Script

The Lord Rees of Ludlow OM FRS

It's daunting to speak after Peter Hennessy. But it's a pleasure to share the stage with him – and with David Willetts, who achieved wide cross-party respect during his years as science minister.

Science impinges on more and more aspects of our lives. And it's substantially funded from the public purse. So it's a topic where the wide public, via their elected representatives, quite properly have oversight. The question is when it's best for this oversight to be light touch – and when, contrariwise, government should be more 'hands on'

Let's start with our universities – where much public research funding is spent.

In the US Harvard , MIT and Berkeley are major national assets though the worldwide 'pull' they exert on mobile talent, the collective expertise of their faculty, and the consequent quality of the graduates they feed into all walks of life. Each is embedded in a 'cluster' of research laboratories, small companies, NGOs, and so forth -- to symbiotic benefit. We're fortunate to have a similar model in the UK and should cherish it.

Why is the Haldane principle important for research universities? It's because the traditional 'compact' which attracts their faculty is that in return for their teaching, they can devote part of their time to research in fields of their own choice, and have reasonable prospects of the necessary support. And there would I think be agreement that such support should be allocated on Haldane principles. We mustn't jeopardise the UK's counterparts of these great institutions by putting this 'compact' under threat here – we'll then surely lose out in the competition for mobile top talent. (And incidentally the Chinese have adopted the same model, and are rising in the international league).

When academics extol 'free wheeling' research, where we choose the topic ourselves, -- we risk being accused of an arrogance that disregards our obligations to the public. We would say this, wouldn't we? But there's a good response to such allegations. A significant outcome is more likely to emerge from people who are committed –even obsessed – with the problem they're tackling. Their choices of project are anything but frivolous: what's at stake is a big chunk

of their lives, and their professional reputation-- more than money.

If you ask scientists what they are working on, you will seldom get an inspirational reply like 'seeking to cure cancer' or ' understanding the universe'. They focus on a tiny piece of the puzzle; they tackle something that seems tractable. They're not ducking the big problems -- but they're judging that an oblique approach can often pay off best.

A frontal attack on a 'grand challenge' may be premature. For instance, 45 years ago President Richard Nixon declared a 'war on cancer'. He envisaged this as a national goal, modelled on the then-recent Apollo Moon-landing programme. But there was a crucial difference. The science underpinning Apollo -- rocketry and celestial mechanics -- was already understood. But in the case of cancer the scientists knew too little to be able to target their efforts effectively.

It needs expertise and judgement to decide what problems are timely and tractable. Some manifestly important ones aren't. And, to quote the great Peter Medawar, "Scientists who fail to solve a problem beyond their competence will earn at best the kindly contempt reserved for utopian politicians".

Even the most 'ivory tower' researchers are surely delighted if their work has practical impact. But it's not always recognised how unpredictable, diffuse and long-term this can be. Even in targeted medical research, new drugs takes up to 20 years to develop. And the 'family tree' of innovations in other fields stretches back even further in time, and is more diversely multi-branched. The inventors of lasers in the 1960s used ideas that Einstein developed 40 years earlier, and couldn't foresee that their invention would be used in eye surgery and in DVDs.

The difference in eventual impact between the very best research and the merely good is, by any measure, thousands of percent. But we can't predict who will make the great advances, and when. So if we want to optimize the prospects for discovery, what matters most, even from a narrow accounting perspective, isn't the few percent savings that might be scooped up by improving efficiency in the 'office management' sense. It's setting the best framework to attract committed individuals, and allow them to back their own judgement, and supporting them properly. That's what, for instance, Manchester did for the future discoverers of graphene. That's the case for allocating a substantial chunk of funds in responsive-mode.

But it's also crucial to foster the translation of new research findings into social or commercial benefits – that's the rationale for the Catapaults, Innovate UK, and so forth. But, it's not a simple linear process. Indeed, most UK innovations and start-ups are unlikely to be based on discoveries made here – 90 percent of research is done elsewhere.

