Science Advocacy is an Institutional Issue, Not an Individual One

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Advocacy is a condition not of the behavior of individual scientists, but of the political and institutional context for science. Advocacy in science becomes an issue when there is *both* uncertainty about facts *and* disagreement about values. Under such circumstances, scientists become different not because they have changed, but because the context in which they operate has changed.

Dialogue I

Mr A.: Professor Sloat, can you report on the findings from your recent field research?

Professor Sloat: Gladly. In short, I discovered a marvelous unconformity separating two apparently similar sedimentary units, units that turn out to be separated by over 10 million years of erosion and non-deposition. Previously they had been thought to be a single unit.

Mr. A.: How fascinating. Can you explain what you're talking about?

Professor Sloat: Well the idea is that we have these two rock units, both are shales—rocks that were originally deposited as mud in a marine environment. They look quite similar, but if you analyze the fossil content of the units, you discover that 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 10 million years younger.

Mr. A.: And why is this significant?

Professor Sloat: Well, previously we had believed that this sequence of shales was deposited more or less continuously in a shallow sea over 30 million years. Now we realize that there is a gap of millions of years—most likely, sediments were deposited during that interval and later eroded above sea level. So the sedimentary history of our region is much more complex than we had imagined.

Mr. A.: And have you presented these findings to your colleagues?

Professor Sloat: Yes, they've just come out in the peer reviewed literature; previously I had presented them at a professional meeting.

Mr. A.: So your colleagues all accept your conclusions.

Professor Sloat: For the most part, but there is still some disagreement.

Mr. A.: Why is that?

Professor Sloat: Well, the scientist who did the original work on these rock units feels that the paleontological analysis is open to interpretation—that the change in fossil types across the boundary might represent environmental, rather than temporal, variation. But my paleontologists assure me this is not plausible.

Mr. A.: Your paleontologists?

Professor Sloat: Yes, my collaborators who identified the fossils in each unit.

Mr. A.: So you didn't do that yourself?

Professor Sloat: Of course not, I'm a sedimentologist, not a paleontologist.

Mr. A.: So how can this be resolved.

Professor Sloat: My next research grant will address the problem.

In this dialogue, the issue of advocacy is unproblematic and apparently trivial. Professor Sloat is advocating for her intellectual position, and she is advocating that more research is necessary to resolve a purely scientific disagreement, but both sorts of advocacy are viewed as appropriate to her role as a scientist. (A subsidiary point is that Professor Sloat's scientific conclusion—and advocacy on its behalf—also depends on her trust in the supporting work of other scientists, but of course this is also standard to the way that science progresses.) But the situation for poor Professor Sloat is about to change.

Dialogue II

Mr. A. Professor Sloat, I'm contacting you again because it turns out your field area is now at the center of some political disagreement, since it contains the proposed site of a new nuclear reactor.

Professor Sloat: I was not aware of that. I stick to science, not to politics.

Mr. A.: So what would you think about siting a reactor on these rocks. Is this a safe site?

Professor Sloat: I have absolutely no idea; that's far beyond my expertise. Talk to a civil engineer.

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

Professor Sloat: I just have nothing to contribute to such a discussion.

Mr. A.: But are you aware that Dr. Lout, a Ph.D. geologist, has said that a fault cuts across the region and that the site is not safe?

Professor Sloat: I know nothing about that.

Mr. A.: Well he has cited your work, and says that the 10 million year gap you identify is really an earthquake fault.

Professor Sloat: Ridiculous.

Mr. A.: Can I quote you on that?

Professor Sloat: I guess so.

Advocacy and Post-Normal Science

Discussion about advocacy and science revolve around questions concerning the appropriate behavior of scientists as individuals, and often focus on seeking rules or norms for governing such behavior, by establishing definitions and boundaries, e.g., distinguishing advocacy from non-advocacy, and, in cases of advocacy, distinguishing appropriate from inappropriate. But in most and perhaps all instances where advocacy and science commingle, separating the two is logically and operationally impossible. In the first dialogue, this commingling is present but untroublesome, because, in essence, no one beyond a miniscule community of specialized experts cares about the outcome of the disagreement.

What brings the issue of science and advocacy to our attention is that scientists are supposed to represent a uniquely legitimate source of knowledge and insight in political and policy matters Resolving factual disagreements in such matters might often seem to be the key to making progress. We want to ensure that our view of the state of relevant knowledge and facts is clear, and we are concerned that when a scientist is also an advocate, this clarity is not assured, because the disinterest of the scientist is not assured.

