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Science, Expertise, and the Democratization of the Decision-Making Process

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Environmental scholars and practitioners are calling for the democratization of science and expertise. Two of the earliest and most influential arguments toward this end come to us from Silvio Funtowicz and Jerome Ravetz, with their now famous discussion of "postnormal science," and Alvin Weinberg, with his well-known distinction between "research" and "trans-science". Such positions, however, prove highly problematic. First, while calling for the opening of some questions to nonscientists, they likewise continue to uphold and justify a closed position of science for others. Second, these arguments fail to highlight how prominent fact/value conflation is in such fields as the environmental sciences (through such concepts as "ecological integrity," "ecosystem health," etc.). This article seeks to redress these problems by shifting attention away from discussions of "science" to that of "expertise," and in doing this, to provide an alternative way of thinking about how to resolve today's environmental problems.

Keywords boundary organizations, democracy, ecology, ecosystem health, expertise, management, science, values

Much has been written of late about the need for public participation in the decision-making process, especially in the face of complex environmental threats—what has been referred to as the democratization of science and expertise (Beck 1992 [1986]). This "opening," it is argued, allows other thoughts, observations, and data to make their way into the scientific processes to the betterment of scientific knowledge. Funtowicz and Ravetz (1992), with their well-known discussions of "postnormal science," forward such an argument. So too does Alvin Weinberg (1972, 1985), through his now famous distinction between "science" (or what he also calls "research science") and "trans-science" (also called "policy science"). Ultimately, these positions argue that an increase in the complexity and uncertainty of scientific questions should likewise result in an increase in the democratization of procedural rules as to how to "do" science. Thus, when complexity and uncertainty are low, science can proceed in a more orthodox, closed manner. In the face of uncertain, complex questions (e.g., environmental risks), however, scientific ways of knowing

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break down as values and uncertainty require scientists to look beyond "the facts" to make determinations.

This essay addresses these positions on two fronts. First, while such arguments seek to increase participation for some questions (e.g., those of high complexity and uncertainty), they also shut down participation in others (e.g., those of low complexity and uncertainty). These models, in other words, represent examples of "boundary work," in that they ultimately serve to uphold traditional distinctions between science and policy by allocating rights as to who can (and cannot) interpret science (Gieryn 1983; Jasanoff 1987).

Second, these positions fail to point out the significant ways that facts and values can become intertwined in some of the concepts used in the environmental sciences. Concepts such as "ecological integrity," "ecosystem health," and "pollution," just to name a few, often pass as terms that correspond to some objective article of reality, but are, on closer inspection, value statements.[1] This is not to deny the existence of the underlying reality (or referent) that these claims speak to, but rather to highlight how these concepts rest on beliefs about what we think "nature" *should* look like.

I begin this essay by briefly outlining the positions of Funtowicz and Ravetz and Weinberg, specifically as they argue for there being different "types" of science. I then critique these positions, suggesting that rather than attempting to construct different types of science, attention should focus on the phenomenon of expertise. To orient this argument to the environmental sciences, discussion then turns to the various ways that values and facts get entangled in the science of many of today's ecological debates—focusing specifically on the concept of "ecosystem health." To conclude, policy-relevant suggestions are made in light of these arguments.

From "Applied" ("Research") to "Postnormal" ("Trans") Science

Funtowicz and Ravetz (1992) provide us with a well-known operational methodology around which to move away from a singular view of science to one of more open and plural character. When speaking of the highly ambiguous nature of today's environmental problems, they acknowledge that understanding and decision making on the basis of many of these problems can only take place in a value-laden context. The practice of objective science, they contend, is unequipped to deal with any intermingling of facts and values, and as such is grossly unprepared as a knowledge system for many of today's environmental problems. Other ways of "doing" science must therefore be developed and nurtured. Toward this end, Funtowicz and Ravetz develop their argument for what they term "postnormal science."

Specifically, they develop their methodological model around the two variables: systems uncertainty and decision stakes. When dealing with questions that involve low levels of each, they suggest the use of "applied science." In dealing with medium levels of both variables, "professional consultancy" is suggested. And when questions involve high levels of these two variables, "postnormal science" is called for. According to Funtowicz and Ravetz, the way we "do" science must move away from that of "applied" to that of "postnormal" as the axiological aspects of scientific questions increase.