But that's why the research universities are doubly valuable. Their faculty and graduates are plugged in to the global 'invisible colleges'. They keep a discerning watch on the world's research --- they can seize on good ideas from anywhere and run with them.

So it's in the UK's interests to foster academic excellence (and 'absorptive capacity') right across the board – even in areas where we can't claim to be world leaders. Our 'dual support' system bemuses our American colleagues -- I tell them that, for all our gripes, it is better than the US system where state-university professors must hustle for grants to meet even basic academic needs. And if we want to retain 'dual support' in a larger and more diverse university system, something like the REF is a necessary evil.

But 'responsive mode' funding alone – an unchecked Haldane principle -- can induce trends that are misaligned with what's best for the long term health of our research ecosystem. I'll mention three ways this can happen.

The first concerns demographics and career prospects for the research population. A recent American report probed the career patterns of HIH funded biomedical scientists. The proportion of grant-holder under 36 has fallen from 16 percent in 1980 to 3 percent today. The proportion of those over sixty has risen even more dramatically. The mean age when researchers get their first grant is 43.

There's a similar trend here, and it augers badly. Some people will become researchers come what may - the nerdish element (I'm among them myself). But a world-class university can't survive just on these. It must attract a share of ambitious young people with flexible talent - the kind who are savvy about their options, and aspire to achieve something by the time they're 30.

Fixing this problem will conflict with the self-interest of older researchers, so one

can't rely on self-government by academics to give a fair outcome

Another tension involves not young and old, but north and south -- between the interests and credentials of the Golden Triangle and the regional pump-priming policy advocated by Heseltine and Osborne. Many feel the government should over-ride Haldane in the interests of balanced regional development.

Be that as it may, and despite the trend towards concentration, it's crucial to retain enough flexibility to allow excellence to sprout and bloom anywhere.

For example: Leicester University is world-class in genetics and in space science; Dundee in bioscience. None of this was planned. Outstanding young researchers in these fields happened to have jobs there and had the enterprise to build up major groups. The system that prevailed in the 1970s and 1980s allowed this. It's important that selectivity shouldn't be so harsh that emergent opportunities like this get choked off.

And a third issue is whether there should be favoured funding – both capital and recurrent -- for priority or strategic subjects. The government in David Willett's time came up with eight 'great technologies'. Clearly the selection of priorities needs expert input, but equally clearly it involves wider criteria than scientific excellence alone.

Some academics are uneasy about all this because they want all funds to be allocated to the best science as judged by peer review. But there's a counter-argument: the total public resources for research, and the matching and follow-up by private sector, will be bigger if the money is boosting topics of obvious timeliness and societal benefit that have a political fair wind.

The 'ring fence' has given predictable funding, albeit steadily shrinking in real terms. If it's perceived by politicians to be supporting scientists unconcerned with a wider agenda it's unlikely to be enlarged or supplemented – we'll lose and forgo opportunities.

Even within a 'pot' earmarked for responsive mode grants, someone has to decide the relative amounts that go to different fields, and different research Councils.

Until 20 years ago, an Advisory Board for the Research Councils, with independent members and an eminent (and competent) chairman, did this apportionment. This ABRC was abolished in 1993, and replaced by a single Director General for the Research Council (John Cadogan). John's successors have had broader responsibilities, thereby diluting their ability to address strategic inter-council priorities. And at the moment there is no senior scientist from outside the civil service in this role. I agree with the Royal Society that this is a serious deficiency – especially as increasingly detailed guidance is emanating from BIS and the Treasury.

There needs to be a high-level, transparent, and independent Science Strategy Advisory Body (SSAB), including representatives from RCUK, CST, research-intensive Government departments and the wider scientific, business and charitable communities. Its primary role would be to advise BIS on strategic investment and national capability. And it would need to advise BIS or RCUK on, for instance, how to deal adequately with expanding areas such as energy research, with IT-related topics such as data analytics and robotics, and with the growing challenges (and expenses) of genomics.

