Paul Nurse - Making science work

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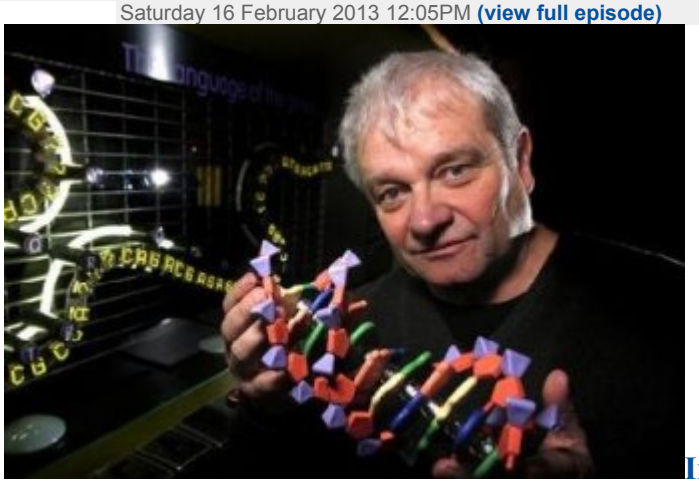

Image: Paul Nurse

Scientific enquiry is concerned with acquiring knowledge and using it for the public good. Deciding what is studied, by whom it is studied, and what is done with the results is the theme of this address by Paul Nurse at The University of Melbourne, January 2013.

Paul Nurse was awarded the Nobel Prize for Medicine or Physiology in 2001 for the discovery of key regulators of the cell cycle. He is half way through a five-year term as President of The Royal Society.

Paul Nurse: Thank you very much. I'm not going to give a scientific talk today, I'm going to give a talk about science policy essentially. And the title of my talk is *Making Science Work*. And there's two aspects I want to consider, and that is how good scientific advice can be given to society, so that's one topic, and the second is how can we make good decisions about what scientific research should be supported for the public good. And I'm using the term 'public good' in the widest possible sense, covering the contributions that science makes to our culture and also the applications of science that benefit society, improving our health, improving our quality of life, securing sustainability and protection of the environment, and driving innovation to support our economy, all of which I put under the 'public good'.

I'm going to start with the topic about making decisions about what scientific research should be supported, and my main focus is going to be on the rather tricky issue of research leading to the applications of science. But it's always important to remember that scientific knowledge leads to a better understanding of ourselves and of the natural world, which is an essential part of our civilisation. And in this respect it's like the humanities.

So we shouldn't simply judge science solely in a utilitarian manner, we should be thinking also about its contributions to culture. This was emphasised very nicely by a quotation from the American physicist Robert Wilson, he was a high energy physicist who was being questioned in the 1960s, I think it was, by the American Congress about the Fermilab particle accelerator which he was setting up. That was the precursor to CERN. And he was asked as to how the Fermilab particle accelerator would help national security. And he was being questioned rather hard on that, and then in the end he answered, 'Actually 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 ourselves, and with using that knowledge for the public good. Francis Bacon, really the first proper philosopher of science and also a civil servant who was Chancellor of England at the beginning of the 17th century, he argued, for example, that 'science improves learning and knowledge that leads to the relief of man's estate'. This argument was reinforced 50 years, 60 years later by Robert Hooke at the birth of the Royal Society of London who emphasised how 'scientific discoveries concerning motion, light, gravity, magnetism and the heavens help to improve shipping, watches, optics and,' I like this one, 'engines for trade and carriage.' So he was linking, clearly, scientific discovery with the use of science.

There's a continuum from discovery science, acquiring new knowledge, through research aimed at translation of scientific knowledge, on to subsequent application and innovation. I prefer to think of this spectrum, this continuum as an interactive ecosystem where knowledge gathered at different parts within the continuum can influencing both upstream in the creation of new discoveries, new knowledge, and downstream in the production of applications. A really good historic example of how investigations downstream (that is applications) can influence research upstream (that's discoveries) was the work on improving the steam engine which greatly informed the subsequent formulation of thermodynamics.