Alvin Weinberg (1972, 1985) has made a similar argument in his distinction between "research" and "trans-science." Specifically, Weinberg uses the concept "trans-science" to highlight the fact that some questions, while still scientific, involve so many variables (biophysical and otherwise) and nonlinear relationships that they are simply beyond the abilities of "research science" (which is based on fact, certainty, and scientific consensus). In doing this, Weinberg admits to there being a gray zone between science and policy. This zone is characterized by questions "which can be asked of science and yet which cannot be answered by science" (Weinberg 1972, 209).

In short, the positions of Funtowicz and Ravetz and Weinberg argue that as questions increase in their complexity and systems uncertainty (coupled with an increase in "dread factor"), so too should there be an increase in procedural openness regarding how we go about answering those questions.[2] Such a position is not without its problems, however.

Focusing on Expertise

The construction of such categories as "research" and "trans-science" serves a strategic purpose, for the categories suggest that the uncertainties intrinsic to the regulatory (policy) process are not intrinsic to science itself. Through this, science remains untouched by uncertainty, as does the undisputed preserve of scientists in terms of how the accompanying questions are answered and who is involved in the evaluation of those answers (Jasanoff 1987). When dealing with "research" (or "applied") scientific questions, such procedural frameworks thus end up stifling communication from nonprofessional experts, viewing their knowledge as unnecessary to the decision-making processes. Consequently, although they are presented as frameworks to open up procedural rules to scientists and nonscientists alike, their arguments cut both ways—for in calling for more open deliberations to some questions, they are likewise suggesting that others would do well to remain closed.

In light of this critique, I suggest moving beyond such attempts to construct different types of science and focus instead on the phenomenon of expertise. After centuries of philosophical debate about what science is, we still lack a shared definition of the term.[3] Some fields of science, for example, are highly experimental (e.g., high-energy particle physics); others almost entirely observational (e.g., astronomy) or based on complex modeling (e.g., meteorology). Contrary to Karl Popper (2002 [1963]), science need not be grounded in the principle of falsification. Even the methodological benchmark of "repeatability," seen by some as the hallmark of that which is "scientific," has been shown to be highly problematic by sociologists of knowledge (Latour 1987; Lynch 1985).

So how, then, do we move discussion to the issue of expertise? In answering this question, I build on the work of Collins and Evans (2002) and their delineation between three types of expertise. From this seminal piece come the following three forms of expertise: no expertise; interactional expertise; and contributory expertise. In developing this distinction, Collins and Evans work from their firsthand experiences as sociologists of knowledge, and through the problems they confront in trying to cognitively grasp the often times esoteric subject matter they are studying. I suggest, however, that our understanding of these typologies can be slightly modified to present a better fit for the environmental sciences. In doing this, I have come up with the following definitions.[4]

- *No expertise*: A degree of expertise insufficient to engage in an even cursory discussion of the topic in question.
- *Contributory expertise*: Enough expertise to contribute to the knowledge base of the topic in question, noting, importantly, that such cognitive authority can come in the form of either abstract/generalizable or local/practical knowledge.

- *Interactional expertise*: A form of expertise that rests on having contributory expertise in the form of either abstract/generalizable or local/practical knowledge *while also* having enough expertise to interact interestingly with those who possess contributory expertise of the other form (thus allowing for important *interactions* to occur between the two).

To illustrate these terms, Collins and Evans draw from Brain Wynne's (1989) famous study of the relationship between scientists and sheep farmers after the radioactive fallout from the Chernobyl disaster contaminated the Cumbrian fells. In this work, Wynne examined the relationships between UK Ministry of Agriculture Food and Fisheries (MAFF) scientists and the Cumbrian sheep farmers after radioactive fallout contaminated their pastures. Wynne argues that the expertise of the sheep farmers with respect to sheep should not have been ignored (which it was by MAFF scientists). In this case, the farmers knew a great deal about the ecology of the sheep, the prevailing winds, and the behavior of rainwater on the pasture land that was relevant to discussions of how the sheep should be treated in order to minimize the impact of the radioactivity. Nevertheless, the MAFF scientists failed to listen to the farmers because the farmers lacked the proper scientific training and credentials.

