

Science and public policy: what's proof got to do with it?

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Abstract

In recent years, it has become common for opponents of environmental action to argue that the scientific basis for purported harms is uncertain, unreliable, and fundamentally unproven. In response, many scientists believe that their job is to provide the “proof” that society needs. Both the complaint and the response are misguided. In all but the most trivial cases, science does not produce logically indisputable proofs about the natural world. At best it produces a robust consensus based on a process of inquiry that allows for continued scrutiny, re-examination, and revision. Within a scientific community, different individuals may weigh evidence differently and adhere to different standards of demonstration, and these differences are likely to be amplified when the results of inquiry have political, religious, or economic ramifications. In such cases, science can play a role by providing informed opinions about the possible consequences of our actions (or inactions), and by monitoring the effects of our choices.

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1. Introduction

The heart of Bjørn Lomborg's recent critique of environmentalism is that many assertions of the environmental movement are unproven and therefore provide no good grounds for sensible public policy. Current debate, he argues in *The Skeptical Environmentalist*, is based “more on myth than on truth (Lomborg, 2001, p. 32)”.¹ We all want our views to be based on truth, and many of us look to science to provide truth. But the truth is not always convenient, and it is rarely convenient for everyone, generating in-

centive for manipulation and misrepresentation of information. This is particularly true in the domain of environmental policy.

Lomborg assures us that everyone is *for* the environment—just as everyone is for world peace and against hunger—but this facile assertion masks the fact that many individuals and institutions, particularly in the industrialized west, have a vested interest in maintaining the status quo. Environmental modification is an effect of economic and social activity; preservation, conservation, and mitigation inevitably mean pecuniary or opportunity costs for some individuals, groups, or nations. Demands for intervention engender opposition from those who might expect to bear these costs. Increasingly this opposition takes the form of attacking, impugning, or otherwise seeking to question the science related to the environmental concern (Herrick and Jamieson, 2001).

In recent years it has become common for informed defenders of the status quo to argue that the scientific information pertinent to an environmental claim is uncertain, unreliable, and, fundamentally, unproven. Lack of proof is then used to deny demands for action. But the idea that science ever could provide proof upon which to base policy is a misunderstanding (or misrepresentation) of science, and therefore of the role that science ever could play in policy. In all but the most trivial cases, science does not produce logically indisputable proofs about the natural world. At best it produces a robust consensus based on a process of inquiry

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I should note that I believe that a good deal of what Lomborg says is true (or at least I accept it to be so): the *statistical* evidence for improvements in many quantitative aspects of human life for most people is very strong. Elsewhere I have critiqued the work of the Club of Rome, and have argued against doomsday predictions by scientists (Oreskes and Belitz, 2001; Oreskes and Le Grand, 2003). However, statistic analysis fails to encompass compelling reasons for environmental protection: moral, aesthetic, philosophical, emotional. A world without elephants would be an impoverished place, and increases in total forest cover do not necessarily increase the number of places where elephants can live. While Lomborg is explicit about writing from the perspective of human needs and expectations (Lomborg, 2001, p. 11), I believe that humans do not have the right to wipe other species off the planet, nor do those of us living today have the right to degrade or destroy resources that might add value to the lives of future humans. From this perspective, Lomborg's arguments are at best partial.

that allows for continued scrutiny, re-examination, and revision.²

2. In a perfect world . . .

Lomborg's desire for truth-based policy can be reframed as a vision of how policy would be framed and implemented in a perfect world. In this perfect world, scientists collect facts, politicians develop policies based on those facts, legislators pass laws to implement these policies, and government agencies enforce the laws, most likely through regulations based on the same kind of facts. Because the laws, policies, and regulations, are based on the truth, they work, and our problems are solved: efficiently, effectively, and economically. More subtly, we might say that science gives us our most reliable understanding of the natural world, and therefore provides the best possible basis for public policy on subjects involving the natural world.

This has been the historical justification for science advice in government, a tradition that in the United States goes back at least as far as the US Fish Commission, established in 1871 to determine the causes of declines in fish catches in New England and suggest appropriate remedies (Allard, 1978; McEvoy, 1986; Smith, 1994). It provided the justification for the creation of the National Academy of Sciences and the National Research Council, and for the great post-war expansion of science and inclusion of scientists as policy advisors in the US government (Dupree, 1957). It remains the justification today for offices and organizations such as the President's Science Advisory Committee and the White House Office of Science and Technology (Kevles, 1978; Snow, 1960; Price, 1962, 1965; Smith, 1990). This perspective has also underpinned various scientific initiatives in support of policy development, such as the US National Acid Precipitation Assessment Program of the 1980s and the Intergovernmental Panel on Climate Change (Herrick and Sarewitz, 2000; Miller and Edwards, 2001). Nowadays a common political response to an environmental problem is to establish a scientific agency, program, or initiative to investigate it.

While there are other kinds of political responses, it certainly is the case that environmental problems *can* be formulated as scientific questions. In part this is because often science establishes the problem *qua* problem. Who among us would know there was global warming without scientific evidence to that effect? Who would know that atrazine might affect amphibian sperm? Who would know there was MTBE in groundwater? Even if we were octogenarian farmers in New England keeping weather almanacs and noticing that winters seemed to be getting milder, how

would we know that this was a global phenomenon, and how would we identify increased atmospheric CO₂ as the likely cause? Questions about hazards almost invariably require scientific data to define the hazard as a hazard (as opposed to being part of normal everyday life), and to evaluate its quantitative prevalence, if not necessarily its qualitative significance to individuals. So we find ourselves posing questions such as: Is the globe warming? Are fish populations collapsing due to overfishing? Is biodiversity required for ecosystem stability? Do anthropogenic chemicals in the environment cause cancer? Are hormone-mimicking chemicals disrupting endocrine processes in animals?

These questions invite answers, and recalcitrant actors may present themselves as skeptics demanding proof. Partly for this reason, many scientists have concluded that their task is to provide the proof that society needs, via better climate models, better biodiversity indices, better estimates of ocean temperature, and so on (see, for example, Levitus et al., 2000; Jackson and Johnson, 2001; Canham et al., 2003). Once we have these, then we will know what action to take and when to take it. Of course, we all know that the sciences never provide absolute proofs, but nevertheless we look to scientific research to provide the nearest approximation to proof that we can obtain. We look to science to tell us if a problem is real, and if so what to do about it.

The difficulty is this: proof does not play the role in science that most people think it does (or should), and therefore it cannot play the role in policy that skeptics demand. In this paper, I explore three examples at the nexus of science, proof, and/or policy: one, an example where scientists successfully forged consensus despite the fact that earlier expressed standards of proof had not been met; two, an example where policy-makers successfully forged consensus despite acknowledged uncertainties and disagreement by some experts; and three, an example of scientists who tried to provide a convincing demonstration of an environmental effect, but were vilified by environmentalists for the attempt. By examining examples of past disputes, we can perhaps gain a more realistic appreciation of what science can and cannot do in aid of public policy.

3. From continental drift to plate tectonics: the proof of moving continents?

When Alfred Wegener proposed continental drift in 1912 as a unifying theory of earth sciences, he also provided abundant evidence of it (Wegener, 1912, 1915, 1924, 1929). Besides the obvious "jigsaw-puzzle" fit of the continents, data from paleontology, stratigraphy, and paleoclimatology strongly suggested that the continents had once been unified, then broken apart, and drifted into their present configurations. Despite cavils over the details of the data by some specialists, most of this evidence was broadly accepted as factual by earth scientists, and had been used by other scientists to support alternative explanatory frameworks (Marvin,

² One could pursue a taxonomy of levels of scientific truth, following the legal model of different standards for criminal and civil law. While this might be useful, it's unlikely that the history of science would fit any neat taxonomy. In any case, such an attempt would be beyond the scope of this paper, whose purpose is simply to suggest that demands for "certainty" and "proof" are asking the impossible, and the unnecessary.

