



Daniel Sarewitz

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When All Scientists Are Activists

I would like to explore in a bit of a Socratic manner some aspects of the complex relations between science and policy that I think are not adequately accounted for in our efforts to deal with the wide range of challenges facing society today. You know the list; climate change and energy production, managing chemicals in the environment, protecting ecosystems, providing for public health and adequate food supplies and so on. For purposes of my contribution the commonality among these sorts of challenges is that they're complex systems problems, and they bring science and politics together in ways that often seem difficult to manage and uncomfortable.

I'll begin with a look at science and political advocacy. I'm not going to say too much about science advice per se but I think advocacy is a big part of that story, and what I'm going to argue is that when science and politics interact, scientists who hope to contribute productively to public discussion cannot avoid taking a political role; that it's an occupational hazard and a condition of the reality of both science and politics. In addition, I want to suggest that this problem can't be overcome by efforts to reduce scientific uncertainty through more research or efforts to increase our understanding of the science that underlies the political disagreements. In fact, I'm going to argue that our expectations of what science can accomplish in the political realm is often part of the reason why we have these difficulties at the intersection of science and politics to begin with.

I'm very sorry, but I'm being interrupted by a call on my cell phone. I know this is rude to do in the middle of an invited talk, but you know the new ethics of social media:

Sarewitz: Hello?

Journalist: Professor Sarewitz, I'm sorry to interrupt you in the middle of your presentation but I'm a journalist and I'm calling with some questions about the findings from your recent field research. Do you have time right now to respond?

Sarewitz: This is rather awkward. Look, can we get it over quickly please?

Journalist: Would you briefly describe your latest results?

Excuse me for a minute. This has to do with some geologic fieldwork that I'm doing.

Sarewitz: What I discovered was that a major sedimentary rock unit in my study area contains a major unconformity. This represents over 12 million years of erosion and non-deposition in my field area. In short, what we thought had been a continuous sequence of sedimentary deposition is in fact two separate units.

Journalist: Can you explain what you're talking about?

Sarewitz: I thought I just explained it. The idea is that we have these two rock units. They're both shales, okay. They're both rocks that were originally deposited as mud in a marine environment. They look quite similar, but if you analyse the fossil content of these units you discover the unit lying to the east of the river contains fossils of late Jurassic age whereas those to the west are early Cretaceous, more than 12 million years younger. So there's almost certainly a long period where the older rocks were actually above sea level and underwent considerable erosion and it turns out that the sedimentary history of the region is much more complicated than we ever imagined.

Journalist: And your professional colleagues all accept your findings?

Sarewitz: I presented them at professional meetings. I published them in a peer review journal, so for the most part they do.

Journalist: For the most part?

Sarewitz: Well the scientist who did the original work feels that the paleontological analysis is open to interpretation. He thinks the change in the fossil types is actually an environmental change and not a change in time.

Journalist: So how can this disagreement be resolved?

Sarewitz: More research, obviously. In fact, I'm writing a grant right now to fund the work. Can we wrap this up please? I have people here who are expecting me to be giving a presentation.

Journalist: Yes, you have been very helpful. Thanks for taking the time to talk. Good-bye.

Sarewitz: Yes, good bye.

Sorry, but in some ways this provides a starting place for the discussion because you could all hear quite clearly that I was just doing my best to explain my research in a manner that might be accessible to non-experts. There wasn't any question of advocacy here, right? And I suppose on some level one could say I was advocating my intellectual position, which I think is quite well-supported by the evidence that I've collected. I've worked in that field area for years after all. I suppose I'm also advocating for more research in order to fully resolve the remaining

uncertainty and disagreement but this is what science is about, right? It's about testing a hypothesis, seeking additional evidence to confirm or refute it and if that counts as advocacy, then the more of it the better I think. That's something we can all agree with. Wait a minute – sorry, that journalist seems to be calling again.

Sarewitz: Hello. Is this really necessary?

