Written Evidence Submitted by Professor Peter Grindrod (RFA0108)

Pete Grindrod is a scientist and entrepreneur, spending half his career within industrial roles and half within academia. He has written widely on research strategy, innovation, and research leadership. He has served on both EPSRC and BBSRC Councils, advised the Wellcome Trust and MOD R&D programmes, and was a founding trustee of the Alan Turing Institute.

1. **What gaps in the current UK research and development system might be addressed by an ARPA style approach?**

   Almost all of the present UK R&D provision is framed, prescribed, and managed in a discipline-based or sector-based manner. The research councils (RCs) each have their own interests and communities (medical, physical and engineering science, economic and social, biosciences and biotechnology, arts and humanities) and they are internally organised into programmes, and thus subdivided further by research field and some specific research field interfaces. There is no real area of R&D endeavour that could not be funded – in theory. In practice though the existing barriers are procedural and to do with framing. The gaps are horizontals not verticals.

   The UKRI has sought to manage the inter-RC interfaces, with a number of thematic calls, each including multiples RC, and led by one RC of behalf of the others. Accepting the existing interfaces and finding ways to finesse them thus became a long-term game (over the past two decades); implicitly accepting this was the right way to arrange these matters - it just required some tinkering.

   More recently the UKRI Strategic Priorities Fund[1] has been introduced to accelerate this process, but in a top down way (as opposed to the RC-collaborative bottom-up way) – though this funding itself is predicated on two further beliefs: that of the research being "multi and interdisciplinary", and "involving two or more delivery organisations" (stakeholders that, by definition, must pre-exist and already have identified the mission).

   There is also a number of *targeted* multidisciplinary programmes (UKRI's Industrial Strategy Challenge Fund[2] and Global Challenges Research Fund[3], for example). These are designed to accelerate research that is towards specific stakeholders and goals, and are often required to be stakeholder-led.

   So where is the problem?

   The role of public funding for research must include investments into basic, long-term and highly disruptive (risky, contentious) research that the existing commercial players cannot justify to their own shareholders and backers. If they could, they would. Too often we see public money being corralled by the research roadmaps of existing commercial actors (especially the larger ones). The problem of the "dominance of primes" was highlighted for the MoD's R&D spend[4]; and is an ongoing issue within UKRI and elsewhere. Companies that

---

[1] https://www.ukri.org/research/themes-and-programmes/strategic-priorities-fund/
[2] https://www.ukri.org/innovation/industrial-strategy-challenge-fund/
[3] https://www.ukri.org/research/global-challenges-research-fund/
[4] Grindrod et al (2013) Report Of A Working Party On MOD Science And Technology Spending Level Report - Phase 2: A DSAC Perspective December 2013 D/DST/01/14/16/12 https://tinyurl.com/y7y8nxm3

have existing roadmaps will always welcome some matched funding: but they cannot think outside of those roadmaps – they have justified them to their management and shareholders (backers) and they cannot unpick them or publicly admit that something more radical may be needed. They develop so as to exploit their own existing paradigms, products and services. Yet, the only thing that we know about the *unknown unknowns* is that they are out there. Disrupt or become disrupted.

The taxpayer (as an investor in R&D for the UK economy) should wish for an R&D portfolio that is not overly constrained by the missions and politics of existing UK commercial players and other stakeholders (who have their noses set to their present grindstones); that is not constrained by the *group-think* inherent within the peer review and funding panel processes of the RCs; and not limited by the aims of the UKRI/RCs, to achieve consistent and often relatively short-term successes (impacts – and especially economic impacts) and to avoid failures.

Too often "like has appointed like" into funding leadership positions. Small-scale framing, mechanism re-design and tinkering, and an aversion to risk (and, God-forbid, failure), has hobbled the true potential of the UK's excellent R&D base.

So what should the correct balance be? Perhaps 25% of the total UK R&D spending should be invested into R&D that would be adventurous, contentious, high risk, transformational and radical, capable of providing a strong technology global leadership within new and exciting fields. Such research cannot be funded using the lenses of existing stakeholders nor the existing funding review procedures (see below). In addition the UK as an investor must *follow-on* with transformational funding, bridging the *valley of death* without jeopardies from conflicted stakeholders and members of panels, creating some real *home runs* (see more on this below). Now, in the immediate post-Brexit world, is the right time to accelerate away from old peer review and funding panel-bound processes, that are mired in internal politics and latent vested interests.

The real problems – called "gaps" here – as threefold: the "adventure and disruption" gap; the "deep innovation and extreme picking" gap; and the "government as a shareholder" gap. These are not entirely independent but it is easiest to address them in turn.

Adventure and Disruption

For almost twenty years RCs have wrestled with the definitions of truly adventurous and potentially disruptive research. The problem is that the RCs, their programmes, and their peer reviewers all frame the challenges around individual calls and/or technologies. They create tension within competitions yet they penalise "out of the normal" thinking. That framing introduces a risk-averse nature and a lack of entrepreneurial research thrust: just one poor peer review for any contentious highly novel idea kills it stone dead at a subsequent funding panel, that is itself under the pressure of limited finances - there is an implicit desire to fund things that they are sure will succeed. No particular funding panel can afford to gamble within the UKRI system. Yet we certainly need some risk within the whole portfolio: this is a framing problem.