Yet, when political and policy questions are in dispute, the legitimacy afforded to scientists is invoked in a domain of contested facts and values. Only in the most trivial of decision contexts, where there is no immediate disagreement about relevant facts, values or decision options, can a fact dictate an action. This trivial case is what Pielke, Jr. (2007) calls "tornado politics": the facts are clear (a tornado is approaching); the values are not contested (everyone would like to survive); and the course of action is obvious (get into the basement).

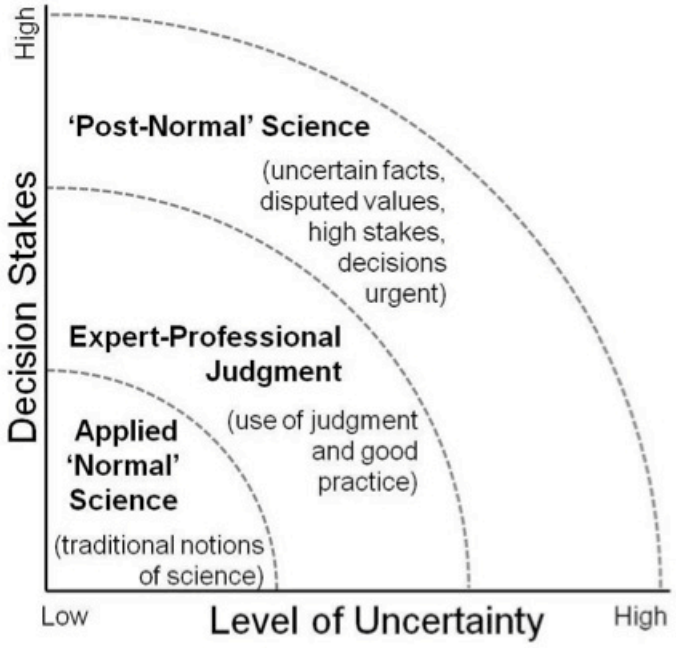
Once there are multiple decision options, "oughts" cannot be derived from "is's." While scientists might be able to say something about the alternative consequences of different decisions, they have no special expertise in deciding which decision (and predicted consequence) is to be subjectively preferred. This situation gives rise to a surprising conclusion: *in political or policy debates, the demand for, and authority of, scientists derives not from their capacity to articulate what is known and agreed upon, but from their authority in advocating for one particular fact-based interpretation of the world over another.* If all scientists agree about the relevant facts, and about the implications of such facts for the future, then any scientist (or report) will do. All arguing will be about value preferences for alternative goals, and for the means of pursuing the goals. At this point, the legitimacy of a scientist acting as an advocate is no different from a non-expert citizen because the scientist (agreeing with other scientists about the relevant facts) is simply exercising his or her opinion about the subjective options and goals to then be pursued.

For complex political and policy problems with scientific aspects, most of the time there will be multiple facts that can be assembled in multiple ways to be consistent with a range of possible actions. To make matters worse, most of the time facts and the consequences of possible actions are each associated with uncertainties about them, so the validity of the facts themselves may be contested. And then, of course, there are the norms, values, ethics, and morals that help us decide which possible action we might prefer.

The second dialogue is meant to show that the transition from advocacy that we don't care about to advocacy we do care about is a contextual matter. Professor Sloat moves into the zone of advocacy not because anything about her role as a scientist has changed, and not because she has made a decision to advocate on behalf of something other than her scientific beliefs, but because an interlocutor has taken Professor Sloat from a domain of contested facts that no one cares about to contested facts that have implications for things that people do care about, like nuclear reactor safety. We have no information about Professor Sloat's political, economic, or psychological commitments, but Professor Sloat's scientific opinion does have potential implications for our own such commitments.

The crucial aspect of this transition is that what has changed is not Professor Sloat, but the context for Professor Sloat's science. Few people would contest Professor Sloat's claim to have discovered a particular type of geologic feature that no one cares about. Let her have her unconformity. Of course it's a scientific fact; it's been peer-reviewed after all. But what we might call the "passive veracity" of Professor Sloat's discovery loses some of its luster as soon as someone cares about it for reasons outside of science itself; that is, because it has implications for action over which there is disagreement. Both the validity of the fact, and the implications of the fact for alternative courses of action, are now subject to discussion and disagreement. Professor Sloat hasn't changed, but her science now looks different.

This situation is nicely captured by the notion of "post-normal science," first developed by Silvio Funtowicz and Jerome Ravetz in the early 1990s (e.g., Funtowicz and Ravetz, 1992). Science is post-normal when values are contested, facts are uncertain, stakes are high, and the need for action is urgent. The terrain on which Professor Sloat claims authority has shifted, from normal to post-normal science, through no fault of her own. The terrain where advocacy becomes something we care about is post-normal science.