Journalist: Yes, I'm sorry but in my haste to finish the call I forgot to ask you the most important thing.

Sarewitz: Alright, go ahead.

Journalist: Well the reason I contacted you to begin with is that your field area is at the centre of some political disagreement because it contains the proposed site of a new nuclear power plant.

Sarewitz: Well I wasn't aware of that. I stick to science, not to politics.

Journalist: Okay, but you're an expert on the geology of the area so what would you think about siting a reactor around these rocks? Is this a safe site? So you can't comment on whether ...

Sarewitz: I've absolutely no idea. That's far beyond my area of expertise. I just look at the rocks. You need to talk to a civil engineer or a nuclear engineer.

Journalist: So you can't comment on whether the rocks are stable or appropriate for siting a reactor.

Sarewitz: Of course I can't.

Journalist: But are you aware that Dr. Tobias, who is also a geologist, says that he has identified a fault that cuts across the region and that the site is not safe?

Sarewitz: A fault? Dr. Tobias says that? Well I don't know what he could be talking about.

Journalist: Well, more specifically, he has quoted work in your recent report and he says that the 12 million year gap you identified is really an earthquake fault.

Sarewitz: That's simply ridiculous. It's an unconformity. I put my hand on it.

Journalist: Can I quote you on that?

Sarewitz: I guess so. Can I get back to my presentation now please?

Journalist: Yes, thank you again. Good-bye.

Alright, so enough of that. Political disagreements and policy choices always call upon our beliefs about how the world really works or how we think it works as a foundation for making claims about what ought to be done and how best to do it. So our »oughts,« that is, our normative beliefs are often justified on the basis of our »is's,« that is, our factual beliefs. To resolve disagreements and make good choices, we need our beliefs about the world to conform to what science and scientists have learned about it. We need reliable knowledge about the state of the world, about the geologic setting for a future nuclear reactor, for example, and so we want our science served up straight, unadorned by political biases or hidden agendas. We want expertise, not advocacy. This much would seem obvious, so why is it so difficult to achieve?

In the first of my unfortunately timed interviews with that pesky journalist, I think we can all agree that political advocacy had no place to enter the conversation, but I have to confess that in the second conversation towards the end things seemed to be getting a little bit out of control in unexpected ways that made me a bit uncomfortable. What changed between the first and the second phone calls? Certainly, I didn't change. I kept a focus on the science and on the limits of my expertise, but the context of my scientific results changed in a way that was surprising and, as I say, difficult to control. Up until now, my results were accepted by most of my community, my community of peers who are interested in the sorts of problems that occupy me. Yes, there was some technical disagreement as I've discussed, but we could, in my opinion, have easily generated a testable hypothesis and done the necessary research to resolve the uncertainties. Everyone who cared about the scientific question, which is admittedly a small rarefied group of experts, would have been satisfied and the facts of the matter would have been settled.

Then the journalist started talking about nuclear reactors and faults. Now, I suppose at the end of the phone call I could simply have said that I didn't want to be quoted; that I had no interest in getting involved in the politics. But I know more about these rocks than anyone else. So if I don't contribute my expertise, how will people understand the science? I was really clear that my expertise didn't extend to the actual problem to siting a power plant. I stuck to what I knew. Should I really let Dr. Tobias, a scientist who works for an environmental advocacy group, set the terms of a debate about a problem that I've been studying for many years? Something about what it means to be a scientist and what it means to make a factual claim seems to change as soon as a problem goes from narrowly scientific to more broadly societal. In the little thought experiment that I've subjected you to, two things happened to change the context.

The first was that a policy choice arose that made new demands, societal demands on the science. Can we safely site a reactor in a particular

location? The second thing to happen was that this new context brought more scientific interest to bear on the question. Now, we have Dr. Tobias, the geologist who works for the environmental group, butting in with his interpretation even though he never had any scientific interest in this area before. In the narrowly scientific case, scientific legitimacy and validity are matters for the scientific community alone to establish, but once the problem becomes one of political or policy choice, of options over which people may disagree, the science begins to look different.