The farmers, according to Collins and Evans, thus possessed contributory expertise (which in some respects even exceeded that of the MAFF scientists). What was lacking in this case, however, was interactional expertise. On the one hand, the MAFF scientists lacked the expertise to understand that the farmers really did know what they were talking about and that their knowledge was meaningful to the broader decision making process. The scientists also lacked knowledge of the local lexicon, which may have further hindered such interactions. On the other hand, the farmers lacked the expertise that would have allowed them to enter into and speak the technical, universal language of science. Yet, as Collins and Evans point out, this is not to suggest that the farmers needed to engage in a symmetrical conversation with the scientists—only that the scientists should have been open to the possibility that they could have learned something from the farmers. Indeed, were "the situation symmetrical, it might have been an arbitrary matter whether the farmers' expertise was absorbed by the scientists or the scientists' expertise was absorbed by the farmers" (Collins and Evans 2002, 256). This highlights, they argue, where the location of change needs to be: namely, within the structures of authority.

Through this, we can begin to see that such concepts as "lay expertise" (Wynne 1989), "community science" (Carr 2004), and "local knowledge" (Fischer 2000) are all ultimately speaking of the need for expertise—or, more specifically, contributory expertise. But contributory expertise is not all that is required to effectively deal with many of today's environmental threats. As others have also noted, great strides have been made in the uptake of local knowledge in those instances where such knowledge is combined with the technical language of objective, universal science (Carr 2004; Epstein 1996; Forsyth 2004)—which, in other words, also speaks to the importance of interactional expertise.

Against this typology of expertise, we can now return to the issue of science, by understanding it as simply that which those with (contributory and/or interactional) expertise "do"—recognizing, importantly, that this "doing" is different, in terms of the technologies and practices employed, from the "doing" of politics. Toward this end, Pickering (1995), for instance, has vividly detailed the sociomaterial underbelly of science—referring to this exchange between technology and practice as a "mangle." Through this, we can begin to talk about the need to "open up" decision-making structures to not only those contributory experts with abstract/generalizable knowledge. Also include in this process would be those contributory experts with local/practical knowledge, which thus requires the inclusion of individuals with interactional expertise (to allow for a useful exchange of information between these two groups).

I would like to now suggest a fourth type of expertise, which is particularly important for the "doing" of environmental science and resource management: that of, for lack of a better term, "public expertise," which speaks to the explicit incorporation of values into the decision-making process. Public expertise is particularly valuable (and some might say ethically mandatory) when dealing with, for instance, environmental threats/risks. Such expertise could be attained by opening up discussion so all concerned individuals can voice their views regarding the environmental threat/risk being deliberated on (especially among those who have to live with it on a daily basis). Or perhaps this expertise can be bestowed on a proxy who is accountable to the public—such as an elected official (or officials; versus an unaccountable bureaucrat or scientist). Inevitably, once we begin to move from questions of what "is" to "what *should* be done" (e.g., policy and regulation)—two questions that are often inseparably intertwined in the ecological sciences—an expertise is required that goes beyond merely possessing knowledge that contributes to the cognitive base of the field being analyzed. What is also required is a gauge of public sentiments and values.

In addition, public expertise can play a role in giving shape to the material practices of science itself. This not to suggest that the public be allowed to actually participate in the "doing" of science—at least, not without possessing either contributory or interactional expertise in the area under investigation. Public expertise should, however, be involved in shaping the flow of money and support to these material practices, and should have some say whenever such practices present a risk to humans and/or the environment.

Ecology and Values

Before concluding, allow me to orient this argument to the environmental sciences, highlighting the various ways that values can implicitly enter into ecological discussions. Concepts such as, for instance, "ecological integrity," "ecosystem health," and "biodiversity" are often viewed as representing objective states of reality, when on closer inspection they represent value statements. They speak, in other words, to what we think "nature" *should* be. Scientifically speaking, however, there is little agreement as to what "nature" is. That is to say, even in those cases where we can speak of what "is" (e.g., species X represents a keystone species), this cannot logically be conflated into an "ought" statement (e.g., species X *should* be preserved) without committing a "naturalistic fallacy" (Moore 1959). So here too, science—or, more specifically, contributory expertise—alone cannot make these decisions for us (Decker et al. 1991; Herrero et al. 2001; Soulé 1986; Roush 1995).

