MAKING SCIENCE WORK

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ved Sir Paul Nurse, direktør ved Francis Crick institute, London, England.

Thank you for giving me the opportunity to speak to the Academy this evening – it is a pleasure to be here and an honour to speak to you. The title of my talk is “Making Science Work”; how we can make the pursuit of science work well, and how we can make good decisions about which scientific research should be supported to work for the public good. The term public good is meant in the widest possible sense: ranging from the contributions science makes to our culture and civilisation through to the applications of science that benefit society, such as improving our health and quality of life, securing sustainability and protection of the environment, and driving innovation to support our economy.

A major focus of my talk today will be on research leading to applications of science, but it is always important to remember that scientific knowledge leads to better understanding of ourselves and the natural world, which is an essential aspect of our culture and civilisation. So science should not be judged solely in a utilitarian manner. This was emphasised by the American physicist Robert Wilson who, when questioned by Congress as to how the Fermilab particle accelerator would help national security, answered: “It has nothing to do directly with defending our country except to make it worth defending.”

The discovery of new scientific knowledge and the application of scientific knowledge are sometimes presented as being very different from each other. The fact is, however, that scientific enquiry has always been concerned both with acquiring knowledge of the natural world and of ourselves, and with using that knowledge for the public good. The early seventeenth century English courtier and philosopher Francis Bacon argued that: “Science improves learning and knowledge, and leads to the relief of man’s estate.”

This argument was reinforced by Robert Hooke at the birth in 1660 of the Royal Society, a sister academy to the Norwegian Academy of Science and Letters, who emphasised how: “Scientific discoveries concerning motion, light, gravity, magnetism and the heavens help to improve shipping, watches, optics and engines for trade and carriage."

There is a continuum from discovery science acquiring new knowledge, through to research aimed at translating scientific knowledge for application, and onto subsequent innovation. This spectrum should be considered as an interactive complex system, with knowledge generated at different places within the continuum, influencing both upstream in the creation of new discoveries, and downstream in the production of new applications. An historic example of how investigations downstream can influence research upstream was the work on improving the steam engine which greatly informed the subsequent formulation of thermodynamics.

It is important to emphasise the need for a continuum of science spanning discovery through translation to innovation. Investing too heavily in a particular part of this spectrum, placing artificial barriers in the continuum, or arguing that different parts of the system are superior to other parts, should all be rejected. Science throughout the continuum shares the same values, skill sets and methodologies, although as I shall discuss there can be differences in emphasis in how the research is carried out.

What factors have to be considered when deciding which scientific research should be supported? One that is crucial is the scientist carrying out the research. Major discoveries in science are usually associated with highly talented individuals who combine a number of qualities: they have in-depth knowledge, are creative, understand the values of science and how research is done, are well motivated, and are effective in achieving what they set out to do.

In-depth knowledge of an area of science is essential, but this needs to be combined with what has been called 'peripheral vision', an understanding and openness to what other sciences can contribute. This is especially required when solution of a research problem needs multi-disciplinary and inter-disciplinary approaches.

Carrying out good scientific research is a creative activity, and scientists have more similarities than might be imagined with those pursuing other creative activities such as the arts, and the media. Like other creative workers, scientists thrive on freedom, and organising them is like 'herding cats'. Freedom of thought, being able to pursue a line of investigation wherever it may lead and to uncover uncomfortable truths, are all crucial to an effective scientific endeavour. A scientist whose thoughts are restrained, who is too strongly directed, or who is unable to freely exchange ideas, will not be an effective scientist. Similarly, societies that are not free and do not encourage the free exchange of ideas, or respect the values of science, cannot be leading scientific powers, because that freedom is closely connected with the creativity required for good science.

Good scientists have to embrace the values of science, have respect for reliable and reproducible data, recognise the need to embrace a sceptical approach which challenges orthodoxy and the scientists' own ideas, abhor the falsification or cherry picking of data, and be committed to the pursuit of truth. Scientific research is hard, and to be effective research scientists need to be highly motivated.