1973; Le Grand, 1988; Oreskes, 1999). Despite widespread acceptance of the bulk of the evidence and widespread discussion of the theory, continental drift was generally regarded as unproven. What would have constituted proof?

Wegener's own answer was the direct measurement of continental motion. His inference about drift was abductive—the observed phenomena would be expected if continental drift were true—but the resistance of many geologists led Wegener to conclude that indirect reasoning was insufficient. One needed direct proof. One needed to see the thing happening. Happily, geodetic measurements in Greenland seemed to reveal a westward drift, and Wegener planned to take further measurements in a return trip in 1929–1930. Unfortunately he died on that expedition (Greene, 2004).

Wegener's conclusion was not idiosyncratic; others also believed that direct measurement of continental motions constituted the definitive test. In 1926, a group of international scientists organized the Worldwide Longitude Operation to prove or disprove continental drift by measuring inter-continental distances through radio wave transmission times. While the scientists involved were admirably patient, after a decade the results were still inconclusive (Oreskes, 1999; Dick, 2003). Then global political events made further work impossible.

In the late 1950s, the question of crustal motions was re-examined. In the mid 1960s plate tectonics became the unifying theory of earth sciences, and moving continents became established scientific fact. By the early 1970s, textbooks had been rewritten in the framework of plate tectonics, and historical treatments were being published (Cox, 1973; Le Pichon et al., 1973; Hallam, 1973; Frankel, 1979, 1982, 1987; Laudan, 1980). Plate tectonics was now accepted by scientists as true, but was it proven? Not by the standard demanded in the earlier debate.

Like the evidence of continental drift, the evidence of plate tectonics was indirect. It consisted of terrestrial rock magnetism, which showed that the continents had altered their positions vis-à-vis the magnetic poles, marine magnetic measurements, consistent with the creation of new oceanic crust at mid-oceans ridges and its lateral displacement, and seismic first-motion measurements, consistent with large crustal slabs moving outward from the mid-ocean ridges and downward under the continents in subduction zones. Again, the relevant inferences were abductive: these phenomena were things that would be observed if plate tectonics were true, and would be very difficult to explain if it weren't. Finally, the data became so abundant and the patterns so clear that no one doubted that it *was* true. But scientists in 1960s had no more direct evidence of continental motions than they had in the 1920s.³

³ Some might claim that the magnetic stripes on the sea floor did constitute direct evidence, because one could calculate spreading rates from the pattern of reversals. But this is still not a direct measurement of the motions, it is a measurement of magnetic stripes, from which one deduces the spreading rates.

When *did* earth scientists finally measure continental motion directly? Nearly 20 years later. In the mid 1980s, very long baseline satellite interferometry made it possible to measure the distances between points on Earth with great accuracy, and to detect small changes in these distances over time. In 1985–1986, a series of papers reported the results, and the general conclusion was that the drift of the continents was now proven (Christodoulidis et al., 1985; Clark et al., 1985; Kerr, 1985; Herring et al., 1986). Given this, it could be argued that for 20 years, earth scientists used, taught, and believed in the fundamental truth of plate tectonics without “proof” that plates were moving. Were they wrong to do so? Was this bad science? Of course not. The evidence of plate tectonics was sufficiently overwhelming that direct measurement of continental motion was not required. Plate tectonics was not proven by the standard proposed by the advocates of the Worldwide Longitude Operation, but it nevertheless met the standards of earth scientists in the 1960s, who forged a consensus around it. Geodesists in the 1980s received relatively little attention for their work, because they had “proved” what by that time everyone already knew (Oreskes and Le Grand, 2003, p. 406).

Given that earth scientists are nearly unanimous that the formulation of plate tectonics was one of the great advances of twentieth century earth science, it seems clear that science does not require proof—neither in the sense of a direct detection or measurement, nor in the sense of certainty or unanimity—to advance. Science can and does proceed on the basis of indirect evidence and abductive inference, so long as the evidence and the inferences are acceptable to relevant scientific experts. In the earth and environmental sciences, in which controlled experiments are rarely possible, this is generally how it does proceed. In the case of plate tectonics, by the time direct measurements were obtained, they were superfluous; the community had already achieved consensus. The satellite data were interesting and satisfying, but from the perspective of the advance of the science, they were not especially important.

Now imagine that continental drift had been relevant to a question of public policy. We can immediately see that defenders of the status quo could have insisted that the data were indirect and the theory was not proven. Moreover, they could have found prominent scientists to support this view. Even in the 1970s and 1980s, there were a few well-known outliers, such as the distinguished geophysicists Sir Harold Jeffreys and Gordon J.F. MacDonald, who rejected laterally mobile continents outright, and the Tasmanian geologist S. Warren Carey, who in the 1950s had organized research on mobile continents based on the alternative framework of an expanding Earth and continued to advocate that view until his death (Oreskes and Le Grand, 2003; Munk et al., 2004).⁴

⁴ The expanding Earth theory continues to be a live option for a small number of Earth scientists, who rarely get their views included in standard textbooks, but have produced volumes of their own. A recent example is Scalera and Karl-Heinz (2003).

If one had looked hard enough, one might even have found someone who was specifically waiting for direct quantitative measures of plate motions; such skeptics could have been trotted out to demonstrate that the theory was uncertain. If money or celebrity had been at stake, it's likely that more skeptics would have been generated. After all, as Thomas Hobbes noted centuries ago, men will argue about the rules of geometry if they find it in their interest to do so (Hobbes, 1969; see also Shapin, 1994, p. 224).

Should earth scientists have waited for these recalcitrant individuals to be convinced? Should implementation of our hypothetical policy been deferred? Of course not: scientific knowledge would not develop if such severe standards were enforced. Indeed, it was precisely this feature to which Thomas Kuhn credited the progressive nature of scientific inquiry: that scientists, unlike artists or humanists, forge stable consensus by ignoring outliers and moving on (Kuhn, 1962; see also Latour, 1987).

This thought experiment makes it clear that the appropriate standard for judging science is neither proof, nor certainty, not unanimity, but a broad and firm consensus of the relevant experts in the field. The reason is simply this: Scientific knowledge *is* the intellectual and social consensus of affiliated experts based on the weight of available empirical evidence, and evaluated according to accepted methodologies. If we feel that a policy question deserves to be informed by scientific knowledge, then we have no choice but to ask, what is the consensus of experts on this matter? If there is no consensus of experts—as was the case among earth scientists about moving continents before the late 1960s—then we have a case for more research. If there *is* a consensus of experts—as there is today over the reality of anthropogenic climate change (Oreskes, 2004)—then we have a case for moving forward with relevant action.⁵

Another point should be evident by now: There is no objective, irrefutable definition of what constitutes scientific proof, and no atemporal criteria upon which scientists have forged consensus. At different times, in different places, and among different communities of practitioners, scientists have adhered to differing standards of demonstration and argumentation and forged consensus by various means. What some earth scientists demanded in the 1920s, others were content to live without in the 1960s. Similar stories can be found throughout the history of science (Galison, 1997; Maienschein, 1991a,b; Pickering, 1984; Rudwick, 1985).

