WHAT MAKES A SUCCESSFUL RESEARCH SYSTEM?

Aotearoa needs a strong RSI. A fundamental step to achieving this is considering what the important constituents of a strong research system are. It is thus **strange that this has not been done (at least explicitly). Three elemental features (beyond money) are required:**

1) People of the highest calibre who are given the capacity to do what they do best.
2) Allowance for risk and failure. The most important, innovative, and impactful research occurs on the bleeding edge of knowledge. It, by necessity, entails a high degree of risk and thus incurs a high degree of failure. This is inherent, and needs to be embraced, not rejected.
3) A broad research portfolio. Security of the country requires a broad portfolio of research so that we have the expertise, no matter what the eventuality. This is akin to the immune system: most antibody types are not critical contributors to our survival, but having that diversity means that one of those antibodies can save your life when something novel threatens.

The green paper largely ignores these three elemental features. All three are recognized at points within the paper, but the paper's general thrust at best ignores and at worst works in direct opposition to these features. Without directly focusing on how to achieve all three, changes that substantively improve the country's RSI will not occur. The evolving situation in Europe has thrown what is at stake into stark relief. If there is a nuclear event or some other cataclysm, we will face massive energy shortages, commodity shortages, etc. We will need to figure how to do things that we previously imported solutions to, and things that no one has ever done before. Many of the problems we will face are not predictable, despite what futurists might claim. A strong and diverse RSI will be a key part of the response, and having a surfeit of 2nd-rate researchers in "priority" areas that are suddenly worthless will be a major impediment. Its not that our RSI should be designed for the possibility of nuclear war, it is that this more vividly illustrates the problem no matter what we want the RSI to achieve.

How, then, to achieve a strong RSI? To do so requires addressing the three elemental features listed above.

Achieving the 1st elemental feature (researcher excellence), we need a focus on improving the excellence of our research workforce. This is partly recognized towards the end of the green paper ("*We have noted that many overseas research systems have a serious approach to talent development, resourcing, attraction and retention, with a strongly international mindset. Many research systems support early to mid-career researchers, with pathways to establish programmes and teams, and have dedicated schemes for attracting and retaining outstanding researchers to establish research programmes.*"). To do this, we probably need a radical rethink of the structure of the research workforce to better emphasize and support excellence. One way is to *move to something more akin to the Australian system where researchers of the highest calibre are given baseline funding for research and largely relieved from other duties* (e.g. teaching, admin). Other researchers would not receive this baseline funding and thus would be expected to contribute more to these other tasks. An existing example is the Rutherford Discovery Fellowships. I'm not aware of analyses of its success, but I imagine overall it would be high. However, it stops at the ECR level and thus doesn't allow sustained research excellence. That is not an argument for continuing the funding of existing RDFs further, but

for providing equivalent schemes at a far broader level at all career stages. However, the system also needs to be flexible enough that people can switch between types – that it is not a canalized system.

It may also be possible to better incentivize researcher excellence through the PBRF (it is ludicrous and emblematic of the fragmented nature of our RSI that PBRF isn't really in scope). The PBRF incentivizes this now, but it could be strengthened. Perhaps an **A** could give the researcher dedicated research funding for the period, none of which can be taken by their institute, with the institute also receiving buy-out money to allow relief of duties. This may not be the best solution - considerable thought is required to develop something that will achieve its aims but doesn't disadvantage younger researchers, etc. *Regardless, somehow we need consistent incentivization throughout the RSI of excellence that is specifically targeted at ensuring recruitment and retention of the highest calibre researchers*.

We also need to ensure that our best young researchers get experience of major overseas research institutes, along the lines of the "overseas postdoc experience". This is both to develop international contacts, but more critically to expose them to the realities of research in elite research-focused institutions. Some will not come back, which is not a problem if we recruit sufficient overseas researchers of equal calibre here.

The good discussion on research job precarity in the green paper, which correctly recognizes the imbalance between the number of PhD students graduated and jobs available, is also relevant. *It is time for a major re-think of PhD training to further encourage the development of excellence*. We train too many PhDs of insufficient quality - less than half the NZ PhD theses I've examined providing sufficient evidence for the ability to act as an independent researcher. A 4-year PhD program is a good option, and could be enacted along with funding 1/3rd less PhD students than currently are (thus funding-neutral). This would allow greater training of these students to better ensure they are equipped for independent research.

Finally, I note there is push-back against the idea of excellence in academia. A quick examination of the arguments readily shows they are as vacuous as it seems.