In my thought experiment, my understanding of the geology of the area would likely be quite appealing to those who favour building a new nuclear reactor, perhaps this makes me uncomfortable because I'm not a big supporter of nuclear energy – but I need to be totally honest and objective about my understanding of the science, right? I want to advocate for the science, not for a particular political position, but the situation also makes me uncomfortable because suddenly my scientific knowledge is a political tool. It cannot be separated from the case that will be made in support of a political choice about the nuclear reactor. Thus, I'm a political actor and my knowledge is political knowledge.

When science gets caught up in political and policy disputes, suddenly the complexity and uncertainty of the world that science tries to understand becomes a political problem as well as a scientific one. This was well-understood by the Nobel Prize winning social scientist Herbert Simon, who observed in 1983: »When an issue becomes highly controversial, when it's surrounded by uncertainties and conflicting values then expertness is very hard to come by and it's no longer easy to legitimate the experts. In these circumstances we find there are experts for the affirmative and experts for the negative« (Simon 1983: 94). This situation gives rise to what may seem like a very surprising conclusion, because we tell ourselves the reason we want science to inform our decisions is so we make the best possible decisions, but in political or policy debates the demand for and the authority of scientists doesn't derive from their capacity to articulate what's known and agreed upon. Rather, scientists are very often valued on the basis of their ability to provide support for one particular fact-based interpretation of the world rather than another.

Now, we might like to think that we can help to avoid this problem by looking for scientists who are unbiased, who are advocates for the science, not for the politics. For example, I hope I've convinced you that my own involvement in the discussion of the nuclear reactor is entirely without political motive and thus I'm lacking in political bias. Certainly, the same cannot be said about Dr. Tobias who works for the environmental group and is a known opponent of nuclear power. Thus, it might seem natural to trust my scientific conclusions rather than those of Dr. Tobias, but perhaps things aren't so simple.

So let's listen in on a conversation between Dr. Tobias and our persistent journalist and see what he has to say for himself.

Journalist: Dr. Tobias, you have reportedly identified an earthquake fault running through the proposed site for a new nuclear reactor.

Dr. Tobias: That's correct.

Journalist: And, as you know, Professor Sarewitz of Arizona State University disagrees with this view. He says that what you identify as a fault is really a – where is that word? – it's here in my notes somewhere. Ah, here it is: an unconformity.

Dr. Tobias: Professor Sarewitz is a fine sedimentologist. I have only the highest regard for his research, but I disagree with his conclusions. Our paleo-magnetic and gravity data indicate a significant change in the thickness of the shales across the area in question, which almost certainly means there has been uplift on east side relative to the west. Moreover, investigation of micro-fabrics near the transition from one unit to the other shows a distinctive sheer fabric. The transition is a fault.

Let me break in here for a moment. One thing to notice about this exchange is that the person with whom I disagree about this interpretation, Dr. Tobias, is speaking a completely different disciplinary language than I'm speaking. He's mobilising a completely different type of evidence, and he claims there's a fault.

Journalist: Is the fault a dangerous one?

Dr. Tobias: We don't know yet. We need to do more research. It doesn't seem to have ruptured in recent times, but that doesn't mean it couldn't still be active.

Journalist: So you would oppose siting a reactor in this area?

Of course he'll oppose it. He's against nuclear power!

Dr. Tobias: Of course. At least until we have done a lot more research to establish that the region is seismically safe.

Journalist: Dr. Tobias, you work for a well-known environmental group that has long been opposed to nuclear power. Doesn't that colour your scientific assessment of the situation?

Let's see how he gets out of this one.

Dr. Tobias: On the contrary, it makes me more aware of the potential risks.

Journalist: But Professor Sarewitz is employed by a university, his work has been published in a peer review journal and his salary does not

depend on reaching any particular research result. Why should we not trust his view of things more than yours?