⁵ The problem of *how* we determine the consensus of scientific opinion is beyond the scope of this paper, although not beyond the scope of sociology of science, in general. Note, also, that I am referring to consensus of experts in the evaluation of *technical* knowledge—such as whether tectonic plates exist, whether the double helix structure adequately accounts for the properties of the DNA molecule, or whether the theoretical basis for linking observed global temperature patterns to increased atmospheric CO₂ is convincing. Consensus over what to do about any of these matters is another domain, one which extends far beyond the boundaries of technical expertise (see Wynne, 1992). My argument should by no means be read as a brief for enforced consensus, in either science or policy (Pielke, 2001).

To demand that scientists satisfy some abstract notion of “proof” is to fly in the face of the historical evidence about how science has ever proceeded.

4. Rachel Carson and *Silent Spring*

In 1962, Rachel Carson published one of the best-selling science and nature books of all time: *Silent Spring* (Carson, 2002). Serialized in *The New Yorker*, it drew enormous attention to the environmental impact of widespread pesticide use, especially DDT. Historians have suggested that *Silent Spring* was to environmentalism what “Uncle Tom’s Cabin” was to abolitionism: a spark for a new consciousness about the environment, ultimately resulting in the banning of DDT use in United States (Wang, 1997). But although Carson was a scientist—a marine biologist with the US Fish and Wildlife Service—her work was harshly criticized by various scientific colleagues.

Carson’s critics complained that her claims were largely circumstantial, that her evidence was anecdotal, her conclusions exaggerated. The book was more emotional than scientific, they charged, playing on fears, including the fear of nuclear fall-out, quite unrelated to DDT (Graham, 1970; Dunlap, 1981; Lear, 1992; Wang, 1997). These critics included chemists in corporate research laboratories and at the US Department of Agriculture, epidemiologists and disease control experts, academic food scientists, and even the National Academy of Sciences Committee on Pest Control and Wildlife Relationships. *Silent Spring* was negatively received in various journals, including *Chemical and Engineering News* and *Science*. Emil Mrak, Chancellor of the University of California at Davis and Professor of Food Science, testified to the US Congress that Carson’s conclusion that pesticides were “affecting biological systems in nature and may eventually affect human health [was] “contrary to the present body of scientific knowledge” (Wang, 1997).

In some respects the critics were correct. Carson’s book was based largely on case reports that were not supported by broad statistical analysis, and it was based on fear: fear of what would happen if we continued with reckless attitudes and actions, and the fear invoked in the book’s title, of a world without song, without beauty, and ultimately perhaps without life. But *Silent Spring* was not written as a scientific paper to be published in a refereed journal; it was written as a popular book, indeed, a polemic. It was not intended to convince scientific experts, it was intended to reach and motivate ordinary citizens. In this regard, Carson achieved her goal spectacularly. She was not a bad scientist, and she was a great writer (Lear, 1992).

In the early 1960s, few systematic studies of the cumulative environmental effects of DDT had been done, in part because the immediacy of the military context in which its efficacy was first demonstrated had obscured the long-term safety issues. During World War II some government scientists had warned of DDT’s hazards, but because DDT was

considered a military technology the relevant studies were mostly classified, and few in the public knew of their results. After the war, safety considerations were largely brushed aside as DDT was hailed as a miracle chemical, and its developer, Paul Müller, awarded the Nobel Prize in medicine or physiology for its use in disease control (Russell, 1999). In any case, existing pesticide regulation was based on assuring efficacy and controlling residues on food, not on environmental impact, and military studies of DDT did not deal with hazards to wildlife. Moreover, in any situation where a problem has not been widely recognized, the initial recognition will inevitably involve anecdotes, case reports, and circumstantial evidence.

But anecdotes are not necessarily false, and Carson's work was also based on her reading of a growing scientific literature. These studies documented accumulating evidence of harmful effects. Carson was reporting to the public what many scientists were seeing in their day-to-day work and reporting in specialist journals. Much of her discussion drew on articles published by wildlife biologists who had witnessed the effects she now summarized. Perhaps for this reason, Carson was firmly supported by many in the scientific community, particularly biologists. Oceanographers who had come to know her through her earlier book, *The Sea Around Us*, were also generally supportive.

In 1962 President John F. Kennedy decided that the circumstances warranted a review of government environmental policy. The President's Science Advisory Committee (PSAC) reviewed the topic. According to historian Zuoyue Wang, the scientists on the committee undertook to examine the problem in a considered manner, and to "take real actions to understand and control the effects of pesticide use" (Wang, 1997, p. 145).

In May 1963, only a year after *Silent Spring* was published, PSAC issued its report, "Use of Pesticides" (President's Science Advisory Committee, 1963). The report is a striking contrast with comparable documents from our own times, both for its brevity and clarity. Only 23 pages long, it frankly acknowledges the trade-offs involved in all human activities, and endorsed the use of pesticides in principle, which have been "remarkably effective . . . in facilitating both the control of insect vectors of disease and the unprecedented production of food, feed, and fiber . . . [The] use of pesticides must be continued if we are to maintain the advantages now resulting from the work of informed food producers and those responsible for control of disease" (1963, p. 1). The report is in no way counter-cultural: it contains no general criticism of industrialization, capitalism, or American life. It acknowledges that there are legitimate interests on both sides of the debate.

Nevertheless, the panelists felt that pesticides might be doing more harm than good. "Proper use is not simple," they wrote, and pesticides may also be "toxic to beneficial plants and animals, including man" (1963, p. 1). There were increasing signs that this was indeed the case, based on rapid increase in use, growing resistance in pest populations, and

the residues of persistent pesticides in food, wildlife, and the adipose tissues of people in the US and Europe. Pesticides appeared to be everywhere, and "although they remain in small quantities, their variety, toxicity, and persistence are affecting biological systems in nature and may eventually affect human health." Concern over adverse effects was "no longer limited to citizens of affected areas or members of special-interest groups" (1963, p. 4).

The panelists noted that their charge was difficult: to contrast obvious, rapid benefits with subtle, long-term risks. Moreover, their task was confounded by a multitude of uncertainties. These included the gap between data on acute exposure (whose risks were not disputed) and chronic effects; the lack of information on synergistic effects; and the fact that existing data probably under-reported adverse effects, as most doctors were ill-equipped to recognize sub-acute pesticide poisoning. Moreover, most of the available data involved animals, rather than humans. Experiments on laboratory animals showed that small doses could cause liver damage, but "the mechanisms leading to these effects are unknown" (1963, p. 12).

Despite these uncertainties, the panel broadly endorsed Carson's concerns, and called for greater control of pesticide use. The evidence of damage to wildlife was clear and compelling, they concluded, even in cases of "programs carried out exactly as planned" (1963, p. 10). Pest control programs had produced significant collateral damage to birds and fish, such as the loss of "[a]n entire year's production of young salmon . . . in the Miramichi River in New Brunswick in 1954, and again in 1956," and the loss of robins "after Dutch elm disease spraying in certain communities in Wisconsin and Michigan" (1963, p. 11). Like Carson, the PSAC committee accepted these case reports as legitimate evidence of harm, which might in time spread to human populations. Indeed, they noted that wild animals were likely to reveal the effects of bioaccumulation before humans did, because the human food supply was regulated.