It's important to emphasise that this continuum of science spanning discovery through translation to innovation is truly connected. Investing too heavily in a particular part of that spectrum, or placing artificial barriers in that continuum, or arguing that different parts of that ecosystem are superior to other parts, all of which I've heard argued, in my view should all be rejected. Science throughout that continuum shares the same values, the same skill sets and the same methodologies.

Now, let me begin with what factors should be considered when deciding what scientific research should be supported? What's important there? There are a number of things that are important, but the one I think is absolutely crucial is the scientist who is carrying out that research. Major discoveries in science are usually associated with highly talented individuals, and those individuals combine a number of qualities: they have to have in-depth knowledge, they have to be creative, they have to understand the values of science and how research is done, they have to be well motivated, and they have to be effective in achieving what they set out to do.

In-depth knowledge of an area of science is obviously essential, but I need to add, this should be combined with what some have called a 'peripheral vision', that is an understanding and openness to what other sciences can contribute. So you need in-depth knowledge but you also need to keep an eye on what's outside that particular specialist interest. This is especially important when solution of a research problem needs multi-disciplinary approaches, obviously, and it's also particularly important I think when you get science close to application.

Creativity. 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, writing, and the media. Like other creative workers, scientists thrive on freedom. Organising them is frankly like herding cats. I want to emphasise this freedom bit, because I'm not sure it's given enough emphasis. Freedom of thought, to pursue a line of investigation wherever it may lead, 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. It follows therefore that

similarly societies that are not free, that do not encourage the free exchange of ideas or respect those values of science, cannot be ultimately leading scientific powers, because that freedom is closely connected with the creativity required for good science. And where we see emerging science powers, they are becoming increasingly aware that they have to deal with freedom in the society if they're going to actually encourage science, and that is a point that I don't think we fully appreciate and make enough of.

Scientists need to embrace the values of science, to have respect for reliable and reproducible data, a sceptical approach which challenges orthodoxy, particularly the orthodoxy of an individual scientist's own ideas. In other words you should be your own worst enemy by attacking your own ideas. Scientists abhor the falsification of data or the cherry-picking of data, they have a commitment to pursue 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 can also be important: a desire to undertake public good through the eradication of disease, for example, to make something useful, to create economic wealth, or simply, frankly, to become rich or famous. I really don't have a concern about what the motivation is, but it is important that the motivation is strong, because I've been doing it for 40 years and, I can tell you, it's difficult.

So in deciding what research should be supported, we need to pay attention, a lot of attention, to the scientists carrying out the work, and as far as possible decisions about research projects should be closely associated with the assessments of the individuals proposing those projects. In other words you make a decision linking the person doing it with what they want to do, rather than saying in a rather abstract way we're going to support this activity when it's divorced from the people, because you've no certainty that you actually deliver it.

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 achievement. Those that have recently carried out high quality research are most likely to continue to do so, at least in the short to medium term future. 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. I'm going to repeat that actually: the objective is not simply to reward those who write good quality grant proposals but to actually support those that will carry out good quality research. I repeat it because not all funding agencies understand that. So, much attention should be given to actual performance.

Obviously such an emphasis needs to be tempered in circumstances, for example, when you're dealing with those who have only a limited recent past record, such as early career researcher, or those who have had a break in their careers, or are returning to research after a fallow period. In these cases it's been my experience that one needs to supplement the grant writing with face-to-face interviews, which are very helpful in determining the quality of the researcher making the application. The truth is it allows you to get rid of the bullshit. There are very few who are bullshitting who can cope with a good committee. After five minutes you know they are bullshitting.

The reason I emphasise that is of course it's expensive to have direct interviews and usually when governments and others tell you to cut back office costs, this is exactly what goes.

What they don't realise is that the quality of decision making can be influenced if you cut back office costs. So the greater costs involved will be more than compensated by the greater quality of the decisions made.