In many ways the this country handles science policy well. As compared to the US the interface with government is closer, the respect for evidence is stronger and the rapport between scientists and legislators is certainly better.

But there are other things we can learn from the Americans. Often the advice that's needed requires a wider range of expertise than a departmental chief scientist and his 'in house' staff can offer – expertise on engineering feasibility as well as science (DECC's off-shore wind farm programme, for instance, would have benefited from more of this). And it's not enough for the experts to sit round a table, have a discussion, and leave officials to take minutes – they need to engage in a more time-consuming way.

The US has the National Research Council (NRC): it's publicly funded –- but controlled by their National Academies, and at arm's length from government departments, and from bodies like the NIH and NSF. The NRC produces reports on technical and policy issues. It also produces, after wide community discussion, regular reviews on fields like space science, recommending priorities -- and these reports carry weight in congressional committees.

Our academies don't now have the resources to do all this. But with these resources, and the engagement of individuals from academia , business, and NGOs, they could provide a better-informed basis for decision-making.

By the way, it's worth mentioning one distinctive advisory body in the US which has no parallel here. This is the JASON group. It was founded in the 1960s with support from the Pentagon. It involves top rank academic scientists. In the early days they were mainly physicists, but the group now embraces other fields. They're bankrolled by the Defense Department, but it's a matter of principle that they choose their own new members. Some -- Dick Garwin and Freeman Dyson, for instance -- have been on it since the 1960s. The JASONs spend about 6 weeks together in the summer, with other meetings during the year.

The sociology and 'chemistry' of such a group hasn't been fully replicated anywhere else. Perhaps we should try to do so in the UK, not for the military but in civilian areas-- the remit of, for instance DECC, DEFRA, or the Department of Transport. The challenge is to assemble a group of really top-ranked scientists who enjoy cross-disciplinary discourse and tossing ideas around. It won't 'take off' unless they dedicate substantial time to it -- and unless the group addresses the kind of problems that play to their strengths .

I've focused on the US, but what about Europe?

In its own way the EU has a Haldane principle. The European Research Council gives grants to outstanding individuals, and has a respected record for its quality of peer review. But of course that's not true of the far larger tranches of funds that DG12 dispenses, as we know from current debate about Anne Glover's post.

In the 'big sciences' -- which require international-scale facilities -- there's long been well-managed European collaboration. CERN in Geneva is the world's leading laboratory in particle physics. And ESO and ESA likewise have world-beating facilities and projects.

These are independent of the EU, with a separate oversight structure -- and of course, as in all international partnerships, we should try to get more than our pro rata share of the action.

These capital intensive sciences aren't of course typical of research. But they're

good portents -- they show that Europe can fully match the US if we optimally develop a European research community. Even 'small sciences' may achieve higher peaks of excellence if they involve more than one nation.

I've run out of time, but just two comments that may resonate with the two humanistic speakers on tonight's panel.

First, although our paymasters focus on the *spin-offs* from scientific research – and that's why this is funded at a higher level than the humanities -- let's not forget its intrinsic value. It's a cultural deprivation to be unaware of Darwinism, DNA and the basic chain of events that led to the emergence of the cosmos, life and our biosphere. Indeed science is the most universal culture, shared by all nations and all faiths.

And if one wants to focus on spin off, here's a final thought:
Two of the most valuable pieces of intellectual property to come from Oxford didn't come from scientists or engineers -- but from Professors of Renaissance Literature and of Anglo Saxon. I refer of course to CS Lewis and JRR Tolkien-- whose works now, decades later, earn billions for the so-called creative industries.

These two distinguished scholars -- both, in style and attitudes, archetype old-style Oxford dons -- would feel disaffected aliens in today's world of REF, line management, and the audit culture. Their values were the traditional ones: commitment to an institution, and to scholarship and learning for their own sake.

Whatever happens, let's hope these ideals won't become extinct – they'd certainly have resonated with Lord Haldane himself.