While not dismissing the prospect for future scientific technological improvements, such as increased use of biological pest control and improved breeds of resistant crops, the panel concluded in favor of immediate action to restrain pesticide use:

Precisely because pesticide chemicals are designed to kill or metabolically upset some living target organism, they are potentially dangerous to other living organisms The Panel is convinced that we must understand more completely the properties of these chemicals and determine their long-term impact on biological systems, including man. The Panel's recommendations are directed toward these needs, and toward more judicious use of pesticides or alternate methods of pest control, in an effort to minimize risks and maximize gains. They are offered with the full recognition that pesticides constitute only one facet of the general problem of environmental pollution, but with the conviction that the hazards result-

ing from their use dictate rapid strengthening of interim measures until such time as we have realized a comprehensive program for controlling environmental pollution (President's Science Advisory Committee, 1963, p. 4).

The panel made numerous specific recommendations: the re-evaluation of some pesticides already on the market and increased stringency in the approval of new ones; increased enforcement powers for the FDA; transfer of authority for non-food pesticides out of the Department of Agriculture and into the Department of Health, Education, and Welfare (HEW); and the involvement of the Secretary of the Interior . . . “in review of all registrations that may affect fish and wildlife” (1963, p. 18). Once authority was transferred, HEW should undertake comprehensive studies of occupational and environmental exposures, and implement monitoring programs for air, water, and soil. Finally, the entire federal pesticide program should be reviewed with the view that some federal pest control programs “should be modified or terminated.” The “present mechanisms” for evaluating pest control programs, they concluded, “are inadequate” (1963, p. 20).

As might be expected from a panel of scientific experts, they also recommended further study, as an adjunct to policy action. As more was learned, actions could be modified—what today we would call adaptive management, although the panel did not label their strategy, perhaps considering it simple common sense. They particularly recommended more study of alternative chemicals and non-chemical pest control; of toxicity in man, especially chronic and reproductive effects; of synergisms and potentiation of effects of commonly used pesticides with each other, and with commonly used drugs such as sedatives, tranquilizers, and analgesics. And they asked for funding for all this, noting that federal programs were financially skewed towards pesticide use: “Approximately US\$ 20 million were allocated to pest control programs in 1962, but no funds were provided for concurrent field studies of effects on the environment” (1963, p. 22).

Finally, the panel demanded stronger enforcement of existing laws and improved mechanisms in evaluating manufacturers' safety claims. In the past, manufacturers had not been required to provide details on how they tested their own products, “and the FDA had no subpoena power to require testimony not voluntarily offered [in the registration process] After reviewing the data on which tolerances are based, the panel concludes that, in certain instances, the experimental evidence is inadequate The Panel believes that all data used as a basis for granting registration and establishing tolerances should be published, thus allowing the hypotheses and the validity and reliability of the data to be subjected to critical review by the public and the scientific community” (1963, p. 17). With further study while stricter regulations were put in place, the United States could and should achieve “orderly reduction in the use of persistent pesticides” (1963, p. 20).

So much for what PSAC did, but equally noteworthy is what it did not do: The committee did not take sides, but neither did it dither. The members acknowledged legitimate values on both sides of the issue—enhancing human food supply, protecting the non-human environment—and were dismissive of neither. They framed their problem within an unapologetically anthropocentric context, yet allowed for the importance of the non-human domain, ultimately recommending action that they felt balanced these contrasting domains of concern.

PSAC never claimed that the hazards of persistent pesticides were “proven,” “demonstrated,” “certain,” or even well understood; they simply concluded that the available data were adequate to show that harms were occurring, warranting changes in the pattern of pesticide use. They also noted that environmental concerns other than pesticides might actually be more serious, but they did not use this to deflect attention from the issue with which they were charged. They took seriously the idea that alternatives to pesticides, such as biological pest control, might be efficacious. They were not dismissive of such alternatives, and they did not accuse Carson and her supporters of harboring hidden agenda. Finally, they did not let a lack of scientific understanding of the *mechanisms* of pesticide damage stop them from accepting the *empirical* evidence of these effects. They called for more study, but they did not use uncertainty as justification for inaction. Policy was made; action was taken. Whether the panel was “right” in all their conclusions and whether the policy adopted was “correct” is a matter for evaluative hindsight—and one's judgment will depend in part on one's own value commitments—but right or wrong, these actions stand in contrast to current strategies of delay on grounds of scientific uncertainty.

An interesting question to consider is where the PSAC members placed the burden of proof—and why. The committee report explicitly invoked the rhetorical standard of reasonable doubt (their words), and placed the burden of proof on those who argued that persistent pesticides were safe. In their conclusion, a pesticide should be restricted or disapproved for use if there was “reasonable doubt” of its safety (1963, p. 20). In making this choice, they implicitly invoked a normative standard: denying privilege to the status quo, and placing responsibility on pesticides manufacturers. Without more detailed research it is not possible to say what arguments guided their thinking, but the use of the legal phrase “reasonable doubt” suggests that they may have been guided by existing legal frameworks, such as the landmark federal Food, Drug and Cosmetic Act (1938), which placed the burden of proof on manufacturers to demonstrate the safety of their products, and the Miller Amendment to it (1954), which extended the Act's reach to pesticides.⁶

⁶ In this sense, PSAC's charge was perhaps less ambiguous than the charges to some advisory committees today: an existing legal framework placed the burden of proof on manufacturers to demonstrate the safety and efficacy of their products. It is not clear what the parallel would be today in the case of global climate change.

The PSAC report helped to move the legislative process forward. In the years that followed, the US government passed a set of laws, such as the Clean Air Act (1970), and established a number of agencies, such as the National Institute for Environmental Health Sciences (1969), designed to address environmental issues, culminating in the establishment of the US Environmental Protection Agency (1970) (Graham, 1970). In 1972, 10 years after the publication of *Silent Spring*, the general use of DDT in the United States was banned (Dunlap, 1981; Lear, 1992; Wang, 1997; Environmental Protection Agency, 2003).

Whether this was the “right” or “wrong” decision, it is a clear example of public policy implemented on the basis of scientific knowledge that was neither proven nor certain, but that reflected a consensus of expert scientific opinion. PSAC was composed of nine prominent scientists in the United States, who listened to testimony from leading experts. While these experts were not unanimous in their views, the PSAC report reflected the committee’s assessment of the weight of relevant scientific opinion. That weight supported the banning of DDT. This is not to say that no one opposed the ban—quite the contrary—or that there aren’t some individuals today who continue to question its wisdom—there are. It simply shows that informed public policy was implemented based on a consensus of relevant scientific experts, a consensus that was accepted by politicians with the authority to act upon it, and with which the public by and large appears to have been content.

Our analysis could of course go deeper. In particular, it would be well to better understand how President Kennedy—as well as Eisenhower before him and Johnson and Nixon after—created a panel that was widely accepted by both scientists and by members of Congress from both major political parties as reflecting legitimate, non-partisan, relevant expertise. The fact that this happened might refute the claim made by Daniel Sarewitz (this volume) that “maybe there is something about science that lends itself to being politicized?” Rather, it suggests the need for an analysis of the historical circumstances under which scientists have been accepted as effective and reliable independent arbiters of information. In the 1960s this happened frequently, but today it does not. What has changed?

Even without such a deeper historical analysis, we can make the point that PSAC made a recommendation, based on what was both accepted then and still appears in hindsight as the consensus of expert scientific opinion, despite some open dissent and acknowledged uncertainty. The policy that resulted was successful in addressing public concerns, and it was based neither on an abstract notion of proof, nor on a demand for certainty, but on the weight of considered scientific opinion and public concern. This may not be the hard and fast principle that some people want, but it is the reality of how things can work in practice—at least under the right conditions. How to create and sustain such conditions is a topic for another paper (and perhaps another scholar).