So making good decisions about research funding requires a focus on the quality, passion and past performance of the scientist proposing the research. And that's a major message. And, incidentally, it's the sort of approach that very high achieving research funding organisations such as Howard Hughes, for example, absolutely follows. Now, I am a Howard Hughes trustee, so I'm bound to say that, but I think it is the focus on the individuals that actually drives Howard Hughes Medical Institute decisions.

The perennially vexing question is how prescriptive research funding agencies should be in determining what research areas should be supported. It's a recurring issue which arises because of the tensions between the scientists wanting the freedom to decide what they want to pursue, and society which supports science not as a cultural activity, but as a method of increasing knowledge at improving the lot of humankind through achieving specific useful objectives. So there's a tension there.

When funding agencies are faced with this, the usual sort of response is for them to carry out some sort of strategic review, decide priorities, identify research areas judged either as being especially timely for future scientific advances or which reflect particular needs for society. This can lead to initiatives with words like shaping and sponsoring future research, for example, ring-fenced allocations of research funding, and the like. This is often well intentioned, but the point I'm going to argue now is that these approaches can be dangerous and they can run the risk of funding lower quality research, and we have to be completely aware of that.

One problem is that decisions are separated from consideration both of the specific projects and of the scientist carrying out that project. As a consequence of that, such initiatives run the risk of attracting less creative and effective scientists, individuals who may simply follow where resources are being made available. And you see this all the time. You know, what second, third and fourth rate scientists can do…just look, oh well, this area has been funded, let's write a grant that looks good in this sort of area.

A second problem is the identification of the favoured research areas (you know, the horizon-scanning) is usually made by committees made up of people like me, and this is a problem. Because people like me…I sometimes call them the 'silver-backs', that's the gorillas, the ancient gorillas, and they frankly are not always very research active themselves anymore, and such committees are prone to coming up with the absolutely obvious and being behind the cutting edge. And that's a serious problem because I seldom see innovative thoughts coming out of committees of that type. Better judgements are more likely to be made by scientists actually carrying out the specific areas of the research who are much closer to the research problem being pursued. So if you're going to be tempted to go into this area, you have to be really, really careful about the horizon-scanning, and mostly it doesn't work terribly well.

Now, having said that I'm not happy with that, how can this difficult tension be resolved? Well, it is difficult., but there are three issues which may help: the first is the Haldane Principle, the second is to use different approaches when considering programs which are aimed at specific applications and specific goals, and the third is the need for more imaginative scientific leaders. So let's deal with all those three.

The Haldane Principle is usually interpreted as meaning that researchers and not politicians should decide how to spend funds. The reason why I want to bring this to your attention is I think it's useful to extend this view further by arguing more generally that decisions should always be made as close as possible to the researchers actually carrying out the research. The more distant you are from them, the more difficult it is to make those decisions. So those leading research funding bodies should focus their attention on really high level priorities and avoid the temptation to become too prescriptive and too finely grained in recommendations concerning areas of research that need funding.

I want to illustrate this point by a metaphor derived from geographical exploration, and it is in part stimulated by my trip to the Antarctic. In the 19th century, early 20th century, the Royal Geographical Society might make a decision that it wants to support an expedition and it might want to sponsor exploration, for example, of the Amazon basin, the source of the Nile, or even the Antarctic. But it would have been very ill advised to be too fine-grained in its deliberations by specifying which Amazon tributary or African lake or South Polar glacier should be the focus of attention. That should of course be left to the explorer on the ground not those in the committee sitting in 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. Rather than, for example, saying to Scott, well, go to the Antarctic but you must go up the Beardmore Glacier and not this other glacier, and when you get to the Beardmore Glacier you can't go up it.

So, research funders, I think, should behave in the same way. They should put their trust in the explorer scientist carrying out the research and not their committee in London. And as far as possible, research funding decisions should be driven by the scientists carrying out that research.

However, this approach does need modification when a research program is aimed at achieving a specific goal or application because that does require more prescriptive behaviour. A large discovery project, such as the human genome sequencing, or large parts of infrastructure like a new accelerator, that's got to involve a much more top-down approach to those sorts of problems. But it must be said, it is more prevalent when thinking about applications for translation and innovation.