Often this motivation is provided by a passionate curiosity about the natural world, a desire to know how things work, or how they can be directed to achieve particular outcomes. But other motivations are also important, a desire to undertake public good through the eradication of disease, to make something useful, to create economic wealth, or even to become rich or famous, deluded as that may be.

But whatever the motivation, it needs to be strong because the pursuit of research is long and difficult. So in deciding what research should be supported, much attention has to be paid to the scientists carrying out the work, and as far as possible decisions about research projects should be closely associated with assessments of the individuals proposing the projects.

Given this emphasis on the primacy of the individuals carrying out the research, decisions should be guided by the effectiveness of the researchers making the research proposal. The most useful criterion for effectiveness is immediate past progress. Those that have recently carried out high quality research are most likely to continue to do so. In coming to research funding decisions the objective is not to simply support those that write good quality grant proposals but those that will actually carry out good quality research. So attention should be given to actual performance as well as planned activity.

Obviously such an emphasis needs to be tempered for those who have only a limited recent past record, such as early career researchers or those with a break in their careers. In these cases making more use of face-to-face interviews can be very helpful in determining the quality of the researcher making the application. Making good decisions about research funding requires a focus on the quality, passion and performance of the scientist proposing the research.

A perennially vexing question is how prescriptive research funding agencies should be in determining what research areas should be supported. This recurring issue arises because of the tensions between scientists wanting the freedom to decide what projects they should pursue, and society which supports science not simply as a cultural activity increasing knowledge, but also as an activity aimed at improving the lot of humankind through achieving specific useful objectives.

One possible response of funding agencies faced with this issue is to carry out a strategic review to decide priorities and identify research areas judged either as being especially timely for future scientific advances or as reflecting particular needs for society. This can lead to initiatives that shape or sponsor research, sometimes with ring-fenced allocations of research funding. Although well intentioned and sometimes useful, these approaches run the risk of wasting money and funding lower quality research. Let me explain why.

One problem is that decisions are separated from consideration both of specific projects and of the scientist carrying out that project. As a consequence such initiatives may attract less creative and effective scientists who simply follow where resources are being made available.

A second problem is that the identification of favoured and non-favoured research areas is usually made by committees made up of "silver back" senior researchers, sometimes not particularly research active anymore themselves. Such committees are prone to coming up with the rather obvious and may well be behind the cutting edge. Better judgements are more likely to be made by the scientists actually carrying out specific areas of research who are much closer to the research problem being pursued.

So how can this difficult tension be resolved? In my opinion there are three issues that are relevant: first, the Haldane Principle; second, a different approach when considering programmes aimed at achieving applications and specific goals; and third, a more imaginative role for scientific leadership in influencing funding.

The Haldane Principle is usually interpreted as meaning that researchers and not politicians should decide how to spend funds. It is based on the Haldane report written in 1918, although the original report made no reference to any specific principle. The principle means that politicians, informed by external advice, should decide on the overall science budget and the allocation between global programmatic areas along with identifying key priorities such as specific challenges or key infrastructures. However, politicians should not be involved in decisions on specific funding proposals which should be made by researchers using peer review.

This is a sensible view which I would extend further by arguing more generally that decisions should be made as close as possible to the researchers actually carrying out the research. Such thinking should be extended to decision makers further down the funding chain. Those leading research funding bodies should focus their attention on high level priorities avoiding the temptation to become too prescriptive and finely grained in recommendations concerning what areas should be funded. This should be left to those close to the research who are more likely to make sensible decisions.