5. Different experts weigh evidence differently

The scientific community was divided about Carson’s claims, just as earlier earth scientists were divided over Wegener’s. In the case of continental drift, the divide was partly geographic: scientists who lived, worked, or traveled in the southern hemisphere, where the empirical evidence was strongest, were more likely to accept it than those who had not. In the case of DDT, the divide was weighted along disciplinary and institutional divides: biologists, oceanographers, the Department of Interior and PSAC generally affirmed Carson’s concerns, chemists, food scientists, and US Department of Agriculture scientists generally did not.

This is a common pattern in scientific debates: specialists from different locales and in different fields weigh evidence differently (see also Sarewitz, 2004). Elsewhere I have argued that scientists have epistemological affinities and chauvinisms, based on education and training, personal affiliations and loyalties, and their philosophies of science (Oreskes, 1999, pp. 51–53). These preferences and prejudices affect how scientists weigh evidence, with a tendency to give greater weight to evidence that is near to hand, with ‘nearness’ being experienced physically, socially, and epistemologically.⁷ As the seismologist Charles Richter once put it, “We are all best impressed by evidence of the type with which we are most familiar” (Richter, 1958; see also Oreskes, 1999). Epistemological affinities can be found in any debate—not merely politically charged ones—but they take on added fervor when scientific debate spills

⁷ My argument here is different from that of Harry Collins, who has argued that “certainty about natural phenomena tends to vary inversely with proximity to the scientific work” (because those closest to the work know precisely what is wrong with it), and Donald McKenzie, proposes a certainty trough wherein people who are very close to scientific work are skeptical for the reasons Collins lays out, and those who are very far from it are skeptical because they may be alienated from it or working in opposition to it. Thus, the greatest certainty is found among those who understand, support, or use the work, but are not actually engaged directly in producing it (see MacKenzie, 1990, pp. 371–372). While I agree that these kind of factors are often in play, my argument is different: When it comes to evaluating conflicting evidence, people tend to trust evidence of the kind which they and their close colleagues have dedicated their lives to obtaining, in part for social reasons, and in part because they have an intellectual, aesthetic, or ethical affinity for that *kind* of scientific work, which helps to explain why they chose to pursue that kind of research in the first place. Often these commitments are both affective and epistemic. Field scientists *like* field work—they like being out in the fresh air and sunshine—and they also believe it to be more likely to capture the basic truths about the natural world, messy though it may be. In contrast, laboratory scientists enjoy working in the lab—they enjoy building and tinkering—and they also believe it to produce knowledge of greater specificity and rigor than field science. While Collins is right that such scientists are well placed to know what is wrong with a particular investigation, they may be equally able to reassure themselves that the difficulties are minor, and soon to be resolved. Scientists may also choose a particular line of inquiry because it aligns with their normative commitments: (field biologists caring about nature, economists caring about the efficient management of monetary resources), in which case they are apt to defend their work strongly on (implicit) normative grounds.

over into the public arena, and an added dimension when financial or political interests are at stake.

In hindsight, the extra-epistemic interests of Carson's critics are obvious—many had ties to the pesticide industry—so we might dismiss them as handmaidens of that industry. That would be a mistake, for it would obscure the fact that *all* debates involve underlying commitments, and clarity requires addressing those commitments. Like Mrak, many of Carson's critics were food scientists dedicated to a large, inexpensive food supply. Like Bjørn Lomborg they were not ashamed to value immediate human needs over long-term ecological concerns. Carson's historical critics thus lie in conceptual alignment with Lomborg today, who is explicit that his focus is on “[c]ounting [human] lives lost from different problems (Lomborg, 2001, p. 11).

Indeed, Carson's critics accused her of indifference to human fate, writing to the tune of the folk song, “Reuben, Reuben,”

Hunger, hunger, are you listening,

To the words from Rachel's pen?

Words which taken at face value,

place lives of birds ‘bove those of men” (Lear, 1992).

Lomborg implicitly makes the same criticism of current environmentalists, placing his valuation almost exclusively on humans. In considering only humans—rather than plants, animals, or even Earth as a whole—he acknowledges “a central assumption in my argument: that the needs and desires of humankind represent the crux of our assessment of the state of the world [T]he focus will always be on the human evaluation” (Lomborg, 2001, p. 11).

While there are certainly environmentalists who share Lomborg's focus on humans, there are many who don't, and it is both logically possible and ethically plausible to reject the premise that human life is the measure of all things. Consider the example of biodiversity.

Many ecologists have emphasized ecosystem services as justification for biodiversity preservation: that biodiversity is needed to preserve the conditions under which human life thrives. But what if it could be shown that humans could live perfectly well in a world with a greatly reduced number of species, that the required ecosystem services could be provided by monoculture tree plantations, golf courses, front lawns, and the like? Would we then accept biodiversity loss? By Lomborg's argument, the answer should be yes, thus illuminating a fundamental limitation of his argument, for life is more than the sum of ecosystem services. A rare flower may be beautiful even if its contribution to atmospheric oxygen is negligible; a venus fly trap may thrill us even if it does little to protect from malaria-carrying mosquitoes.

Indeed, the very word service reveals a kind of consumerist bias—as if life were a matter purchasing services from the natural world.

Ultimately the gap between the perspective for which Lomborg argues and that advocated by Carson boils down to the familiar, yet still important, distinction between quantity and quality. This point is evident when we consider that Lomborg's focus is not just on any human concerns, but on dimensions that can be *quantified in terms of individual human lives lost* (or saved). Such measures obviously say nothing about the quality of those lives, yet quality of life is precisely what traditional conservationism was historically concerned with, and what many would claim is at stake in environmental policy debates today.

Rachel Carson was *not* indifferent to humans—a good deal of her discussion was about bioaccumulation and its potential affects on the human food supply. Nor was it clear that indiscriminate pesticide use was required to address world hunger, anymore than than it is now, nor that DDT was the best means to malaria eradication. But while Carson *was* concerned about humans, she was also concerned about non-human nature. Even if DDT had been utterly harmless to people, Carson's point would have stood: that DDT was doing serious harm to the natural world. Carson's preceding book was entitled *The Sea Around Us*, and *Silent Spring* could have been entitled *The World Around Us*. Carson's concern was with the ethics of eradicating whole species of birds, whether or not they were of use to us, and of leaving to our children a world that was ecologically impoverished. This is a point that Lomborg seems to miss—or dismiss. While his emphasis is on counting, Carson's argument was about things that can't be counted, yet still count.

6. From DDT to global warming: the unfulfilled promise of ATOC

We can see why proof might not be required in politics if people get sufficiently fired up about something and the risk of inaction is perceived to be great. But surely we should seek proof if we can? Surely it is better to have proof than not to have it, particularly when mitigation will be costly? In the current highly contested domain of climate change, where mitigation will likely require changes in the patterns of life in the industrialized world, we might think that environmentalists would welcome a definitive demonstration that climate change has in fact occurred, but consider ATOC—Acoustic Thermometry of Ocean Climate.