So it's necessary and indeed important to identify sectors when they are close to application as being areas that are worth supporting. This more prescriptive approach applies to all sorts of research close to application, both for-profit activity such as driving the economy when it is mostly discussed, but also not-for-profit activity such as improving health and protecting the environment. They fall in the same area.

Just to emphasise the point I just made, discovery research, like research that involves large datasets such as genome sequences or meteorological data clearly does require coordination top-down. We don't all want to be sequencing the same bit of the human genome, that's just daft. You don't want to have all your thermometers in Melbourne and nothing in Adelaide. So you do have to be a bit more sensible about this.

The third issue is 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 support, the usual way they respond is to ring-fence resources and have an initiative. I think it would

be more useful to undertake a process of education and inspiration by those leaders so that researchers can become motivated to work in that area.

Should the area really be as promising as the research leader thinks, then high quality scientists will be persuaded that this could be something important and that's worth doing. Should it not be so interesting then high quality researchers may be less impressed and are less likely to be engaged? And if that's the case the research leader should perhaps think again as to whether his or her enthusiasm for that is well placed. So I'm arguing that research leaders do need to be proactive, but they actually need to be leaders, they need to inspire and educate, and they shouldn't just immediately resort to ring-fencing and micro management of the research agenda. I think that's a really critical issue because I do not see research leaders being motivational, inspiring and educating, I see them saying, right, this is a problem, let's ring-fence $100 million into that and see what happens. I don't see them getting out there and saying, look, these are the reasons why this is interesting, why don't you put applications in and we'll see how we can respond to that. It's a different way of doing it and I think we need leadership to do that.

Are there any other features concerning decision making with respect to science closer to application? It's more likely to require teamwork, not only covering more scientific disciplines but also activities outside of science, including finance, market analysis and law, as examples. It requires real effort to get individuals from such diverse backgrounds to work well together, and attention needs to be paid for encouraging mutual respect and to breaking down barriers between them.

I've worked in biomedical research for many, many years, and getting laboratory scientists to work well with clinical scientists is really, really difficult, and the reason it's really, really difficult is because we are trained very differently and we have different cultures. Unfortunately we have in place too many barriers, too many silos that inhibit free transfer and even encourage suspicion between the very people that need to be working well together.

One of the problems is that increasing knowledge has led to specialisation, making interactions even between scientists quite difficult, let alone between scientists, industry, the public services and other professions. It was easier to make such contacts in the less complex society at the time of the Industrial Revolution in the 18th century, and I want to give you the example of the Lunar Society which operated in England in the late 18th century. It was made up of chemists, biologists, doctors, industrialists, engineers, entrepreneurs, and social reformers. And they used to meet every month outside the golden triangle of Oxford, Cambridge and London in the Midlands, North of Birmingham, and I think outside the centre of power maybe it would have been another interesting thing.

Anyway, they met together, including intellectuals and entrepreneurs such as James Watt, Josiah Wedgwood and Erasmus Darwin, and it was in this sort of atmosphere that the Industrial Revolution was taking place, and I think we could reproduce it again today. They were called the Lunar Society because they met under the full moon, by the way, because they could ride home at night. If you read about it you get the impression they may have sometimes drunk too much during their deliberations and that the full moon was perhaps required for them even to see the way. But I've sort of been stimulated by that setting up a new institute, and I think we should have a monthly party in the new institute which is in central London where we encourage individuals from other sectors, like lawyers or patent lawyers or venture capitalists or whatever, and we mix the 25- to 35-year-olds together in a monthly lunar cycle driven party. We'll see if that one works.

Much is spoken when we talk about this science of application about the so-called valley of death. This is the gap between the generation of new knowledge and the application of that new knowledge particularly for commercialisation. And the way it's presented is that there is no money to fill that gap, and so discoveries don't actually happen to drive the economy. So usually the focus of discussion is on providing research support to bridge that gap.

