RESEARCH, SCIENCE AND INNOVATION - TE ARA PAERANGI FUTURE PATHWAYS SUMMARY 2021

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Defining Research as "research" seems pretty silly. The glossary is slightly better, but it is a pretty nebulous concept. How aobut "acquisition of new knowledge"?

" seeking to encourage combined property planning and co-location between CRIs and universities" - Largely ineffective, as the CRI buildings are locked, and one can't wander in and have a cuppa, and so one doesn't. Also, it creates jealousy to see well-funded CRIs when the unis are starving for funds. And so CRIs aren't much interested in what their poor relations are doing with recycled plastic bags etc. A more major shift in the relationship is required. How do you fund collaboration, when the CRIs (and indeed any organisation) think that the money will be best spent at home, rather than wasted on Uni staff or students, even though the latter are the cheapest innovators science can get.

investment in "new, proactive investments in research areas of emerging importance".
But for an area to be "emerging" there needs to have been some research which indicates it could be important. This is called "blue skies' research" by the Marsden fund, but there it is so sparsely funded that there is little "emerging" info being obtained. You cannot predict the future areas without serendipity in research.

EG: We are TRYING to publish a paper on carbon sequestration in tussock grasslands, C seq being the balance between fixation and release of carbon. Our ho. of changes in Cseq with altitude (AKA temperature) has turned out to be trash. Instead, the fixation follows established trends, but the release (ie decomposition) does not. We did NOT expect to find this, but it is a) true, and b) virtually unpublishable, because it represents a paradigm shift from the 1000 ecological papers saying release changes with treatment. (I'd like to take credit for that shift, but soil scientists have known about it for years!)

How could we have expected that outcome? We were not funded to find it - it merely emerged from our data, and it took me 3 years to accept it. You have to know something is there to find something, and recognise that you have found it.
Most successful Marsden applicants now put in bids for project which they have already completed, so they can guarantee the outcomes to satisfy the fund. This is not "Emerging" research. We need to fund scientists/ researchers to just dip around, and see what they come up with. What is "blue skies" about that? You cannot plan for outcomes here. And most funding does not find these serendipities by random chance, so managerial expectations need to relax and funding needs to increase in quantity and decrease in rigidity.

"Priorities" - these will be governmental, and driven by money. It makes sense to do most of this kind of research through the CRIs' who can focus on it (given they have no teaching loads). There will be some serendipitous outcomes here, just by chance. But funding for sere.dip is best put through the unis, who do not have the same pressure to deliver expected results, and can stop and explore odd ones.

"1. What principles could be used to determine the scope and focus of research priorities?
2. What principles should guide a national research priority-setting process and

how can the process best give effect to Te Tiriti?
3. How should the strategy for each research priority be set and how do we operationalise them?"

Answers:
1) Level of nationwide panic, intersecting with level of scientific outcries.
2) The intersection of human well-being (not profit) and environmental sustainability. (Actually, only the first is usually taken into consideration, but that can't last past ecological collapse.) Te Tiriti needs to get this message too.
3) Ask the researchers - they will know. Don't always go for majority votes here though, because you need to keep an eye out for paradigm shifts. Operationalise by adding money, preferably in small amounts. Large sums only create bureaucracies intended to harvest same.

"regionally-based Māori knowledge"
Knowledge is good. Research is different. It is the creation/extraction of new knowledge.

"4. How would you like to be engaged throughout the Future Pathways programme?
5. What are your thoughts on how to enable and protect mātauranga Māori in the research system?
6. What are your thoughts on regionally based Māori knowledge hubs?"

Answers:
4) Would like to be engaged, but expect to just be frustrated and despondent. Go on - surprise me!
5) This sounds racist, but if Maori wish to do (or be seen to do) more research, they either need to establish (or just explain) their own set of protocols or adopt accepted ones. The advantage of the former is the prospect of a different random sampling of serendipitous discoveries. The advantage of the latter is acceptance and thus publication.
6) Is this like a library? What does this mean? What will stop it ossifying? Knowledge can be stored as it is an outcome. Science cannot, as it is a process.

"unproductive competition"
Is there any evidence that this exists? Is competition unproductive? Usually trajectories are so variable (randomly) that apparent competition quickly turns to divergence in goals. I suspect this comment is predicated on the costs of multilying equipment and supplies. EG we have a very expensive spectrophotometer in our building now that no remaining employee knows how to operate. Is this sensible? But this problem was not brought about by competition, but by lack of commitment (ie funds) to an on-going project in a particular area.

"by around 75% since 2010"
Well, none of this has trickled down to me. Only in my new school have I come across any money for years (decades), and that is only $900 pa. (But more than I am used to!) If it costs money, I don't do it.)
And money ($10 000) I've earned from contracts (at $8 per hour!), and reserved to fund post-grads, was taken from me to shore up the uni's annual accounts. I think the problem here is not competition, but the costs of administration.

"New Zealand funds the 'full cost' of research via an overhead component"
Clearly this idea is stupid. It doesn't work for hip replacements any more than it works for scientists. Most good uni science is not only sere.dip., but it is almost always effectively gratuitous. You can't fund a postgrad to work 18 hour days exploring some daft byway which has consumed their attention. You can only make sure they have enough to eat and a place to sleep (the sick room will do for the latter).