The technical dimensions of global climate change can be reduced to two simple questions: Is Earth warming up? If so, how does this change compare with the historical variability of Earth's climate, before humans started to substantively alter their world? Recent reports of the Inter-governmental Panel on Climate Change accept that there has been an increase in global average temperatures of approximately 0.5°C since the industrial revolution, but the data are noisy, and temperature variations larger than this have been a normal part of geological history (Houghton et al., 1995, 2001).

To obtain global averages from historical records involves numerous inferences and assumptions: old records are of variable quality and geographically clustered, and there is no thermometer that permits us to measure directly the average temperature of Earth, itself a highly abstract and constructed concept.

But what if we really could take a measurement of Earth's temperature? This was the idea of a group of scientists in the late 1970s, led by oceanographers Walter Munk and Carl Wunsch. While many factors generate fluctuations in atmospheric temperatures that complicate assessments of global averages and trends, the oceans present a more tractable situation. The high heat capacity of water, combined with global ocean circulation, makes the oceans a robust sink for planetary heat. In comparison with the atmosphere, temporary fluctuations are damped and long-term patterns should be more readily assessed. Although different ocean basins behave differently, viewed collectively they are an important indicator of global patterns.

On the other hand, the problem of how to measure the average temperature must be addressed in the hydrosphere equally as in the atmosphere; one can no more stick a thermometer into the ocean to get a global average than into the air. Here the oceans present a second advantage: the speed of sound in water is directly dependent on the water temperature. A long-range transmission, say from La Jolla to Honolulu, provides an integrated assessment of the thermal conditions of the water between those two points. In this way, acoustics can provide information on the large-scale thermal structure of the oceans, without being overly affected by temporally or geographically local fluctuations. (In particular, the integrating effect of tomography dampens the 10–100 km scale of ocean “weather,” that dominates the temperature variability spectrum.) Take measurements at strategic locations across the world's oceans, and you come close to measuring the whole world ocean temperature. Do this repeatedly over the course of several decades, and you may have an answer to the question of whether Earth's oceans—and therefore Earth—is warming up, independent of noisy and perhaps unreliable instrumental temperature records and unverifiable climate models.

The scientists involved originally dubbed this the “ocean acoustic thermometer” (Spiesberger et al., 1983); in time it became known as Acoustic Thermometry of Ocean Climate (ATOC). Like the sea-floor magnetic stripes that revealed plate motions, the acoustic thermometer was admittedly indirect—measure sound velocity and from that calculate water temperature—so one's conclusions could be only as good as the science of underwater acoustics. But that science was very, very good. Besides nuclear physics, few subjects in 20th century physical science had been studied in as great detail. Since World War II, and throughout the Cold War, the US (and other countries) had put enormous resources into the understanding of underwater sound transmission for its use in pro- and anti-submarine warfare. During World War

II, the study of underwater sound transmission had been a major initiative of the National Defense Research Committee (National Defense Research Committee, 1944; Ewing and Worzel, 1945; Eckart, 1968; Research Analysis Group, 1969) for its use in submarine hiding and tracking. With the Cold War development of SOSUS (the SOund SURveillance System)—the secret US underwater ocean acoustic system that tracked the activities of Soviet submarines—and submarine launched ballistic missiles, these research programs continued to flourish throughout the 1950s, 1960s and 1970s (Frosch, 1964; Urlick, 1979; Spiess, 1997). Over the course of nearly half a century, physical oceanographers had become intimately familiar with the physics of underwater sound. While salinity and currents also affect ocean temperature, it was well established that these effects were secondary (Munk and Wunsch, 1979). Because of the high heat capacity of water, the ocean is a significantly larger reservoir of global heat storage than the atmosphere. So one might reasonably say that $\Delta T_{\text{Ocean}} = \Delta T_{\text{Earth}}$.

The link to military projects was not just in terms of the knowledge base; the ATOC program would also draw on military hardware. The SOSUS network would provide the equipment needed to detect the sound transmissions—which were no different from the transmissions used for military surveillance—and early work was funded through the US Navy Office of Naval Research (Woods Hole Oceanographic Institution MC6, 1983). Later, scientists received funding from the Strategic Environmental Research and Development Program, created to make military systems available for civilian scientific research (Potter, 1994). By using the SOSUS network, the scientists would rely on a technology whose reach was global, and that was well tested, well maintained, and well-understood theoretically.

From the start, the scientists involved recognized the relevance of their proposal to the “big question” of global warming. As Woods Hole oceanographer John Spiesberger wrote to oceanographer Henry Stommel in 1989, “our intention [was] to set up acoustic observations to detect hypothetical greenhouse effects on climate change” (Woods Hole Oceanographic Institution MC6, 1989). Answers would not be obtained quickly, but would require persistent measurements over decades. “One can imagine measurements extending for 100 years or more where perhaps the gradual heating of the oceans due to the increase of CO₂ could be detected. Just as astronomers have established observatories where measurements have been taken for hundreds of years, the oceanographers might establish an acoustic observatory of the type described herein” (Spiesberger et al., 1983). Like the proponents of the Worldwide Longitude Operation, these scientists took the long view, envisaging a research program in which oceanographers would answer fundamental questions about the Earth, just as astronomers over the centuries had answered fundamental questions about the heavens. For the less patient, Walter Munk pointed out that it was not necessary to wait for centuries; a decade of measurements would be sufficient to detect the predicted

warming effect (Munk and Forbes, 1989). In 1991, the Heard Island Feasibility Test demonstrated that the transmissions could indeed be detected at global ranges and a meaningful signal obtained (Munk et al., 1994, 1995).

ATOC was a clever, creative, and insightful proposal to apply basic scientific knowledge to answer a significant environmental question, but this promising avenue of inquiry hit a wall of controversy when biologists suggested that the sound signals might injure marine mammals. The ATOC permit requested permission for a “take”—defined as any injury or harm—of a variety of marine mammals, including whales, dolphins, seals and marine turtles, and encompassing several threatened or endangered species. Potentially, several hundred thousand marine mammals could have been affected (Potter, 1994). While the word “take” in this context meant any effect, no matter how small, and the application insisted that any effects would be transitory and minor, some biologists disputed the grounds for this optimistic assessment. Louis Herman, Director of the Kewalo Basin Marine Mammal Laboratory, Honolulu, noted that the ATOC signal fell within the frequency band of the humpback whale song, which might render the songs less detectable or even unrecognizable (Herman, 1994). The ATOC permit included a plan to monitor possible effects on humpback whales, but acknowledged that long-term effects would be difficult to detect. Yet it was precisely such long-term effects, Herman noted, that “are of the greatest concern” (1994, p. 65).

In 1994, a consortium of environmental groups, including the Natural Resources Defense Council, the Environmental Defense Fund, and the Humane Society of the United States, filed suit to stop the project. The plaintiffs accused the researchers of violating the National Environmental Policy Act, the Marine Mammal Protection Act, and the Endangered Species Act. Scientists who saw themselves as addressing a significant environmental question were cast by their opponents as environmental villains. As word of the project spread, opposition grew among marine biologists, conservationists, and, especially, aficionados of whales. As one conservationist put it, “whale lovers went wild” (Rose, 2001). Led by Dalhousie University biologist Hal Whitehead, opponents of the project took to the internet, drawing on a listserv of persons interested in marine mammals (marmam@uvvm.uvic.ca), which had over 1500 subscribers. (A search of this web site on 16 January 2001, turned up 1937 messages under the heading “ATOC.”) The issue became heated as the story was picked up by California newspapers, and US Senators Dianne Feinstein and Barbara Boxer asked then-Commerce Secretary Ronald Brown to block the approval of the necessary permits.