I actually think we need to be a bit more sophisticated about this discussion, and I want to continue the metaphor of the valley of death, because I think another issue is that we don't have strong bridgeheads coming out from both sides of that valley of death, and so the gap is too large. This can be a problem when attempts to translate are made too prematurely before knowledge is sufficiently reliable and complete, and I think that particularly applies in the biosciences, by the way, which are very complex.

If you're doing discovery science in a way where you don't really care too much where you're going, what happens is you get a certain amount of research data, you interpret it, you have an idea and you start going in this direction, you get more data and you find that wasn't such a good idea and you get shifted over to here, and then when you get to here you find you're shifted over to here. You're constantly readjusting your position and your direction dependent upon the data and dependent upon the experiments and observations you're making.

Now, if too early in that process you take on a translational objective, you switch off that check and that balance, because what happens is you're now funded to get to here, not funded to make discoveries. And if the work starts telling you that this isn't the direction to go, you become inhibited from properly considering it because you know you have to go in that direction. And this is one of the reasons why we waste so much money on certain translational type objectives, simply because they are set up too early and people are being driven to achieve an objective which they can't achieve, and it's simply not fair. So my metaphor is to build a better bridgehead so you do not attempt to cross the valley of death until you're clear about it.

If I can use a film title, to rush into translation runs the risk of becoming lost in translation. If you know that film, a great film, you know what it is? There's somebody who is making a whiskey advert in Japan and he's being directed by the Japanese director who only speaks Japanese and he's got a translator, and there's this scene in the film where the Japanese guy is speaking for about two minutes non-stop and the translator says, 'Turn your face to the left,' and we have no idea what's been lost in translation.

As well as the bridgehead from discovery research we need a firmer bridgehead on the other side that needs to be extended out, and we do need investment in research in industry not simply for carrying out the research but so they have the capacity in industry that can capture new knowledge from the other side of the valley. Because without research capacity and knowledge in industry it is quite difficult to build back over the valley of death. And that's another issue, because I think that, rightly, industry has thought, well, there's a lot of discovery research out there so we should be taking note of it, but they haven't fully appreciated in many cases that you still need to have the research capacity to capture it. It needs to involve people who can still do research, not people who just read papers, apart from the fact that papers are actually mythical structures about what actually is done, but that's another issue which I won't talk about but could do. So we need bridgeheads.

Let me turn to this term 'impact'. The first point to make here is researchers always want their research to have impact. I do not do research of any kind where I'm attracted to research that

has no impact, they want to have impact, and that could be to increase knowledge, it could be to contribute to culture, it could be to generate societal benefit, it could be to support the economy. The problem really comes when crude metrical applications of impact are made a compulsory part of research funding decisions and assessments.

In my view, the potential impact of research should be clearly identified if it makes sense to do so, but it doesn't always make sense, in fact quite often it doesn't make sense to do so. So to demand a statement in every research proposal or assessment about impact for societal or economic benefit, and then put to that 22% of the score or 27% or whatever these people do in their back offices, is bonkers. It simply results in a unhelpful flights of fantasy of no value, and I know it's true because I write them myself because we're asked to write them, so of course I can write them and of course I'm moderately imaginative and of course I've had three glasses of claret before I do it, but they like it, and yet I know what nonsense it is.

Impact is just one aspect out of a number of factors that need to be considered when assessing a research proposal, and absolutely should be provided when relevant and not at all if irrelevant. And the funding agencies have to recognise that and be nuanced.

Funding high-quality research will produce the knowledge we need for public good, and that includes innovation for the economy. So get it right and then science will play its proper role, but getting it wrong will simply waste money, and I think we waste too much money. And we also lose the great opportunities science can play to improve the lot of humankind.

To state the obvious, high quality scientific advice is dependent upon high quality science. Good science is a reliable way of generating knowledge because of the way that we do good science. It's based on reproducible observation and experiment, taking account of all evidence and not simply cherry-picking data that supports your argument, and recognising that scientific issues are settled by the strength of evidence combined with rational, consistent and objective argument.