7. How should we determine what constitutes a core function and how do we

fund them?
8. Do you think a base grant funding model will improve stability and resilience for organisations? How should we go about designing and implementing such a funding model?

7) Use a semi-plasuible name for an organisation and then allow its functions to drift in the direction of the knowledge current. Once a decade, revisit, and decide either to redirect the current (if you can do so without damaging it) or rename the unit.
8) Yes. Hand out some dollars (on a regular basis) and butt out.

"Our CRIs were created in the early 1990s"
Yes, we wondered at the time, if you were doing anything useful there. Some really good govt. departments were trashed.
Maybe return to the old model, with overhead funding, and leave the researchers to DIY. Note that only agriforestry and science was covered by govt depts. Are you thinking of a research body on NZ history or society or literature etc? Do all the govt depatments have research wings? Is that intentional? Is it good?

"Māori, to build strong research relationships and navigate the system."
Are you aware this is downgrading, racist rubbish? My Maori colleagues tell me they are (over)due for some of this "building", but I suspect it will turn out to be poisoned fruit. If/When their research is any good, it will/does stand on its own.

"Fewer, larger, more resilient organisations could result in greater connectivity and inter-disciplinary research, more co-ordinated investment in research infrastructure creating hubs of capability across multiple sectors."
The sectors can't be both "fewer" and "multiple" Make up your mind.
Large organisations (according to all the specialists) quickly become bureaucratic monsters. EG Teaching and research are the goals of a university, but these activities are ONLY funded from the small amounts of money left over after the admin has taken all it can (though probably not as much as it wants). Maybe many, smaller organisations, with specific fields, might be better? Maybe somewhere in between?

Innovation agency,
You can't innovate to order or by plan. You can only leave capacity for people who happen to innovate (by sere.dip.) to explore what they want to.
I think you are confusing "problem-solving" here with innovation. Often innovation comes alongside problem-solving, but it is not the driver. Look at all the innovations from NASA's space programme (CRC!!), none of which were planned. They were byproducts of someone's solution to a problem.
So having silly, meaningless unachievable goals like going to the moon or Mars is helpful as a way of funding sere.dip. by accident.
Maybe NZ could aim for "pest-free, sustainable NZ", and lots of good stuff will pop out of that along the way?

9. How do we design collaborative, adaptive and agile research institutions that will serve our current and future needs?
10. How can institutions be designed to better support capability, skills and workforce development?
11. How should we make decisions on large property and capital investments under a more coordinated approach?
12. How do we design Te Tiriti enabled institutions?
13. How do we better support knowledge exchange and impact generation? What should be the role of research institutions in transferring knowledge into operational environments and technologies?

Answers
9) You give funding with minimal constraints, and leave scientists alone as much as possible.
10) Let the decisions be made by the scientists, not by managers. Use scientists to run organisations not managers.
11) Get the organisations to inventory their expensive gear, decide if they really need it, and pass it on to those who do need it. Remember some science is location-specific, like agriculture, or wetlands or volcanoes, but other stuff is universal, like physics and chemistry. Plan for that.
12) You shouldn't need to.
13) *Knowledge exchange* - well, there are hardly any journals left which do not have page charges, and since I can't pay any (though I do have $100 for them for this year!!) that limits my exchanging. Is this a sensible outcome for research?
Impact generation - people are being battered to death by info already. Do you need to add more clouts? Marsden has lots of impact, but the scientific advance can be more dubious. Is impact the best goal, or do we want utility (to humans and environments)?
It is difficult to avoid dumbing down info for the understanding of the general public without a long-term campaign to increase their scientific background. But eventually they got to handle covid modelling pretty well, so clearly this is achievable.

"Career precarity"
What is this? A new word for "precariousness"? I am all for new words, but it helps if they also carry new meanings.

14. How should we include workforce considerations in the design of research Priorities?
15. What impact would a base grant have on the research workforce?
16. How do we design new funding mechanisms that strongly focus on workforce outcomes?

Answers
14) It is deeply offensive to appoint someone because they are LGBT or female or purple or whatever. Don't do it. This problem requires social modification, not scientific. Even today men tend to appoint women only if they find them sexually attractive. Maybe, until society wises up, appointments should be made by teams that reflect themselves the characteristics you want appointed?
15) What is a base grant?
16) That is the basic problem. You cannot guarantee "outcomes" from science, and any emphasis on outcomes tends to be counter-productive. Actually this question might just be about gender-balance etc, which idea is offensive.

"targeted to high-priority areas"
The problem is that by the time you know an area is "high-priority", you are already too late. Perhaps a better model is to fund such a priority when you have recognised it, in addition to all the background research which allows you to identify and articulate problems. This seems to have worked well during Covid, though I suspect that all the epidemiologists haven't been paid for all/any of their time, work and expertise. This can't go on for ever, and remember that during the BSE outbreaks, the UK govt. couldn't buy epidemiologists for trying. NZ needs to be self-sustaining in its science.

17. How do we support sustainable, effcient and enabling investment in research infrastructure?

Answer
Try giving them some money.