The point I am making here can be illustrated by a metaphor derived from geographical exploration. Let us imagine that in the nineteenth century a Royal Geographical Society, say based in Oslo or London, supporting an expedition might decide that it wants to sponsor exploration of the Amazon basin, the source of the Nile, or the Antarctic. But it would have been ill advised to be too fine grained in its deliberations specify which Amazon tributary or African lake or South Polar glacier should be the focus of attention. That should be left to the explorer on the ground, not those in Oslo or London. The funder's role should be to define the general geographical region of interest, identify the best explorer and then properly equip that explorer so they can be most effective in the field.

Research funders should behave in the same way. They should put their trust most in the explorer scientist carrying out the research rather than in a committee in Oslo or London. As far as is possible, research funding decisions, especially at the discovery end of the research spectrum, should be driven by the scientists carrying out the research because they are the ones best placed to shape the research agenda. This is the reason why response mode funding is such an effective way to deliver new scientific knowledge.

However, this approach needs modification when a research programme is directed at achieving specific goals or applications which can require more prescriptive behaviour. Goal directed research can occur anywhere in the scientific spectrum, but tends to be more prevalent when thinking about applications through translation and innovation. It is necessary and valuable to identify sectors which are close to application as being areas that are worth supporting. However, identification of sectors worthy of support should be broadly scoped and involve both those carrying out the research and those who want to use outcomes of the research being supported, including financial contribution from those wanting to exploit the research as a statement of their commitment and support.

This more prescriptive approach applies to research close to application across the whole spectrum, as well as for-profit activities driving the economy and not-for-profit activities such as improving health and protecting the environment. But even when decisions are more prescriptive, they always need to be driven by quality, both of the research proposed and of the researcher.

Two further points need to be made. The first is that not all research close to application should be prescriptive; there is an important role for bottom up response mode funding in the translation and innovation parts of the research continuum. The second is that more prescriptive approaches are also sometimes needed at the discovery end of research, for example when assembling large data sets such as genome sequences and meteorological data, or when investing in large infrastructures such as particle accelerators.

A third issue concerns the role of scientific leadership. If after getting good advice a research funding leader decides that a particular research area is important and should receive more support, rather than ring-fencing resources, it is also useful to undertake a process of education and inspiration of researchers so that they become motivated to work in that area. Should the area really be as promising as the research leader thinks, then it will be easy to persuade high quality scientists that there is interesting work to be done, and as a consequence, they will submit proposals to the normal response mode system. Should it not be so interesting, then high quality researchers will be less impressed, and are less likely to be persuaded to submit proposals. Research leaders should be proactive, but not by solely ring-fencing or micro-management of the research agenda, but by educating and inspiring the research community.

Are there any other special features concerning decision making with respect to science closer to application? Science across the whole continuum shares many similarities, and this includes the importance of supporting talented individuals with the ability and passion to get the job done. However, work closer to application is more likely to be multi-disciplinary and may well require greater team work, not only covering more scientific disciplines but also activities outside science, for example finance, market analysis and the law.

It requires effort to get individuals from such diverse backgrounds to work well together, and attention needs to be paid to encouraging mutual respect and to breaking down barriers between them. This would be encouraged if there was much greater permeability between sectors encouraging the transfer of both ideas and people more freely. We have in place too many barriers and silos that inhibit free transfer and encourage suspicion between the very people that need to be working closely together.

One of the problems is that increasing knowledge has led to specialisation, making interactions between different scientists, industry, the public services and other professions more difficult. It was easier to make such contacts in the less complex society at the time of the Industrial Revolution. Take the Lunar Society for example, which met in England during the late 18th century, made up of chemists, biologists, doctors, industrialists, engineers and social reformers, regularly meeting every month to talk and to exchange ideas. This included intellectuals and entrepreneurs such as James Watt, Josiah Wedgewood, Matthew Boulton and Erasmus Darwin. They met together in the Midlands once a month under the full moon, to illuminate them during their ride home after dinner, probably after too much wine.

It was in this atmosphere that the industrial revolution was born, and we need to reproduce it again today. Greater permeability should be promoted, starting with the young, by giving them wider intellectual exposure during higher education and their research training. They need more diverse placements early in their careers with easy exchanges between sectors at all career stages. This is a key message, the promotion of translation and innovation requires good permeability across the sectors.