Central to science of course is the ability to prove that something is not true, an attribute which distinguishes science from beliefs based on religion and ideologies, which place more emphasis on faith, tradition and opinion and do not easily take on the demonstration that something is believed is not true, whereas that is central in science.

Good scientists should be sceptical, particularly of their own ideas, as I've said. And if an observation or an experimental result does not support a specific idea, then that idea should either be rejected or modified and then tested again. This is standard Karl Popper stuff which many of you will be familiar with.

Sometimes scientific knowledge is rather tentative, and that's especially the case at early stages in an investigation, and it's only after repeated successful testing that knowledge becomes more secure and reliable. I think a part of the problem in the policy sphere is a failure to fully understand this process, that sometimes science is tentative and sometimes it's not. And certainly when we teach science in schools it is usually taught chiselled in granite, you know, and the laws of Newtonian physics, for example, which is how we are taught science, doesn't always apply to what's going to happen with BSE, for example, and that's part of the problem we've got because we're taught science as if it's in granite, yet quite often it isn't. And that can give us problems when scientists are called upon to give advice on issues about which the science is still uncertain, because society often wants clear and simple answers, and sometimes we cannot provide.

Okay, so what do I think is important for scientific advice for policy? Well, the first I think is it should be based on the consensus view of scientists who are expert in the area concerned. That may sound absolutely obvious but I have to say it isn't followed, and sometimes mavericks have too much emphasis. And those experts need to be fully aware of conflicting explanations and of the evidence upon which explanations are based.

I think it's very useful also as a further check to have advice challenged through peer review carried out by other expert scientists, so that a group who are coming to advise can be checked by others, simply because it stops the dynamics of a group distorting the interpretation. But that should be by other expert scientists to ensure the conclusions reached are reliable and secure.

If there is no strong consensus or if the knowledge is still tentative, then these uncertainties need to be reflected in the advice, even if society is demanding certainty. It's what we have to do. It's not always helpful but it's what we have to do.

I think these conclusions so far are relatively uncontroversial. But what makes giving scientific advice more complex is the fact that advice is being used to inform public policy, and the development of policy is not based only on the science but on a wide range of societal considerations and opinions, not all of which are as evidence-based or as rational as science. When the lines between these two become blurred, that is when science starts to go over into the political debate or the other direction, when those lines become blurred the science can become mired in controversy, which is not good either for science or for the development of good public policy.

Given this complexity I want to consider two controversial areas to see what lessons can be learnt about how scientific advice should be given. One controversial area is climate science, particularly contentious here in Australia. Is the world warming, is human activity responsible, how much is it expected to warm in the future?

The consensus view of the majority of expert climate scientists is very clear, that the globe has increased in temperature by around 0.8°C in the last 100 years, that this is largely due to increased greenhouse gas emissions, and these are a consequence, at least in part or a significant way, of human activity, and that a further rise of around 2° or maybe up to 4° can be expected in the next century. That would be the approximate consensus view.

Within this mainstream consensus view there is quite a lot of debate about aspects of the science, and that is a legitimate debate, you know, is it 1.5° or is it 3°, et cetera, and it particularly applies to predicting the future. And it's made difficult because of the complexities of feedbacks within the global climate system. That makes it difficult to come to decisions.

But outside that consensus and outside that proper scientific debate that is occurring within that mainstream there are more extreme opinions. At one end it is argued that there is either no warming taking place or, if it is taking place, then human agency is not important. And at the other end it's argued that global warming will be absolutely catastrophic. That's the outliers.

There are supporters in both of these extreme positions in the public sphere but it is the former arguments, the ones that are more sceptical and denialist, that have gained more

traction, even amongst individuals who normally would actually trust consensus scientific opinion. So why is this the case? What can we learn from this?

A feature of this controversy is that those that deny there is a problem often seem to have political or ideological views that lead them to be unhappy with the actions that would be necessary should global warming be due to human activity. I think that is a crucial point, because these actions are likely to include measures which involve greater concerted world action, curtailing the freedoms of individuals, companies and nations, and curbing some kinds of industrial activity, potentially risking economic growth. These are all critical key issues about which we should be worried.