Dr. Tobias: I put it differently. Because I work for an environmental group I need to consider all of the possibilities when I'm evaluating a potential reactor site. Professor Sarewitz is a disciplinary scholar and he's not forced to explore as many alternative hypotheses as I am. I'd say that makes my science more robust than his.

Journalist: But your organisation is on record as being opposed to nuclear energy. How do I know that you are not just shopping around for hypotheses and evidence to fit your pre-existing anti-nuke conceptions?

Dr. Tobias: That's backwards. I'm concerned about reactor safety, so I must consider all the possibilities. This means that my science has to be both more careful and more imaginative because there is much more at stake. Professor Sarewitz is a sedimentologist and he came up with a sedimentological interpretation of the geology at the reactor site. He published his work in a sedimentological journal, but I must work in the real world where there are no disciplinary boundaries.

In the conventional view of things, we would recognize Dr. Tobias as an unalloyed advocate, and if we had to choose between his scientific claims and those of an academic scientist with no apparent political agenda, we'd go for the disinterested scholar, which in this case would be me. But Dr. Tobias' argument obviously has merit. If he has a political commitment to opposing nuclear power, this commitment may also motivate him to explore more ideas, mobilise more methods, scrutinise data more close and so on just as he claims. Nor is it the case that an academic scientist can escape from bias just because he or she lacks a political motive or interest.

To begin with, every scientist must understand the world through a particular set of methods, theories and assumptions that create no more than a partial view of the world. As Dr. Tobias explains, these distinct lenses may have nothing directly to do with politics, but they're nonetheless a source of bias and one that is much less likely to be subject to open examination or discussion than the more explicit sources of bias like ideology or financial self-interest. Unexamined biases may include those provided by the analytical tools of particular disciplines or self-disciplines, and by particular theoretical or methodological approaches, for example, the types of statistical tests one chooses to use. Biases may derive from the desire to achieve a publishable conclusion, or by a concern that one's professional reputation would be tarnished if one is contradicted, and so on.

Thus, for example, Dr. Tobias' opposition to nuclear power might bias him toward over-valuing data that suggests that the site is unsafe. My

disciplinary orientation might bias me toward overvaluing data that's consistent with the theories and methods that ground my understanding and allow me to gain professional recognition. Put somewhat differently, every expert is in some way biased because the act of being sentient in the real world is the act of acquiring bias. Bias, from this perspective, is an ugly synonym for the inescapable incompleteness of our knowledge in a world that's too complex to ever be fully seen, let alone understood.

To underscore this point let me talk for a minute about a completely different area of research that would seem to be much more insulated from political ideology and pressure. Starting in the late 1990s, researchers in the US and the United Kingdom began to uncover evidence of systematic positive bias in clinical medical trials funded by the pharmaceutical industry. Now, this seems like an obvious problem of bias due to conflicts of financial interest. It hardly seems surprising that companies that were not only paying for the clinical studies but also stood to benefit from positive results would reveal persistent positive bias. The problem wasn't with the science, but with the poison of the profit motive. Closer examination, however, began to reveal that systematic positive bias in medical science was also turning up in the academic laboratory, far upstream from the clinical application and direct links to a profit motive.

A now famous 2005 paper by John Ioannidis called »Why Most Published Research Findings Are False« identified numerous causes of systematic positive bias in biomedical science (Ioannidis 2005). Subsequently, there has been a growing tide of studies that back up his concerns. A particularly alarming example came to light in 2012 when Amgen, a biotechnology firm in California, reported that their scientists had attempted to reproduce the results of 53 published »landmark« academic studies in pre-clinical cancer research, and were only able to confirm six of them – an astonishing result that suggests that something is seriously amiss with the bioscience research enterprise (Begley/Ellis 2012).