Achieving the 2nd elemental feature (appropriate risk-reward structure) requires *concerted culture change throughout the RSI to properly understand risk-reward and ensure it is managed for the best outcomes*. Essentially, this requires viewing research as akin to venture capitalism, where the magnitude of the rare successes outweighs the more frequent failures. This fundamental aspect of research is largely unrecognized or ignored - explicit ways of embedding it in the RSI are required. It is beyond my capabilities to know how, but recognizing its importance is the first step.

To achieve the 3rd elemental feature (broad research portfolio), we need to move away from the ***overwhelming*** emphasis in the green paper on research priorities. *Research priorities work in direct opposition to making the research system broad and diverse (and to increasing researcher excellence)*. Research priorities are deeply problematic for several reasons:

- They assume we already know what major problems we will face, which we do not (three years ago research on coronaviruses in Aotearoa would have been seen as a

waste of money). Thus, they are, necessarily, reactive and backward- (rather than future-) focused
- they lower excellence, as fit-to-priority means that lower calibre people perceived as fitting the priority are hired over higher calibre people that are seen as not fitting
- they retard research excellence, as researchers bend their research programs to fit a priority rather than conducting research in the most productive way
- they suppress innovation, as the priorities are necessarily subjectively interpreted, leading to status quo ideas being favoured at the expense of out-of-the-box thinking (e.g., health-focused money is not directed at basic yeast research, even though this research has provided some of the most pivotal breakthroughs in cancer biology)
- they are end-goal driven, which makes them inevitably risk-adverse (research must reach milestones, produce outputs, etc, rather than investigate transformative ideas with a high failure risk). Thus, they don't invest in the high-risk, high-reward research portfolios that are required for major progress
- they constrain the breadth of research that is undertaken, both explicitly (by hiring in and funding some areas but not others) and implicitly (by forcing researchers to gravitate to certain areas to follow the money)

It is even recognized in the green paper that priorities constrain transformation: "*important that parts of the research system are not solely priority driven, to allow for wide ranging innovative and transformative research*". Despite this, the paper still focuses almost entirely on priorities.

Research priorities are also completely unnecessary. The highest calibre researchers are already driven to answering the most important questions, including those seen as "priorities", as is recognized in the green paper: "*Researchers naturally seek to address the most important and pressing opportunities and problems that are facing people and the planet*." But it is immediately (and without any justification) contradicted: "*The research system needs a set of clearly expressed, whole-of-system research Priorities*". It does not – what it needs is excellence and diversity, and ways to support both.

That research priorities are a poor strategy compared to a focus on excellence is seen when comparing CoREs with NSCs. There is evidence that CoREs produce better research than NSCs, which is expected as CoREs are based on excellence and thus don't suffer the limitations that research priorities impose on NSCs. **Expansion of the CoRE portfolio would be a better way of ensuring high quality research directed at national priorities** because A) they deliver high quality research, B) they can have diversity of research area as one criterion, C) they have an explicit focus on excellence, including of the researchers, and D) an expanded portfolio will inevitably include CoREs on areas of priority to the country, assuming that people hired in those areas are of sufficient quality. This will require some substantial changes in the ways CoREs are envisaged and selected, as currently the process strongly favours the status quo over transformative ideas.

CRIs are also relevant in this context, as they are essentially research priorities. The green paper does a good job of identifying some of the major problems with CRIs, many of which are also a direct result of the limitations imposed by research priorities that I listed above. CRIs should be the jewels in the country's research crown. The key to them achieving this ambition is to loosen their research priorities – they should be interpreted more broadly to create a broader research portfolio within each general area, whilst changing the focus to

transformative research that appropriately views risk/reward/failure and that is long-term in its outlook (with the recognition that those who create money-making knowledge are not those who will use that knowledge to make the money).

One place with sensible discussion of research priorities is 3.2 "FUNDING CORE FUNCTIONS". I agree that certain activities of strategic importance need core funding. This is deeply tied to section 6 "RESEARCH INFRASTRUCTURE". I think the "data-as-infrastructure" idea here ("*Research infrastructure can also include key data infrastructure*.") is exactly right. It may be worthwhile looking at what Australia has done with their *BioPlatforms* program as an example of how to achieve this (in biology). Thought should also be given to **a CRI on data infrastructure**: a central place for developing and running data-based infrastructure (databases, etc) would save continual reinvention of informatic, security and ethical wheels, and would promote data interoperability where useful. Difficult decisions would still need to be made on what is included, as recognized.