Very often existing RC bids contain elements that have been de-risked by prior research and focussed on specific outcomes (and possess limited upsides, by design). In fact, to be funded by the present competitive processes, bids must achieve almost unanimous and strong support from both the peer reviewers and funding panel personnel. This is a near impossibility for ideas that are basic, long-term, contentious, risky and yet potentially disruptive, breaking barriers and “outside of the box”. Most research programmes and projects are too prescribed for this: they are too low risk and lack contention amongst experts (and thus adventure). They are from “inside the box”.

Attempts to think at larger scales (for example, a RC “big ideas” suggestions box) remain unfunded, and may be cynical *mirages*, with inherent risks filtered by stolid conventional thinking immersed within the UKRI/RC paradigm. This results in too much caution.

I suggest that the UK taxpayer would be interested in the government funding and delivering a wide portfolio of research including some high risk and highly disruptive research, within the wider portfolio of the present research outcomes (outcomes not outputs).

The ARPA’s horizon scanning activities should be open to both novel “push” from all basic research fields, and to game-changing “pulls” from commercial and public potential users alike. The ARPA must constantly revise what may be possible, and create and articulate visionary challenges. Many radical basic ideas ought to be *dual-usage*, being exploitable within both public and commercial sectors (including via new ventures). Hence the ARPA should be constantly engaged with the widest possible range of actors.

My experience of many UK government horizon scanning functions is that generally they are too closed (containing civil servants and domain experts who are mired within the present culture, constraints, and mindsets). They should instead subsume external entrepreneurial views from those that can think in terms of disruptive early-mover opportunities. This is a great challenge – staying ahead of the mainstream curve : many lessons may be learned from DARPA and similar national institutes.

We should not limit the ARPA to science and technology-based programmes *a priori* – though these are most likely to have the deepest and long-term possibilities for catalysing and delivering contentious disruptive outputs; and defence and security, and online society, own a very wide tech remit. However it may be that social sciences and cultural fields might beget highly scalable themes, via digital platforms, with new knowledge generating new sectors resonating with some citizens.

Deep Innovation and Extreme Picking

The word “innovation” has become devalued. For most purposes within UKRI it is now equivalent to “multidisciplinary + stakeholder-led”. Leading universities have bought into this definition to avoid the UKRI naughty step.

This misses the possibility of deep disruptive innovation within a subject silo[5], or even highly novel methods for doing fundamental things[6].

The ARPA should consider how stellar individuals, leading “fail fast” and highly enabled project teams, could be funded to create staggering proofs of concept. No such basic R&D is fundable at present.

The ARPA should also develop visionary future research leaders, within its own paradigm.

---

[5] e.g. There may be deep innovations within algebraic topology that, with next-generation computation, might produce radical methods for massive data-set characterisation/comparisons that would be disruptive across many fields - a decadal methodological challenge.

[6] e.g. DARPA, Turning to Chemistry for New “Computing” Concepts https://www.darpa.mil/news-events/2017-03-23.

The ARPA must avoid formal peer review and funding panel processes at all costs. The selection of innovative leaders and appropriate R&D programmes requires a new adherence to an "extreme picking" methodology. We should not have open calls within topics, instead we should examine leading researchers and research teams with a view to having them work-up suitable proposals with the ARPA (with no promises).

Only a few distinctive programmes should ever be progressed, and the assessment criteria needs to be based around the radical and disruptive nature of the range of potential outcomes; timeliness (why now?); and the visionary qualities of those selected to lead such programmes. While timely the ARPA must have programmes that have a 10+ year vision for disruptive impact. UKRI is much shorter-term.

I have been very struck by the "extreme picking" methodology adopted by Syncona (owned by the Wellcome Trust). Like many venture funds owned by companies (such as Roche, J&J GSK and Pfizer) they tend to invest for strategic and financial reasons, to access to the *next big thing*, subject to their own horizon scanning.

The government as a shareholder

It is essential that an ARPA should work continuous along-side its own programmes and researchers. The ARPA should exploit its own convening power across both public and commercial sectors to bring in potential exploiters, at the right time, while working with the research programmes to ensure the relevance of proofs of concept and other outcomes.

Translational activities, taking technologies across the *valley of death* to the point where they might be exploited by brand new ventures or existing players, will be essential and highly non-trivial. The ARPA must maintain some entrepreneurial zeal within. Existing RCs have no competence here, aiming to *hand-off* this responsibility to host research institutions. Indeed, partnerships with venture expertise and existing sector expertise may offer the most savvy and effective routes to disruption and global success. The ARPA will need access to (or to ring-fence) follow-on translational funding; freeing up the original researchers to return to further more basic research, while placing the IP in the hands of those best placed to exploit and disrupt. Collaborations with commercial teams, even at a very early stage, may be highly desirable, and should be ARPA-introduced.