Like iron filings in a magnetic field, the multiple potential sources of bias in biomedical science are being aligned strongly in the same positive direction by powerful cultural forces, such as the shared belief that the essence of science itself is to move positively away from ignorance and towards knowledge. Such movement makes science valuable and unique as a system of knowledge creation and a source of progress in the world. Scientists are intellectually and professionally rewarded for positive results; science administrators are empowered; and the public desire for a better world is answered and in turn leads to more support for science.

The lack of incentives across the scientific enterprise to report negative results, to replicate specific experiments, or to fully acknowledge inconsistencies, ambiguities and uncertainties in research results, is well appreciated among some in the scientific community, and often discussed

informally »around the water cooler,« but extraordinarily difficult to change. The problem manifests at every stage of the research process from formulating appropriate testable hypotheses to designing and conducting sufficiently rigorous experiments to ensuring valid analysis of results. Now, my point here is not to be critical of science. Developing reliable and useful understandings of complex systems, whether the human body or the earth's coupled ocean-atmosphere system, is really hard. However, I raise the example of biomedical science simply to underscore my point that even when a field of science is not caught up in political controversy, the complexity of reality and the incompleteness of our knowledge makes bias an inescapable element of the scientific process, as difficult to see as water to a fish.

Thus, maybe when thinking about problems of science advice and policy, what we really want to know is not if an expert is biased, but if the expert is intellectually honest to the extent possible, if he or she is sincere. Here, of course, it is impossible ever to be sure. The common suspicion might be that the academic scientist is more likely to be sincere than the scientist who has an ideological or financial interest in the outcome of political decisions. Attacking the sincerity of an expert with whom one disagrees is a common tactic in political debate. We see it in climate change, in the debate over genetically modified foods, nuclear power and waste disposal, chemical exposure risks, clinical trials and lots of other zones of intersecting science and politics. These issues are suffused with sources of bias that invite questions about the sincerity of the experts with whom one disagrees. But in my view and what I'm going to try to convince you of is that this instinct dangerously over-simplifies the sources of error in complex decision processes, especially insofar as it ignores the inevitability of bias and the impossibility of complete understanding.

Undoubtedly there are insincere and intellectually dishonest scientists but even if all scientists always acted from pure motives, our problem doesn't go away. As the biomedical science example shows, institutional cultures can powerfully influence scientific judgment in ways that scientists themselves may not perceive and, of course, there are always uncertainties over which scientists may legitimately disagree. But even if such institutional factors and scientific uncertainties could be magically resolved for any given particular disagreement, we still have a deeper problem that can't be evaded. So, maybe I'm right about the interpretation of the geology near the proposed nuclear power plant, but then what about the integrity of the reactor design, or the problem of disposing of radioactive waste or the danger of terrorist attacks on reactor sites or the safety of transporting the fuel or the risk of weapons proliferation? Each of these additional concerns is related to the nuclear siting problem, and each provides more space for competing disciplinary

perspectives, factual claims, uncertainties and disagreements about what science is really telling us.

This whole messy situation is nicely captured by the notion of »post-normal science,« which was first articulated by Silvio Funtowicz and Jerome Ravetz in the early 1990s and which provides a useful theoretical framing for thinking through problems of science and politics (Funtowicz/Ravetz 1990). Science is post-normal, Funtowicz and Ravetz say, when values are contested, facts are uncertain, stakes are high, and action is urgent – attributes that characterise problems such as climate change, genetically modified foods, nuclear waste disposal and so on. What changed between the first and second phone call that you were forced to listen in on? What changed is that my science moved from the domain of normal to that of the post-normal – a transformation that neither scientists nor politicians want to acknowledge even exists. The special authority of science and scientists in the political world depends on the claim that the science they do is normal in the sense that Thomas Kuhn articulated, in that it's an enterprise delivering increasingly reliable insight into a world understood through stable scientific paradigms of theory and method (Kuhn 1962).