But what in fact appears to happen is that the concerns at least of some of those worried about these types of actions, have led them to try and convince society by attacking the science of the majority of climate scientists and to use scientific arguments that on the whole are rather weak and unconvincing, and nearly always involve the cherry-picking of data. In other words, what's happened is those who are very concerned about the outcomes and what one would have to do, in trying to make their argument have over-spilled into the science.

We saw that, for example, in Britain with a politician, Nigel Lawson, who would go on the television and talk about the scientific case, and he was trained as a politician; you made whatever case you can to convince the audience. So he would choose two points and say, look, no warming is taking place, knowing that all the other points you chose in the 20 years around it would not support his case, but he was just wanting to win that debate on television. And that is of course over-spilling political views into your science.

Several other features have complicated that situation. One has been a failure of some climate scientists to be as open as they should have been in making all their data available, and this has caused some to argue that the climate scientists are not behaving properly, that their data is wrong or that they are manipulating it. So that hasn't been helpful.

Another feature, as I've already mentioned, is the complexity of climate science which leads to uncertainties in predictions. And this allows space for poorly evidenced but confidently stated opinions, which are sometimes mixed with personal attacks and misrepresentations to attract public and political attention. So there's a bit of an unholy mix in all of this.

So what can we learn from it? Firstly it reinforces the points about the need to rely on the consensus view of expert scientists and the need to avoid cherry-picking of data and argument. But it also emphasises the need to keep science as far as is possible from political, ideological and, for that matter, religious influence. This can be difficult, because after all we're all human, but it is what we have to do, we have to keep the politics out of it.

A second controversial area is genetically modified or GM foods, and that is the introduction of genes by genetic engineering into crop plants. Again, the consensus view of the majority of expert scientists is that in principle this is a safe approach and can lead to benefits, but not only commercial ones but also may allow us to tackle global problems such as sustainability and food production, crop yields, marginal habitats for crop growing, for example.

These scientists would usually argue that you need precautionary checks, but generally these should be similar to those used for conventionally produced crop plants, that is you use a case by case specific crop plant basis to determine safety and effectiveness, that there is nothing different about GM. This consensus view has been accepted by the public in some countries but in others it has not. So, again, why is this the case?

In my view the key features of this controversy that we need to think about are peoples' sensitivities about what they eat, concerns about scientists playing at God, and worries about the influence of over-bearing commercial interests. Interestingly, by the way, those that worry about GM crops are usually the complete opposite from those that deny climate change. I find myself being attacked by all sides of the spectrum as a consequence of this.

Anyway, this approach has led to deep suspicion amongst some of the public about GM foods. Let's start with the fact that human beings have a tendency to be conservative, even fearful about what their food contains, because it's true. I come from a working class family in the UK and I went to Birmingham University and it had many curry shops, so I learned about curry when I was 18. So when I went home I wanted to take my parents out to a curry house. They had never had curry. The deep suspicion and conservatism when I took them for their first curry meal, you could not imagine. So we really are conservative.

One anxiety I noticed when public consultation took place after GM crops was essentially banned from being used, and I was involved in doing surveys about this, was a concern from the public that they didn't want to 'eat food that contains genes'. The reason I say that…I mean, it completely shocked me when I heard this but this was the commonest thing that was said. And this was an issue that a scientist was unlikely to even think about, in fact I'd never thought about it, but frankly it was a perfectly reasonable one for the public to express who wouldn't be very knowledgeable about this.

So when we had the public debate we were wittering on about things that weren't the primary concern and we weren't even aware of some of the concerns that the public had. And this concern was exacerbated by newspaper headlines calling GM crops 'Frankenstein Foods', this was the *Daily Mail*, which I suppose is a newspaper, which conjures up images of white-coated scientists playing God and tampering with the purity of food.