This requires a distinct model for the exploitation of IP generated by the "extreme picking" winners. Quite clearly the ARPA teams will invest time at the preparation or bid stage and should thus be a full partner in the research and its outputs. Like the Wellcome Trust it should retain some part ownership in IP generated as a condition of funding in return; and retain a first option on any future consideration of follow-on funding. This model is very distinct from that of UKRI (with their InnovateUK smart grants or RC grants).

Summary Points

- Wide and wise remit across all technologies and all disciplines
- Eschewing consensual peer review and funding panel processes
- Valuing contentious and risky research goals framed as part of the whole national portfolio
- Independence from commercial & public actors imposing their own strategies and roadmaps
- Avoidance of embedded stakeholder engagement at too early a stage
- Deep innovation in basic programmes: so not necessarily multidisciplinary
- Identification of stellar fundable individuals: development within the ARPA paradigm

- Multiple tensioned “fail fast” radical research threads within single challenges
- Horizon scanning that subsumes entrepreneurial and other external views
- Dual usage of disruptive outcomes
- Timely programmes, yet with 10+ year visions for disruption
- Articulating visionary and inclusive challenges
- Extreme picking methods of selection of both researchers and research teams
- Part-ownership of IP with first options to follow-on
- Translational follow-on funding
- Brokering public, commercial, and venture partnerships

2. What are the implications of the new funding agency for existing funding bodies and their approach?

The existing funding bodies should carry on: the ARPA funds adventurous long-term research that they cannot. They rely on peer review and funding panels to ensure excellence, create tension, and some assurance of potential successes. They would be undermined by using the “extreme picking” mechanism, proposed above.

The RCs do a really good job in *gingering-up* their communities and have brought them a long way over the past two decades - for example, in thinking about pathways to impact, relevance to the UK, and so on; and aligning with more permissive classes of beneficiaries. The “impact” question has too often been ducked though, with implicit biases towards either “soft impact”, as opposed to creating new jobs, products and services within the UK (“hard” economic impacts), or to “presumed impacts”, where the existence of KE mechanisms and some engaged identified stakeholders are enough simply to assume that impacts will occur.

In fact for some years I chaired the selection and periodic review of the thematic Integrated Knowledge Centres (funded by InnovateUK and RCs together) and the successful impacts required continuous leadership from the top (the Director, not minions) and governance structures that avoided conflicts of interests between the steering or advisory committees (who might redirect the mission and outcome to new or better opportunities) and the early-identified exploiters (who, of course, would not wish for that).

3. What should be the focus be of the new research funding agency and how should it be structured?

I have discussed the ARPA focus above, as well as a number of ways that the ARPA funded activities are distinct from those funded by present mechanisms.

The ARPA requires a strong vision and an independent director who can assert the agency as radical national presence, and one employing an “extreme picking” funding mechanism, deliberately distinct from those of the other agencies with UKRI. The ARPA must focus on its own mission and not on any particular fields/sectors of R&D. Potentially it could fund any deep scientific R&D investigations, that would be designed to become ventures, through to similar strands with cultural and social beneficiaries.

ARPA must have excellent and visionary programme managers who are hands-on compared to those within existing RCs. At present, once a grant is let by an RC there is rather little interference with the grant holder or even any proper review with real teeth. I can recall being involved in only one case where a large grant (more than £10M) was threatened with termination.

ARPA programme managers should always stretch their investigators to produce proofs of concept in ways that are relevant to all kinds of non-academic stakeholders, not journal publications, including, and especially, venture funders.

4. **What funding should ARPA receive, and how should it distribute this funding to maximise effectiveness?**

   Thinking of the UK citizen as a shareholder in the portfolio of nationally funded R&D, it is clear that the ARPA mission and funding should be about 25% of the whole (it cannot be 10% or less) and cannot be much larger (assuming a zero-ish sum) given the sizable and ongoing reliance of the research providers (such as universities, etc) upon research income won under the present UKRI/RC tensioned calls.

5. **What can be learned from ARPA equivalents in other countries?**

   DARPA is close to the right model, although the UK's ARPA would need to create something more permissive in the way it deals with potential existing exploiters of all types.

   The ARPA itself needs to be completely independent of the present components of UKRI. In part this is because of the "single point of failure" issue, and in part this is to ensure that the ARPA money is strictly ringfenced and independent from the existing mechanisms. We should not allow funding of projects to be shared between the ARPA mechanism and the existing UKRI mechanisms (even that of SFP) since the latter rely on certain framing and axioms which the ARPA should avoid.

6. **What benefits might be gained from basing UK ARPA outside of the 'Golden Triangle' (London, Oxford and Cambridge)?**

   It does not matter much where the ARPA is located. Its research should be carried through with the best people and the very best teams available who can achieve the most with it. Thus we should expect that the UK's best universities will be highly involved, and to continuously be talking to the ARPA and engaged in early planning. So one would not seek to locate where ARPA managers' access to the "Triangle" would be time-consuming or expensive.

   I also believe it would be essential not to co-locate the ARPA with UKRI agencies, at Swindon, to assert its independence.

   Few commentators would be influenced by the tokenism of avoiding the "Triangle". But there are likely to be 100 jobs or so. My vote might go to somewhere in the West Midlands as close as possible to a railway station.

(June 2020)