The negative publicity was abundant and intense. As the media pursued the story, press releases from the Scripps Institution of Oceanography denied that the transmissions would harm marine life, noting that the sound from the project would be only a marginal addition to the noise that already filled the oceans. Rather than placating opponents, these press releases inflamed them, as they seemed to

dismiss the conservationists concerns as irrational and to justify further harms on the basis of past ones. While the physical oceanographers involved in the project insisted that no harm would be done, some biologists began to question whether physical oceanographers were qualified to make that judgment. Oceanographers’ proposals to monitor effects during ATOC transmissions seemed to miss the point: if harm was detected, then harm would have been done.

After 18 months of intense controversy, the plaintiffs and defendants agreed to an out-of-court settlement, establishing a Marine Mammal Research Program (MMRP) to test the claims that the transmissions would not affect marine mammals, monitored by an independent advisory board of marine mammal experts. In the spring of 2000, a Draft Environmental Impact Statement (DEIS) was released for public comment, and the National Research Council issued a report reviewing the status of the project. The NRC report stated that the MMRP had found no statistically significant effects, but it was not possible to determine whether this was because there were no effects or because there were insufficient data to detect any effects (National Research Council, 2000). Meanwhile some marine biologists continued to oppose the project. In September 2000, after expiration of the DEIS public comment period, Canadian biologist Paul K. Anderson wrote a scathing denunciation, which he made public via the internet. Effectively accusing ATOC supporters of dishonesty, he wrote:

Both the DEIS and the small take permit application pretend that the Acoustic Thermometry of Ocean Climate Marine Mammal Research Project effectively dispelled any concerns as to the effect of these sounds on marine mammals. [T]he ATOCMMRP not only did not demonstrate long-term effects, but . . . it failed to adequately investigate short-term responses. The proposal for continuation of ATOC is based on false [premises] (Anderson, 2000).

While scientists continued to try to address the environmental issues throughout the late 1990s, by the end of the decade the project was grinding to a halt. In 1999, the initial permits were not renewed, and the scientists were required to remove their instrumentation. The project ended on a tragic note in August 2000, when an ATOC source was being retrieved from Pioneer Seamount near Half Moon Bay, CA. While no whales were known to have been killed during the course of the project, one man was: a winch operator named Ron Hardy, struck in the head by a piece of equipment while trying to remove a 12,000 pound transmitter off the sea floor (Worcester, 2000).

6.1. *Why did environmentalists oppose ATOC?*

The ATOC scientists were stunned by the opposition of environmentalists, which they considered wrong-headed and ill-informed. Oceanographers felt that environmentalists had misunderstood the project, that the risks had been grossly

exaggerated, and that the news media had misrepresented the permit language of “taking” to mean “killing” (Potter, 1994; Munk, 2003). Most felt that environmentalists should welcome the project, because it was motivated by an environmental concern. Why didn’t environmentalists see it that way?

One reason is clear: most environmentalists already accepted that global warming was real. They did not need more information to be convinced, and therefore were not interested in accepting risks to get that information (Potter, 1994). Moreover, while scientists were proud of the “swords to plowshares” aspect of the projects, for many environmentalists the military association was grounds for suspicion. In the words of oceanographer Stanley Flatte, “folks thought it was some kind of secret Navy project” (Flatte, 2000). Even if the project were what it claimed to be, the US military has not been not known for its history of environmental sensitivity, and in the past has been exempted from much environmental regulation and indemnified from litigation. To environmental activists, the US Navy as steward of the environment was simply not plausible.

One argument in defense of the project was that the US Navy had been using this sort of acoustic transmissions for decades, but this did little to satisfy environmentalists for whom such an argument merely proved the point: that the Navy was used to operating without environmental oversight. Naomi Rose, a biologist with the Humane Society, put it this way. “The oceanographers asked: ‘Why would you even think we would hurt the environment?’ and environmentalists responded, ‘Why would we think you wouldn’t?’” (Rose, 2001). From the perspective of environmentalists, the scientists were aligned with a Goliath who had run rough-shod over the environment in the past, and would likely do so again in the future.

And what if ATOC “proved” that there was no climate signal? Then what? Put another way, why should anyone accept any particular line of evidence as a scientific trump card? After all, is there really such a thing as direct scientific evidence, or is it simply that the ambiguities inherent in some forms of evidence are more evident than in others? That some forms of data production are more transparent than others?

Consider again the Worldwide Longitude Operation. The latter was promoted as the direct measurement of continental motion, and therefore less ambiguous than the various indirect, largely historical, arguments that supporters of drift had used. But if the project had continued uninterrupted by world war, yet failed to detect the continental motion, what would scientists have concluded? That drift had not occurred? Would they then have dismissed the other evidence of drift as disproved? Or might they have questioned the experiment, wondering if it there were a mistake in it somewhere? Both options would have been possible, because the Longitude Operation, like all scientific experiments, was based on certain premises, certain background assumptions. In this case, those assumptions included, among other mat-

ters, premises about how radio waves travel through the atmosphere: for the experiment to have worked, those travel paths would have had to have been unaffected by ionospheric fluctuation. Today we hold that radio waves travel paths are affected by ionospheric fluctuations. Moreover, the Operation was based on assumptions about the rate of drift—tens of meters per year—that turned out to be much too high. In hindsight, the Longitude Operation was doomed to fail—World War II or no.

ATOC was similarly touted as a direct measurement of changing ocean temperature, more reliable than historical climate records. But, as in the Worldwide Longitude Operation, the proposal involved various assumptions: about sound travel paths, about the accuracy of signal processing, and about the reliability and consistency of instrumentation. No matter how good the science, there are always uncertainties.

Experimental premises may be faulty, limited, or incomplete. Instruments may not be sensitive enough to detect faint signals. Theoretical understandings may turn out to be erroneous. One independent paper evaluating the feasibility of the ATOC approach concluded that, when all the uncertainties were considered, there was a “realistic chance of detecting the expected greenhouse-induced warming in the World Ocean” (Mikolajewicz et al., 1993). One could equally conclude from such language that the prospect of *not* detecting the expected signal was also ‘realistic.’ The scientists involved in ATOC emphasized how well understood the basic physics were, but a project like this is never simply a matter of basic physics. Environmentalists never said so explicitly, but they might reasonably have viewed ATOC as a Trojan horse—trouble masquerading as a gift.

Finally ATOC ran aground simply because people will go to great lengths to protect the things they love. As Paul Anderson put it, “It is the misfortune of physical oceanographers that the sea contains organisms that are culturally valued, and ecosystems and populations of ecological and economic importance” (Anderson, 2000). From the perspective of oceanographers, the objections to ATOC may have seemed irrational, but consider a mother bear who charges a solitary hiker. The hiker has no gun and no intention of hurting her cubs, but she does not know that. From her perspective, she’s not taking any chances. What may be presented as a scientific problem—a matter of technical facts—reveals itself to be a question about which particular chances we are prepared to take.⁸

7. What happens when scientists don’t agree?

Most of us realize that proof—at least in an absolute sense—is a theoretical ideal, available in geometry

⁸ This of course is the logical problem with the “precautionary principle.” Different people will want to take different precautions based on what consequences they fear or dislike most. On this, see Wynne (1992) and Pielke (2002).

class but not in real life. Nevertheless, many of us still cling to the idea that some set of facts—some body of knowledge—will resolve our problems and make clear how we should proceed. History suggests otherwise: earlier scientific wisdom has been overturned, earlier generations of experts have made mistakes. This is as true in physics and chemistry as in biology and geology. The criteria that are typically invoked in defense of the reliability of scientific knowledge—quantification, replicability, falsifiability—have proved no guarantee.