Another feature is often that those who object to GM have political or ideological opinions which really dislike the power yielded by powerful commercial corporations, and they typically quote Monsanto, for example, because they feel that they are trying to manipulate small farmers, the public and so on. But what has happened is that these anti-GM opinions of people who don't like that type of power has been adopted in the way they think about the science too. And it's been adopted by certain environmental NGOs who campaign against the use of GM crops, even when their use is aimed entirely not for commercial benefit but for the public good, such as reducing vitamin deficiency in the developing world in children for example, and such crops have been burned and trashed, even though their aim was simply not commercial.

What can we learn about the public debate concerning GM crops? First, it's clear that there has been a failing on the behalf of the scientists, people like me, to properly engage the public and pay attention to what they say. We scientists have to listen to the public to be completely aware of their concerns and of the questions they want answered by the scientific advice. Scientists and, for that matter, single interest pressure groups are not always the best

individuals to frame or discuss these questions. Single interest pressure groups are not representatives of the public.

Second, is the need for high quality debate in the mass media. Scientists need to be part of this debate from the very beginning to ensure that it's based on evidence and rational argument. That's also important. Thirdly, scientific advice, whenever possible, is best delivered by scientists who are impartial, rather than those who may have other motives. This can be the case for a company trying to promote the use of GM, or an NGO attacking GM who rely on the support of individuals ideologically opposed to such technologies.

An important question is what groups of scientists and scientific bodies can be relied on to give good advice to the public. I think that's actually a critical issue because it's a question of trust between society and science. It's obvious that it's best to involve those who are expert, that is those who have a relevant research track record. It's also useful to engage experienced scientific generalists, that is scientists who understand the attributes of good science and are familiar with science policy issues, because us scientists mostly aren't exposed directly to this very much, so you need both types.

A further extra helpful check is to have expert scientists who peer review original advice, but separate up to that point. The corollary is also true, that those who are not expert and who cannot properly assess the relevant specialist evidence and argument are not likely to be appropriate, although you will find that those who write columns for newspapers nearly always fall in that category. Not journalists, by the way, I'm not knocking the scientific journalists, I'm knocking the ones who write commentaries.

Scientists giving advice need to be open and impartial, they shouldn't cherry-pick data and argument. They need to explain the range of possibilities with assessments of the probabilities, and also they need to explain it in a way that the public can understand.

A range of different bodies offer scientific advice on policy issues. What are the characteristics of those bodies, therefore, that should be trusted? Well, the first thing to say is it's always useful to look at what everybody says, because even those that you don't really trust may have something useful to say, so you should actually look at what everybody has to say. However, some types of bodies are much more likely to be more reliable at giving scientific advice. The characteristics I think we should look for are: are they broadly based, are they impartial, do they understand the methods and values of science, do they respect openness, do they carry out proper peer review?

More specialist organisations with specific objectives such as lobbying groups, a company or an NGO, may find it more difficult to be impartial. In some cases, scientific advice is offered by more shadowy organisations who do not want to declare where their support comes from for their policy work. They are much more likely to be acting as lobby groups without revealing for whom or for what they are lobbying, and frankly should not be relied on for giving impartial scientific advice.

Similarly, organisations that are bombastic, or resort to personal attacks or misrepresentations are likely to be resorting to such tactics because they have lost the scientific argument, and they're just using the tricks of the political trade. So their scientific advice should be treated with great caution. And there are quite a few bodies out there pretending to be scientific bodies in some sense but basically lobbyist groups.

So in summary, what is good practice for the provision of scientific advice? It should be based on the totality of observation and experiment, rational argument, the consensus view of experts, rigorously peer reviewed. If there is no strong consensus or if it's still tentative, these uncertainties should be reflected , and it has to be kept separate from political, ideological and religious influence. It requires good public engagement to make sure public concerns are taken account of and the scientific questions are framed correctly.

Scientists also need to be involved in public debate from the beginning, the outset of an argument. And finally, scientific bodies who can be trusted to give advice should also be broadly based, also be impartial, also understand science and be completely open about income, sources of income and the conflicts of interest in their policy work, attributes generally that apply to national science academies such as the Australian Academy or, for that matter, the Royal Society, which incidentally has been providing scientific advice to society for 350 years. Thank you very much.