Adapted from: Funtowicz and Ravetz, 1993

But Professor Sloat claims she has no opinion about nuclear power, and has no view about whether or not a reactor ought to be sited in her field area. Doesn't this exempt her from being labeled an advocate? She's simply offering her best opinion, based on her expertise, as supported by the scientific community's processes for validating research results, and perhaps bolstered by an ego-commitment to being right about her research findings. Yet disinterest does not shield Professor Sloat from occupying a de facto advocacy position. In the realm of post-normal science, no expert can be in possession of a complete understanding of the decision context—of the potentially relevant facts, values, and uncertainties. This is not her failing; this is a condition of the complexity of reality and the limits of cognition and of science.

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. It is the relevance of her facts for the debate, the existence of other, competing facts, and her willingness to assert the validity of her facts over the competing facts—not an explicit statement of support for one subjective position or another— that turn her into an advocate. Professor Sloat's claims of neutrality or disinterest in the debate do not exempt her from advocacy; she can only escape from this condition by refusing to participate in the discussion, or by publicly insisting that she has no idea whether her factual assertions are any closer to being right than those of the scientists who disagree with her—but at that point she would also abdicate her authority in the discussion, and no one would seek her views.

Dialogue III.

Mr. A.: Dr. Lout, you have reportedly identified an earthquake fault running through the proposed cite for a new nuclear reactor.

Dr. Lout: That's correct.

Mr. A.: And as you know, Professor Sloat, of Bras-Cubas University, disagrees with this view. She 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. Lout: Professor Sloat is a fine sedimentologist in a highly ranked department at an excellent university; I have only the highest regard for her research. But I disagree with her conclusion for several reasons. Our paleomagnetic and gravity data indicate a significant change in the thickness of the shales across the area of question, which almost certainly means there has been uplift on the east side relative to the west. Moreover, investigation of microfabrics near the transition from one unit to the other shows a distinctive shear fabric. The transition is a fault.

Mr. A.: Is it a dangerous one?

Dr. Lout: On the time scale of the life of a reactor? 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.

Mr. A.: So you would oppose siting the reactor in this area.

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

Mr. A.: Dr. Lout, you work for a well-known environmental group that has long been opposed to nuclear power. Doesn't that color your scientific assessment of the situation.

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

Mr. A.: Have your results been published in a peer reviewed paper?

Dr. Lout: No, I don't have time for that.

Mr. A.: But Professor Sloat's work has been peer reviewed. And she is employed by a university, so her salary doesn't depend on her reaching any particular research result. So shouldn't we trust her view of things more than yours?

Dr. Lout: I'd 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 Sloat apparently isn't interested in the real-world implications of her work, so she is not forced to explore as many alternative hypotheses as I am. I'd say that makes my science more robust than hers.

Mr. A.: But aren't you just shopping around for hypotheses until you find one that fits your pre-existing anti-nuke conceptions?

Dr. Lout: That's backwards. Because I'm concerned about reactor safety, I want to consider all the possibilities. This means that my science has to be more careful, and more imaginative. Professor Sloat is a sedimentologist, and her conclusion reflects a sedimentological interpretation. She didn't have to consider the possibility that her unconformity was really a fault because it just doesn't really matter to anyone what she concludes.

Sincerity and Bias

Let's say that both Professor Sloat and Dr. Lout are sincere—that they both have made their best effort to understand the geological setting of the area in question, and both believe that their conclusions represent the best interpretation of the situation, given the available data. A conventional view of the sources of bias in science would in this case rate Professor Sloat's science as less subject to bias and thus likely more reliable than Dr. Lout's science. In this conventional view we would view Professor Sloat as the more disinterested scientist, and Dr. Lout as the unalloyed advocate, less deserving of our confidence.

Yet Dr. Lout's argument has merit. If he has a political commitment to opposing nuclear power, this commitment also may motivate him to explore more ideas, mobilize more methods, scrutinize data more closely, and so on. Nor is it the case that Professor Sloat is without bias. Her unavoidably partial view of the world imposes its own bias, one that is, perhaps, less subject to examination than that of Dr. Lout. Such biases may include those provided by the analytical lens of her subdiscipline, or by her view that her science is "pure," or by a desire to achieve a "publishable" conclusion to her research, or concern that her professional reputation will be tarnished if she is contradicted, and so on. Thus, if for example Dr. Lout's opposition to nuclear power might bias him toward over-valuing data that

suggests the proposed site might be dangerous, Professor Sloat's disciplinary orientation might bias her toward over-valuing data that is consistent with the theories and methods that ground her understanding and allow her to gain professional recognition.