But in the realm of post-normal science no expert can be in possession of anything close to complete understanding of the decision context, of the potentially relevant facts, values and uncertainties. When the science is post-normal any particular partial view of things may have very different implications for actions, options and values than any other partial view of things. Multiple competing »oughts« can be derived from multiple scientifically reasonable »is's.« This is not a failing of science or of scientists. It's a condition of the complexity of reality, of the limits of cognition and science and the diversity of values and beliefs that human beings hold.

Our normal science instincts tell us that if we were to effectively deal with complex mixes of science and politics, we have to do two things. We have to try to ensure that the science and the advocacy are kept as far apart from one another as possible and we have to do more research in order to reduce uncertainty about the behaviour of complex systems relevant to the problem. But when our problems push us into the domain of post-normal science, these instincts are likely to get us into more trouble, not less, and we need to think differently about how we arrange and conduct both our sciences and our politics.

Thus, to explore this concept, I want to briefly contrast two distinctive approaches, one taken by the United States and the other taken in Sweden, to managing science and advocacy related to nuclear waste disposal – a complex politically controversial post-normal science issue if

there ever was one.¹ Now the US approach to disposing of high-level nuclear waste hinges on a particular political decision. After preliminary scientific assessment of multiple possible repository sites across America, the US Congress in 1987 selected the lonely desert locale at Yucca Mountain, Nevada, near the nuclear test sites used during the Cold War.

Having selected Yucca Mountain from among a variety of competing possible sites, Congress then tasked our Department of Energy to undertake scientific characterisation of the proposed waste site and its risks to determine whether or not Yucca Mountain should be certified to become the nation's repository. The idea was that preliminary investigations demonstrated Yucca Mountain to be a reasonable choice, and now scientists would continue to do research to ascertain with greater certainty and comprehensiveness whether the site would be able to safely isolate high-level nuclear waste from the surrounding environment for the next ten millennia or so. The research involved an assessment of a variety of possible risks to the safety of the site, for example, from groundwater contamination, from corrosion of the waste containment vessels, from volcanic activity, from earthquakes and so on.

The plan was for the growth of scientific knowledge to reduce uncertainty about the behaviour of the site and of the waste stored at the site into the distant future. To this end, more than ten billion dollars was spent over two decades studying the site, making the Yucca Mountain area probably the most intensively studied piece of real estate on the entire planet. From the beginning, however, there was an intense opposition to the Yucca Mountain site, especially from the state of Nevada but also from environmental and other anti-nuclear groups. Controversy has mostly been framed by competing scientific claims about the long-term safety of the site. Scientific disagreement has been managed through formal adversarial processes, for example, through legislative action, through legal action, through administrative actions in state and national bureaucracies, but it has unavoidably as well spilled out into the more informal adversarial world of the media in local, state and national politics. Highly qualified scientific experts – advocates – have been mobilised both for and against, exactly in the manner described by Herb Simon.

Nonetheless, in 2008 the administration of President George W. Bush decided that the evidence of the site's adequacy was sufficient to justify proceeding with the creation of the site, and the same year it submitted a 9,000-page application to the US Nuclear Regulatory Commission asking for a licence to begin construction of the repository. Meanwhile the State of Nevada continued to lead the opposition, mobilizing its own

1 For additional background on these cases, see, for example: Metlay (2000) Sarewitz (2009) Metlay/Sarewitz (2012).

scientific experts to argue that the hydrological and tectonic setting of Yucca Mountain is too uncertain to guarantee safety.

The politics around this science changed entirely with the election of President Barack Obama who had pledged during his presidential campaign that he would close down the Yucca Mountain site. This position was largely a political one. Both as a candidate for election and as a newly elected president, Obama depended on the support of a powerful politician from Nevada, the Senate majority leader, Senator Harry Reid. Obama depended on Reid's support both to help deliver Nevada during the presidential election of 2008 and then to move the President's agenda through Congress after the election. Science that had been good enough to support the certification of the site under President Bush was no longer good enough to justify selecting the site under President Obama.