Take, for instance, the concept of "ecosystem health." As others have noted, a state of "health" is not an inherent property of ecosystems, but rather comes out of value statements about what we think a healthy ecosystem should be (Lackey 2001; Wicklum and Davies 1995). A common assumption often made toward this end is that there is a similar state among ecosystems to that found in the homoeostatic physiological system. Any deviation from this "natural state" can thus be used to measure changes to the overall health of the ecosystem in question (Jorgensen et al. 2005). As our understanding of ecosystems increases, however, this "steady-state" argument is being overtaken by that of dynamic equilibrium (Gunderson and Holling 2002; Jorgensen 2002). This new paradigm in ecology greatly undercuts assumptions that a particular state of an ecosystem is the "right" one (in an objective, scientific sense).

Another approach is to select benchmarks that describe a "healthy" ecosystem—such as higher biological diversity (Wilson 1992). But, again, science alone cannot hierarchically rank ecosystem states in terms of their inherent superiority (e.g., high biological diversity) or inferiority (e.g., low biological diversity) (Lackey 2001). We make those preferences for them, due to cultural beliefs as to what we think nature should look like (Kapustka and Landis 1998).

In failing to critically engage these concepts, we mask over underlying value judgments, which are then packaged to the public as objective statements of fact. While contributory expertise can still play a valuable role in helping to lay out options, and describe the potential consequences of each, it possess no more (or less) cognitive authority in deciding the "shoulds" of environmental policy than any other form of expertise. Interactional and public expertise should thus be viewed as essential components within environmental management and policy making circles, given the confluence of complexity science, stakes/risks, and fact/value conflation in the knowledge producing practices that inform them.

Conclusions

Many readers of *Society and Natural Resources* spend a great deal of time developing contributory expertise (specifically, that of abstract/generalizable knowledge). Yet far less energy is spent developing among professional environmental scientists knowledge that would allow them to interact in meaningful ways with nonprofessional local experts who possess practical, lived contributory expertise. What is often lacking, in other words, is sufficient interactional expertise. This point is supported by research illustrating that citizens (*not* professional scientists) have often been the ones that have had to develop this expertise if they wanted their voices and accompanying data heard by the larger scientific community (e.g., Carr 2004; Epstein 1996; Forsyth 2004; Lachmund 2004). Perhaps this type of expertise is something we could work to nurture as we train future generations of environmental scientists.

Beyond this, institutional frameworks also often lack the means to incorporate public expertise into the decision making process. One potential solution to this is the consensus conference, which has been used in such countries as Denmark, England, Australia, and Sweden to significant effect. We must thus remind ourselves that contributory expertise, while important for purposes of informing public policy, cannot dictate it, and that, ultimately, access to decisions making structures should not be dictated by formal "scientific" credentials alone.

One potential example of multiple forms of expertise "in action" is the "boundary organization" (although to my knowledge this connection has never been made) (Guston 1999, 2001). Boundary organizations refer to those social and organizational arrangements that attempt to mediate between the institutions of "science" and "politics." A successful boundary organization therefore "facilitates collaboration between scientists and nonscientists" (Guston 2001, 401), or, in terms of expertise, it seeks to facilitate coordination between contributory, interactional, and public experts. Admittedly, since "boundary organizations" have yet to be discussed in terms of the types of expertise they nurture, such a link is tentative at best. I end this article mentioning it, however, because this represents the next step in discussions of expertise: debating how best to organized decision-making structures around these multiple forms of cognitive authority.

Notes

1. I thank an anonymous referee for encouraging me to pursue this point.
2. I thank Jerry Ravetz for clarifying just what "decision stakes" mean in his and Funtowicz's model. Specifically, if the risk in question has (on whatever grounds) a high "dread factor," then the decision stakes are high, regardless of how great the uncertainties are (personal e-mail communiqué, December 31, 2004).
3. Philosophy of science, however, did not form as a subfield until the early 20th century.
4. Given some of the empirical examples used by Collins and Evans to illustrate their typology [such as the Wynne (1989) case study to be discussed shortly], I question whether my reconceptualization departures much from how Collins and Evans envision their typology once put into practice.

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