Finally, *extreme care is needed in any use of "impacts"* (in section 4.6) in the RSI, as these can be dangerous and counter-productive for similar reasons to research priorities. The critical point that needs explicit recognition is that impacts are OK as *one possible* research outcome, but are limited to applied research close to actual application (i.e. a narrow slice of the entire RSI portfolio). Requiring impact more broadly forces research away from high-risk/high-reward, transformative endeavours into incremental research. The full spectrum of research outcomes, i.e. generation of fundamental knowledge; transformative ideas with an applied goal; and implementation of the applications (where "applied" includes both commercial and public good outcomes) are overlapping domains that all need to be catered for. I see little explicit recognition of this in the green paper.

In summary, *making a diverse foundation of research endeavours characterized by excellence requires de-emphasis of research priorities*. The profusion of research priority structures (NSCs, CRIs, HRC, other targeted funding, etc) should be dismantled or re-made as broad and as loosely-interpreted as possible. **If the Govt wants research of a specific nature conducted, consideration should be given to research prizes rather than priorities**. The idea, based on what has been shown to work for other research questions internationally, is that achieving a stated goal results in a prize being awarded, rather than funding being awarded in advance. The research that goes into to achieving the goal would need to be funded from grants (i.e. existing, not goal-specific, grants). A funding system based on excellence rather than priorities would fund any good ideas in the area, allowing the research team to build towards achieving the prize's goal. The prize would be a large monetary sum that is essentially a post-hoc research grant where the researchers are entirely free to decide what research to undertake with it (i.e. no strings attached). There could be different award amounts, depending on the scale involved (e.g. something requiring substantial researcher collaboration would have a much larger prize than single-investigator-type research). Care would be needed to ensure a gig economy-type dynamic does not appear, but the prizes would not be fundamentally competitive –researchers would, in general, be encouraged to band together, rather than compete.

TE TIRITI, MĀTAURANGA MĀORI AND MĀORI ASPIRATIONS

I was heartened that the green paper has realized the two distinct aspects to this question, namely how to enable Māori research aspirations and how to prevent misappropriation within the RSI: "*The dual challenges of underinvestment in and mismanagement of mātauranga Māori highlights an overarching need to strengthen the ways our system understands and invests in mātauranga Māori. Our research system needs stronger and explicit processes, procedures and mechanisms to ensure that mātauranga Māori is not misappropriated within our system, and that the mana or mandate for its use is appropriately retained by its Māori owners or kaitiaki*."

These two aspects are becoming more-and-more conflated in the RSI, to the detriment of both Māori research aspirations and general research success. In particular, there appears to be a belief that addressing the second one takes care of the first one, and therefore both can be addressed simply by applying mechanisms designed to address the second one well beyond where they have any relevance. This sentence encapsulates the issue: "*How might [MBIE] proposals be assessed in a manner that upholds Te Tiriti and will genuinely involve and beneft Māori?*" MBIE-funded research is supposed to support research with a direct applied outcome. By conflating this goal with the goal of supporting Māori research, we are left with a situation where applied research that involves Māori is favoured (which is fine), but at the expense of applied research that does not involve Māori and non-applied research that involves (or is led by) Māori, even if both the latter are better. **To maximize Māori research success AND research success generally, it is critical that we separate the two goals and address each in its own right**.

Enabling Māori research aspirations

The solution to this is simple, although requiring strong political will. However, the fact that it is the right thing to do, research-wise, morally, and legally, should help. The solution is to listen to what Māori say and give them the ability to determine their own research aspirations, directions and practices. To do this, we should *automatically allocate an appropriate proportion of the total research budget to Māori for them to spend on research as they see fit*. They would, of course, also be able to access all other RSI money (for example grants, etc), but they would also have their own slice to have full control over. That way, integration of Māori aspects into the RSI would come organically from a basis of equality, under the guidance and direction of Māori.

Preventing misappropriation of mātauranga Māori

As I understand it, this was one objective behind the Vision Mātauranga initiative. It is better left to Māori to determine if this framework is still appropriate, even if it has not yet completely solved this problem. However, one possible contributor to it not yet fixing the problem is that the conflation I mentioned above has left many researchers confused about what the point of VM is. Making sure VM's goal is clearly stated and making sure its practice explicitly aligns with that goal may help better attract the focus of researchers who, through ignorance rather than bad intent, engage in research that risks appropriation. I also think my solution to the first goal above will help: the rich network of Māori researchers that will result are likely to wish to engage with Crown-based researchers on various research aspects, thus enabling proper bi-directional research engagement and more clearly highlighting where engagement should have occurred but hasn't.

Finally, I note in passing my dismay that the green paper contains a term with white supremacist overtones, “Western science”, even if it is just quoted. The racist overtones of this term that marginalize and demean current and budding non-“Western” scientists in this country and beyond should have no place in a Government document.