The idea that good science ought to be the principal factor in choosing a nuclear waste site seems sensible, and Sweden initially adopted an approach similar to that of the US with the selection by the national government of several candidate locations for assessment based on criteria of technical suitability. As in the US, local political opposition to all sites quickly arose and this is where the Swedish approach took a different path. Rather than narrowing its options and looking toward science to reduce uncertainty and guide the decision process, Sweden instead conducted a nationwide search for communities that would be willing to host a site investigation and potentially the repository itself. There were both economic and civic benefits to be gained, and several communities volunteered. By the year 2000, the number of candidate sites was gradually narrowed down to three on the basis of technical criteria, one of which subsequently removed itself from the selection process due to popular opposition.

The two finalist municipalities in this rather unconventional competition were to share a 240 million dollar reward – three quarters of which would go to the *loser*. In 2009, the Municipality of Östhammar was selected as the repository site and as of now it looks like this decision process will lead to the actual construction of a waste repository.

The difference between the US and the Swedish experience here can be understood in significant part as a difference in the way that science advocacy was managed. The US process looked toward science to adjudicate in a variety of adversarial settings the political disagreement that the site selection process raised. Scientific risk assessments were at the centre of endless political and legal debates about the suitability of Yucca Mountain. In Sweden, on the other hand, the approach diffused the need for adversarial science advocacy by starting with the communities that were in favour of being considered as repository sites. The disproportionate award that was offered to the «loser» in the competition created

a counter-balance to pro-repository biases that would be created by the volunteer approach to the site selection process.

The characteristics of post-normal science were managed not by trying to reduce scientific uncertainty by doing more research or by adjudicating among various science advocates, but by creating a political context that did not demand that science would resolve political disagreement. Once the politics was dealt with, good old normal science was good enough science.

I will briefly describe a second example from a very different domain, the management of toxic chemicals. In the US, regulation of toxic chemicals is quite reasonably supposed to be based on science, principally on epidemiological and animal model approaches to assessing risk to health. In the US, the result of such approaches has been an intractable morass of litigation, politics, science and uncertainty. Debates over how best to regulate some chemicals such as dioxins or formaldehyde have dragged on for decades while the backlog of unevaluated substances that keep entering the environment continues to grow. Under the principal US statute for regulating chemicals, the Toxic Substance Control Act of 1976, over 84,000 chemicals have been inventoried, and five of these have been regulated to limit their production (Congressional Research Service 2008). While perhaps this tiny ratio is a testament to the safety of our vast assortment of chemical products, most believe it is a testament to the failure of the regulatory regime.

The current system pits those who have an interest in using a particular chemical, which is mostly industry and its allies, against those who have an interest in getting rid of the chemical, which are mostly environmental groups and their allies. Technical experts (and their lawyers) line up on either side and the often fuzzy demarcation between science and politics seems to vanish entirely.

The US State of Massachusetts chose a different and very innovative path that I want to briefly describe. Rather than imposing a standard science-based regulatory process, the 1989 Massachusetts Toxic Use Reduction Act required firms only to inventory their use of toxic chemicals and develop a plan for reducing their use. An independent, university-based organisation called the Toxic Use Reduction Institute was created not to conduct risk assessments, or reduce uncertainty about the health effects of toxic chemicals, but to work with firms to figure out how to reduce their use of toxics, especially by replacing them with safer substances.

At the core of this whole strategy was a different role for science, not to reduce uncertainties about risk or explain causal mechanisms, but to offer solutions by demonstrating that a socially and economically valuable function served by a toxic chemical can be equally well-served by a less toxic or non-toxic chemical. As with the Swedish nuclear waste

case, the approach turns the standard model on its head by using the politics to get the right kind of science rather than expecting the science to solve the politics. The science necessary to support the Massachusetts approach, by the way, doesn't lead to many high prestige publications. It doesn't lead to huge government grants or discoveries that make scientists famous (or rich). It doesn't, therefore, appeal to the culture or incentives that motivate the mainstream scientific community and its sponsors in universities and government. It did, however, help participating Massachusetts firms to reduce toxic chemical use by 40 per cent and chemical waste by 70 per cent between 1990 and 2005 (Sarewitz et al. 2011).