Put somewhat differently: every expert is in some way biased, because the act of being sentient is an act of acquiring bias. But is every expert sincere? Of course not. The common suspicion might be that Professor Sloat is more likely to be sincere than Dr. Lout, and thus less likely to allow her biases to influence her scientific findings, because her biases are not explicitly related to the value disputes in play around the reactor siting issue. Yet it is precisely the nature of post-normal science that the sincerity of the expert need not be compromised by bias,[1] because the complex world permits such a wide range of scientifically legitimate interpretations. It seems to me that there is no obvious reason to suspect Professor Sloat of being either more or less sincere than Dr. Lout, although there might be good reason to suspect that her biases are less subject to critical examination than his are.

Of course attacking the sincerity of an expert with whom one disagrees is a common tactic in political debates. Climate change, genetically modified foods, nuclear waste disposal, chemical exposure risks, clinical trials, and many other post-normal science zones of political conflict are suffused with sources of bias that invite questions about the sincerity of experts with whom one disagrees. Yet this instinct dangerously oversimplifies the sources of error in complex decision processes, especially insofar as it ignores the inevitability of bias, and the impossibility of complete knowledge. For example, it was tempting for opponents of the U.S. invasion of Iraq to interpret the claim that weapons of mass destruction had been identified in Iraq as nothing more than a cynical—insincere—pretext to do what the Bush Administration intended to do anyway. Yet, as analyses of the invasion decision have documented (e.g., Jervis, 2010), this interpretation is more-or-less backwards. The Bush Administration convinced itself that the risk of Iraqi WMD's was real and urgent because its cognitive landscape was already constrained (biased) by its belief that invading Iraq was a good thing to do for other reasons. Those making the decision to invade appear to have sincerely believed that WMDs were present. The problem was not sincerity, but lack of competition with the dominant bias.

A Tale of Two Waste Repositories

When uncertainty is high and numerous interpretations of information are possible—when the science is post-normal—bias is an ugly word for the cognitive attributes that allow a person (a group; an institution; a culture) to make sense of what would otherwise be an incoherent world. When post-normal science is in play, biases may reflect many sources: disciplinary orientation (e.g., Cartwright, 1999); political orientation (e.g., Schwarz and Thompson, 1990); cultural predispositions

[1] Otherwise sincerity would be impossible.

(e.g., Douglas and Wildavsky, 1982)); institutional incentives (e.g., Simon, 1983); and so on.

The position I want to advance in the remainder of this paper is that our most effective tools for managing advocacy are found in the arrangement of our democratic institutions, not in the governance of the behavior of individual scientists, or of scientific organizations. To explore this perspective I will briefly consider two distinctive approaches to managing advocacy related to nuclear waste disposal—a complex, politically controversial, post-normal-science issue if there ever was one.

The U.S. and Sweden have had starkly different experiences in their efforts to create permanent storage repositories for high level nuclear waste. While the U.S. effort remains mired in political controversy, and the Yucca Mountain, NV site has, at least temporarily, been abandoned, with no alternative in sight, the Swedish process seems to have led to the successful selection of a repository site at Osthammar. (See Nuclear Waste Technical Review Board, 2011 and 2009 for a comparison of the two selection processes.)

The U.S. approach started with a top down political decision. After preliminary scientific assessment of multiple possible sites, Congress in 1987 selected Yucca Mountain as the nation's preferred option. It then tasked the Department of Energy to undertake a scientific characterization 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 research involved assessment of a variety of possible risks to the safety of the site, for example from ground water contamination, corrosion of containment vessels, volcanic activity, and earthquakes. The idea was that the growth of scientific knowledge would reduce uncertainty about the behavior of the site, and of waste stored at the site, during the next ten or more millennia. To this end, more than $10 billion was spent over two decades, making the area the most intensively studied piece of real estate on the planet.

From the beginning, however, there has been 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 claims (science advocacy) about the safety of the site. Scientific disagreement has been managed through formal adversarial processes—legislative, judicial and administrative, but has unavoidably as well spilled out into the more informal adversarial world of the media and local, state, and national politics. George W. Bush's administration argued that the science sufficiently demonstrated the site's adequacy, and in 2008 it submitted an 8,600 page application to the Nuclear Regulatory Commission for a license to construct the repository. Meanwhile, the state of Nevada led the opposition, mobilizing its own scientific experts to argue that the hydrological and tectonic setting of Yucca Mountain was too uncertain to guarantee its safety.