Much is spoken about the valley of death, the gap between the generation of new knowledge and the application of that new knowledge particularly for commercialisation. Usually the focus of discussion is on providing research support to bridge that gap, but attention also needs to be paid to pushing the bridgeheads further out into the valley. There can be a problem when attempts to translate are made prematurely, before knowledge is sufficiently reliable and complete, especially in the biosciences given the complexity of living organisms. "To rush into translation runs the risk of becoming lost in translation."

A firmer bridgehead needs to be built, involving a more extended and secure knowledge base in the area of interest before attempting to pass over the valley of death. Similarly, the bridgehead on the other side needs to be extended out, with more investment from industry in research aimed at capturing new knowledge from the other side of the valley. Without research capacity and knowledge in industry it will be difficult to build back over the valley of death.

I should say something about impact. Researchers want their research to have impact, to increase knowledge, to contribute to culture, to generate societal benefit, to support the economy. Problems come when naïve and crude metrical applications of impact are made an obligatory part of research funding decisions and assessments. The potential impact of research should be clearly identified if it makes sense to do so, but it does not always make sense to do so. To demand a statement in every research proposal or assessment about impact for societal or economic benefit will often simply result in unhelpful flights of fantasy, of no or limited value. Impact is just one aspect out of a number of factors that need to be considered when assessing a research proposal, and should be provided when relevant, and not at all if irrelevant.

So, how can we make sure that science thrives and continues to bring benefits to our economies? The first requirement is to have a high quality science base.

Several features are important for a high quality science base. There needs to be a tradition of respect for empiricism, emphasising reliable observation and experiment. Science has to be carried out in a culture of openness and freedom. Scientists need to be able to freely express doubts, to be sceptical about established orthodoxy, and must not be too strongly directed from the top, which stifles creativity. We also have to keep a spirit of adventure in science, to take risks and be prepared sometimes to fail, as research at the cutting edge is not always successful.

For science to flourish, a broad portfolio of research investment is required. Funding should be across the continuum of research, ranging from discovery science, through research aimed at translating knowledge for application, onto subsequent innovation leading to the development of new technologies.

Research often needs a longer time scale than is usual with the more short-term priorities of private business, or for that matter of politicians elected on a 5 year cycle. This causes problems with longer-term projects, such as translating scientific advances into useful applications. Bridging the often short-term pressures from commerce and politicians with the longer times required to develop discovery research into effective applications, is crucial. Greater collaboration between publically funded research and private companies can help move science to application.

Excellent scientific research requires talent. The most accomplished scientists in the world need to be trained, and attracted, and the necessity to attract highly trained scientists from around the world has to be reflected in immigration policies. Citizens need an education that allows them to fully participate in a democracy that will increasingly require engagement with scientific matters. Teaching should be of a quality such that those pupils with the talent and inclination to become scientists are inspired to do so. There should be attention on practical science in schools, including natural history, reinforcing the fact that science is built on observation and experiment. Pupils must be inspired by the wonder of science, and need to understand why science generates reliable knowledge. At the very least, everyone leaving school should know the difference between astronomy and astrology.

There are too many barriers between scientists and technologists and engineers, blocking the exchanges needed for innovation. There are further blocks between these communities and those who lead the public services and industry, who need the applications of science. It is essential to break down these barriers, through increasing the permeability of both ideas and people between different sectors. With permeability will come more innovative ideas and greater mutual respect, leading to better progress in translating science into useful applications.

Combine all of this with sufficient resources and good decisions about research funding, then we can make science work for us all, for our culture, for our health, for our quality of life, for protecting our environment and for driving our economies. We need to make science work for the public good, for improving our quality of life, securing sustainability, protecting the environment, and for promoting economic growth. In these ways we can make science work, both to produce the best science and to promote science for the public good.