to that “absorptive capacity” I talk about above.

That makes the omission of the agencies that fund research from this call for submissions even more remarkable. In my view, the decision to disestablish a dedicated purchasing agent for public good research as part of the reforms that spanned the 2011-2012 period, has been disastrous for role clarity and proper oversight of the expenditure of public research dollars.

As far as environmental research is concerned, the decision to collapse a purpose built research funding institution into MBIE effectively handed the allocation of environmental research dollars to a bureaucracy with no close connection to the environment or to those elements of government responsible for the environment. I urge the Advisory Group to read chapter 4 of my 2020 review which details the plethora of strategies, roadmaps and direction statements that sprouted in the wake of the reforms but which don’t seem to have had much impact at all on how resources are allocated.

The Advisory Group cannot do its job just by asking questions about the role of Crown Research Institutes. Their role is very clearly set out in a statute. No such clarity of role



governs MBIE or its allocation processes. There is profound irony that the Advisory Group is not seeking advice on the role of MBIE. Question set 4 asks about MBIE's role in overseeing contestable research and I comment on that below. But MBIE is also responsible for the Strategic Science Investment Fund which is, in effect, largely CRI core funding that is not contestable except at the margins and for good reason. This is where large swathes of non-sexy, defensive environmental research have been parked and left to wither in real terms. Despite its strategic importance to the Government's core environmental agencies, it is doubtful whether these agencies have any sort of oversight of how these funds are invested.

In my view the Advisory Group should not waste too much time thinking about the role and purpose of CRIs. Their focus can and should change if the way we look at the world and the way we marshal our resources changes. But for the core environmental CRIs – Manaaki Whenua Landcare Research, NIWA and GNS – I consider the broad domains they focus on to be appropriate. Their behaviour as research agencies is driven by the way they are funded. If the Government decided to spend most of its environmentally related money contestably, they will compete to exclude one another. If the Government decides to spend that money on the basis of long-term strategic priorities and demand evidence that these institutes and their university and non-governmental partners are collaborating to leverage their combined resources, they will behave differently.

The Government's purchase and investment function in respect of environmental research has been subjected to little scrutiny and its formal detachment from the Government's and the nation's key areas of risk management is one of the most serious shortcomings of the last decade. My 2020 report provides advice on how to remedy this situation.

In respect of the governance and management of CRIs, there is a need to consider whether we are maintaining core physical infrastructure and the necessary breadth and depth of the research workforce. But before conclusions are drawn about management or governance failure, the Advisory Group should satisfy itself of the extent to which MBIE has paid any attention to the consequences of its contestable funding mechanisms for the core, long-run functions and capabilities of CRIs.

I do not wish to suggest that everything can be secured through the 'purchase' of research, but if funding is left to wither the best leadership in the world is not going to be able to maintain public good research outputs. The fate of collections and databases, discussed at length in my 2020 report, is a particular case in point.

As an aside, an important point to consider when looking at the efficacy of contestable funding is whether or not the playing field is level for different research institutions. Much of the overhead costs and some of the research staff costs universities face are funded through general tertiary education funding. As a result, university researchers bidding into science system contestable funds need only cost the marginal additional cost of doing the research (including non-tertiary education funded staff). By contrast CRIs (and independent research originations) who must operate commercially need to fully cost



their contestable research bids to cover both the direct research costs as well as the overheads. (While a portion of overheads can be covered by SSIF, SSIF is a contract to purchase specific research and should only cover the overheads related to that research.)

Question 4 (d) asks how public research organisations manage intellectual property. In the environmental space, most of what is purchased is purchased for public good reasons. CRIs should not be trying to leverage that investment in a way that means that the general public pays again for data that has already been funded. But CRIs will inevitably see opportunities to add value and if that can be done in a way that allows private entities to create new products and services, that seems to me to amount to useful collateral advantage to the economy. But those new products and services should not effectively privatise core knowledge assets that were generated for the public good. This is what boards and shareholding ministers are there to provide oversight of.

My comments above are also relevant to Question 8 (set 5) about the Government's research needs.

Question Set 4

I have already dealt with question 7(i) above which seems to me to be misplaced here.

Question 7 talks about mission-led and contestable research. These are not alternatives – the former describes the purpose for which a type of research is commissioned, the latter describes a mechanism for triaging applications for funding. As such these are not helpful descriptors for what it is that is being compared.

I am assuming that mission-led research is research that is undertaken to address defined challenges, risks and opportunities that a government considers to be the appropriate subject of public research investment.

Mission-led research can be funded either by grants negotiated with research providers or through a contestable mechanism that invites alternative ways to tackle defined research missions, or more likely a mix of both. A contestable mechanism also lends itself to the allocation of funds to much more blue skies research where the Government does not pretend that the purpose of the funding is anything more than contributing to the broad knowledge base of the society. The hope is generally held that such research will yield insights that can support mission-led research (and wider social, environmental and economic progress) but it would be a mistake to imagine there was some linearity about the relationship.

Rather than critique the fruit salad of mechanisms currently on offer, I refer the Advisory Group to my 2020 report which suggests the establishment of an Environmental Research Council appointed to be the Government's expert investor in a long-term environmental research strategy. The strategy should not be developed by the Council. Its development should be led (not dictated) by the Ministry for the Environment. This should be high-level and address the key challenges that for the time being demand attention under the six



major environmental outcomes identified in *Environmental Reporting, Research and Investment: Do we know if we're making a difference?* (PCE 2022, p. 61).

Once medium-term priorities are communicated to the Council, it should be left to invest in a range of research activities that it considers most likely to make progress without interference by political or bureaucratic players. It could deploy whatever range of long-term negotiated contracts or contestable tools it considered most likely to galvanise the research community to deliver. The Council would be accountable to the Government for its execution of the investment strategy. In other words, has it invested in a way that supports the strategy and represents value for money.

It is not my role to comment on whether this model could apply to other research domains. On the face of it, it could work well for a social research strategy. My instinct is that it would be unnecessarily *dirigiste* for allocating money to research designed to boost the productivity of sectors of the economy. Again, in line with my comments about the heterogeneity of research, I am agnostic about the need for all public research investments to be made through the same channels and the wisdom of having some over-arching 'NZ Inc Research Council'. You would do that if you saw public funding for research as a cake that needed to be apportioned according to some rational calculus.

Such a mechanism could undoubtedly be made to work and it would certainly be preferable to a government ministry like MBIE trying to be the choreographer of the entire research ballet. Such a Council would almost certainly need to delegate its investment decisions to sub-councils to enable them to attract the expertise they need.

But I prefer a less centralised approach in which environmental, social and productivity-related investments are made by different expert funding councils deploying the tools that make sense given the different research challenges these fields present. An Environmental Research Council should be made up of eminent people who understand the environment, understand environmental research and are drawn from both within New Zealand and abroad.

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Te Kaitiaki Taiao a Te Whare Pāremata