The politics changed entirely with the election of President Obama, who had pledged during his presidential campaign that he would close down the Yucca Mountain site. Senate majority leader Harry Reid of Nevada had a strong political stake in his opposition to Yucca Mountain, and Obama depended on Reid's support both to help deliver Nevada during the presidential election, and then to move the President's agenda through Congress. Science that had been good enough to support the certification of the site under President Bush was no longer good enough under President Obama.

The Swedish approach began similarly to America's, with the selection by the national government of several candidate sites for assessment, based on criteria of technical suitability. As in the U.S., 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. Many communities volunteered, and the number of candidates was gradually narrowed on the basis of technical criteria. Throughout the process, communities retained a veto power. In 2000, three towns were chosen as finalists; one then exercised its veto. The two finalists in the competition were to share a $240 million reward—three-quarters of which would go to the *loser*. In 2009, Osthammar was selected as the respository site.

The difference between the U.S. and Swedish experience here can be understood in significant part as a difference in the way that science advocacy was managed. The U.S. process looked toward science to adjudicate, in a variety of adversarial settings, the political disagreements that the site selection process raised. Scientific risk assessments were at the center of endless political and legal debates about the suitability of Yucca Mountain. In Sweden, the approach defused the need for adversarial science advocacy (perhaps a redundancy), by starting with communities that were in favor of being considered as repository sites. The disproportionate reward offered to the "losing" community created a counterbalance to the pro-repository bias created by the volunteer approach to site selection. The pathologies of post-normal science were managed not by reducing scientific uncertainties, but by reducing political uncertainties.

The U.S. and Swedish nuclear waste experiences suggest three main lessons for democratic institutions seeking to manage science advocacy:

- Premature political closure is likely to heighten the incentives for post-normal science advocacy. (Climate change provides another depressingly vivid illustration of this reality.)
- Traditional, formal institutional approaches (judicial, legislative, administrative) to managing adversarial processes do not tame post-normal science advocacy.

- Voluntarist approaches and early stakeholder engagement can help reduce the incentives for post-normal science advocacy.

While Sweden has shown that it is possible to substantially design science advocacy out of democratic institutions, I want to emphasize that this may not always be either achievable or desirable. In the world of post-normal science, pervasive uncertainty about the consequences of complex political and technical decisions means that good decisions may in fact depend on the adversarial engagement of expert advocates holding competing biases (as the Iraq invasion example was meant to illustrate). Here, the key design principle for democratic institutions may be to ensure balance in the biases of science advocates, and transparency about the nature of those biases.

This was well understood by Herb Simon many years ago: "When an issue becomes highly controversial—when it is surrounded by uncertainties and conflicting values—then expertness is very hard to come by, and it is no longer easy to legitimate the experts. In these circumstances, we find that there are experts for the affirmative and experts for the negative. We cannot settle such issues by turning them over to particular groups of experts. At best, we may convert the controversy into an adversary proceeding in which we, the laymen, listen to the experts but have to judge between them." (Simon, 1983, p. 94).

Yet Simon's insight has not been widely institutionalized. The political scientist Mark Brown has noted, for example, that the Federal Advisory Committee Act does not require technical experts serving on FACA committees to be classified in terms of their political interests, but only in terms of their area of technical expertise. (At the same time, FACA does require that non-expert "representatives" be classified in terms of political interest.) FACA, that is, treats scientists as if their opinions are dictated by their technical perspectives alone—a view that, as I've belabored, is incoherent. FACA thus covers up the diversity of advocacy biases that should in fact be transparent in democratic debate. As Brown notes, "the prevailing approach seeks to prevent the politicization of expert advice, [but] it actually promotes it." (Brown, 2008, p. 547).

The problem with science advocacy is not that scientists do it, it's that we continue to view it as a problem to be addressed, and hopefully avoided, by managing behavior at the individual level, rather than a condition of reality that must be accommodated through the appropriate structure of our democratic institutions. My own view is that institutions that host adversarial science advocacy proceedings, or who assemble experts groups who are expected to arrive at a consensus about the state of knowledge relevant to a politically hot issue, need to be transparent about the biases of the participating science advocates. So perhaps this implies a component of individual responsibility after all, but not one that is usually envisioned: Any scientist who wants to make a public proclamation that a) takes advantage of his or her own legitimacy as an expert and b) is relevant to a public policy debate, ought to be willing to make his or her political and social preferences

as clear as his or her area of technical expertise. (I'm a liberal democrat, by the way, whatever that means anymore.) While this might seem threatening to the authority of experts upon whom society greatly depends for wise decision making, the much greater threat to science, I believe, is from the continued pretense that scientists can remain disinterested participants in complex and divisive political controversies. The public relations disaster resulting from the release of emails from the University of East Anglia Climatic Research Unit in November 2009 is one example of the consequences of trying to maintain this pretense.

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