Moreover, experts do not always agree. Even when there is no transparent political, social, or religious dimension to a debate, honest and intelligent people may come to different conclusions in the face of the “same” evidence, because they have focused their sights on different dimensions of that evidence, emphasizing different elements of the evidentiary landscape. Even when a scientific community reaches consensus on a previously contested issue—as earth scientists did in the 1960s over moving continents—there are always dimensions that remain unexplained. In the future, plate tectonics no doubt will be modified, perhaps overturned entirely. Indeed, there are a handful of scientists today who advocate Earth expansion to explain continental separation, and they are of course eager to detail the limitations of plate tectonics theory (e.g. Shieds, 2003). Nevertheless, for now plate tectonics remains the consensus of most Earth scientists: our best basis for understanding the Earth.

Contrary to the Thomas Kuhn’s widely accepted theory, anomalies are always hovering about, even in ‘normal science.’ Scientific consensus is a complex process—involving a matrix of social, political, economic, historical considerations along with the epistemic—and history shows that its achievement typically requires a long time: years, decades, even centuries. But even when a stable consensus is achieved, scientific uncertainty is not eliminated. Rather, once we have deemed the remaining problems as “minor”—which is to say, insufficiently great as to warrant further concern—we simply live with them (Engelhardt and Caplan, 1987). Moreover, the grounds on which scientific communities have concluded that evidence is “good enough” to warrant living with the uncertainties have varied enormously throughout the course of history. A determined individual may choose to pursue these uncertainties, and that determination may successfully destabilize the prior consensus. In a “purely” scientific debate, that determination would, ideally, arise solely from the demands of empirical evidence, but no debate is ever “purely” scientific, given that, at minimum, credibility, reputation, and, perhaps future funding are at stake.

When there is a policy dimension to a scientific debate, we can expect such determination to be common, as scientists pursue issues whose importance is measured against a backdrop of political significance, as the media focus attention on ‘mavericks,’ and as money flows into scientific research from parties with stakes in the outcomes. Louis Pasteur noted this phenomenon long ago, writing in the

19th century about impassioned debates in the 18th over the reality of spontaneous generation: “Very animated controversies arose between scientists then as now—controversies the more lively and passionate because they have their counterpart in public opinion, divided always, as you know, between . . . great intellectual currents” (Geison, 1995). Conversely, when there is a scientific dimension to a policy debate, we can expect that science may be used as a basis for competing political or moral claims (Nelkin, 1995; Herrick and Sarewitz, 2000; Jamieson, 1996; Sarewitz, 2004).

How can we evaluate scientific claims when scientists themselves don’t agree? There is no good answer to this question, but certain perspectives may help us to judge the information we are receiving. First we may ask: Who are the relevant experts? Or better, what is their expertise relevant to? In the case of DDT, food scientists were qualified to speak to the agricultural benefits of DDT, and wildlife biologists were better placed to speak to the ecological harms. In this sense both sides were right in what they affirmed but wrong in what they denied, and ultimately the question was not so much who was ‘right,’ but which set of concerns—an enhanced food supply for humans or greater protection of wildlife—would be viewed as more pressing.

A similar point can be made about the ATOC case. Physical oceanographers are experts in the make-up, behavior, and properties of the ocean as a body of water, but not as an abode for life. They could discuss the potential results of the ATOC experiment, but cetacean biologists were better qualified to consider its potential effects on whales. If the question at stake was—will the ATOC experiment provide useful information about global climate?—oceanographers were the relevant experts to answer that question. If the question was—what will the effect of the ATOC experiment be on whales?—then biologists were the relevant experts.

Biologists do not map the distribution of earthquake hazard, endocrinologists do not forecast the weather, and chemists are not permitted to perform heart surgery. There are good reasons why. Traditional markers of expertise—training, experience, academic appointments and honors—are no guarantee of an expert’s honesty, integrity, or wisdom, but, *ceteris paribus*, it makes sense to trust those persons whose competence is closest to the question at hand. Why else do we support colleges, universities, and research institutes if not to develop and sustain expertise that we believe to be of value?

Expertise can of course be compromised and even bought outright, so we also need to ask: what are the non-epistemic interests of the experts? How might they be affecting the scientific results? All parties to debates have interests that condition their responses to evidence and arguments, so it is legitimate to inquire into those interests (Etzkowitz, 1996). The recent growth of corporate sponsorship of scientific research on university campuses has raised the question of how financial interests are currently shaping not merely the subject of scientific research, but also the outcomes.

An obvious and well-studied area is tobacco research, which, through its obviousness, drives home a point that may be less obvious yet still relevant elsewhere. Research by scientists at the University of California, San Francisco, has shown the ways in which the tobacco industry has tried to generate uncertainty over the issue of second-hand smoke by directly sponsoring scientific studies whose purpose is to destabilize the existing consensus. These studies are far more likely to find no evidence of ill effect than studies not funded by the tobacco industry (Hong and Bero, 2002; Shamasunder and Bero, 2002; Montini et al., 2002; Bero, 2003; Bryan-Jones and Bero, 2003). In an overall review of the effects of industry-sponsored research, Boyd and Bero (2000) conclude that research clearly documents “an association between single-source sponsorship of clinical research and publication of results favoring the sponsor’s product” (see also Stelfox et al., 1998; Angell, 2000). The critical point here is not that the fact the research was funded by industry, because all science is funded by some institution, group, or individual, and it’s not clear that industrial patronage is intrinsically more problematic than support from a prince, a foundation, an armed service, or a government agency. Rather, the issue is that the research is supported by a sponsor who wants a *particular* result—a particular *epistemic* outcome—and the researchers know in advance what that outcome is, producing an explicit conflict of interest, which undermines the integrity of the research performed.

This point brings us to what may be the most important point of this paper: scientific proof is rarely what is at stake in a contested environmental or health issue. Bjørn Lomborg’s focus is on humans—on quantitative measures of the conditions of life for the majority of persons on the planet—and given this perspective, many of his claims are surely right. More people do live longer, eat more calories, and have thicker roofs over their heads than was generally the case in the past. But many environmental claims are not so much about life’s quantities as its qualities. They are about aesthetic and moral choices. They are about equity and ethics. To be sure, we humans have enhanced our lives by controlling, diminishing, and even eradicating certain forms of non-human life, and few people would defend viral or bacterial rights. But increasingly our actions are impacting the Earth in ways that will affect future generations, who will have had no say in those choices and may be unable to undo them. In the past, human actions tended to be local and reversible, but increasingly our actions appear to be global and irreversible. As Roger Revelle astutely pointed out nearly 50 years ago, speaking of the human contribution of CO₂ to the atmosphere, we are performing a “a great geophysical experiment” on our planet without the consent or the knowledge of future generations, and which cannot be undone (Revelle and Suess, 1957).

Scientists debate unresolved epistemic and methodological issues in their own specialties all the time, but these rarely receive public scrutiny. Lack of consensus becomes a public issue when there is a public stake, which means

a moral, political, or economic stake. In such cases, natural science can play a role by providing informed opinions about the plausible consequences of our actions (or inactions), and by monitoring the effects of our choices (Herrick and Sarewitz, 2000). Social science can do the same. But there is no need to wait for proof, no need to demand it, and no basis to expect it.

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