Let me draw these ideas to their conclusion. Our societies face increasingly difficult problems of managing highly complex systems that bring nature, technology and politics together in uncomfortable ways. While several decades of research in cognitive science and behavioural economics and decision science, anthropology and related fields, continue to make clear that humans do not behave according to simple models of rationality and utility maximisation, we nonetheless seem to expect that the best path to progress lies in pursuing greater understanding of the causal structures that apparently underlie these diverse problems. That is, we continue to confront problems of post-normal science with the tools and institutions and expectations of normal science.

This faith, a naive faith that science can cure us of politics, is strongly reinforced by the built-in cultures of powerful institutions such as universities and political bodies. It is also reinforced by the trajectories of scientific and technological advance, for example massive increases in our capacity to gather, monitor, and analyse data about various systems ranging from the oceans and the atmosphere to the structure of the brain and the behaviour of consumers and firms. Despite the continued political gridlock around so many of the issues that I've mentioned, the promise of normal science continues to boost our confidence that we can develop comprehensive explanatory and even predictive models of complex phenomena as a basis for decision-making.

Our scientific institutions, of course, value and reward work on such problems at the cutting edge of the knowledge frontier, and our political institutions are happy to encourage this confidence because, of course, it's easier to wait around for more information than to make decisions that might be unpopular or difficult. Politicians and policy-makers are also happy to encourage the notion that there is such a thing as science pure and unadulterated by politics because that provides them with the source of allegedly unbiased authority, which they can then use to advance political claims.

I've sketched two brief illustrations of a different way to think through the difficulties of complex problems where the science-politics boundary

becomes difficult to manage or even recognise. The process of selecting a site for a nuclear waste repository seems to be working in Sweden because it's based on the understanding that the political conditions that influence the selection are as important as the scientific characterisations. As counter-intuitive as it may seem, getting the politics right first actually makes it easier to get the right science for addressing the problem. In the case of reducing toxic chemicals, the Massachusetts strategy starts with the insight that chemicals, even poisonous ones, provide function and value that people depend on for many reasons. Seeking to ban chemicals motivates those people to oppose action on scientific and political grounds, but seeking to replace dangerous chemicals with safer ones that are functionally equivalent lowers the political stakes of taking action and in key ways actually lowers the expectation for science and thus defines a new path to progress.

In these cases knowledge and technology are not expected to resolve conflicting values and eliminate deep uncertainties, and scientists aren't expected to act as other-worldly paragons of objective detachment. Instead, the creative restructuring of the political context has itself reduced the complexity and uncertainty around action, and thus reduced the political stakes of taking action by opening up alternative pathways to action. These pathways can bring together political actors and interests that had previously been opposed to each other and had used science as the language of opposition.

One key element of pursuing these alternative pathways is to recognise the limits of normal science for taming political divisiveness, and this means – and this is the difficult part – accepting that science in the post-normal context is a different thing than science in the lab or the field or the textbook or the professional meeting. Different not because the practice of science is itself different, but because the context within which it is practiced and communicated and deployed *makes* it different, makes it unavoidably political, and makes activists out of its practitioners. Perhaps if we think about the difference between a soldier in training and a soldier in combat, or a young person at home at the dinner table with his parents versus wandering around lost in a strange city, we can get some feeling for this difference. There's nothing »wrong« with post-normal science, and I've tried to explain why there's no escaping it, but in denying it and insisting that it is not so, or somehow wrong, we ask more from science and scientists than they can hope to deliver. By being open and honest about the true nature of science in the post-normal political realities of our complex world, we can open pathways to more creative and inclusive solutions to problems that have seemed stuck in the quagmire of political disagreement and scientific uncertainty, a result that might even help to protect and improve the social value of the scientific and political institutions upon which our societies depend.

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