



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 pro-

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¹ 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, 2001). 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.

58 duces a robust consensus based on a process of inquiry that
59 allows for continued scrutiny, re-examination, and revision.²

60 2. In a perfect world . . .

61 Lomborg's desire for truth-based policy can be reframed
62 as a vision of how policy would be framed and implemented
63 in a perfect world. In this perfect world, scientists collect
64 facts, politicians develop policies based on those facts, leg-
65 islators pass laws to implement these policies, and govern-
66 ment agencies enforce the laws, most likely through regu-
67 lations based on the same kind of facts. Because the laws,
68 policies, and regulations, are based on the truth, they work,
69 and our problems are solved: efficiently, effectively, and eco-
70 nomically. More subtly, we might say that science gives us
71 our most reliable understanding of the natural world, and
72 therefore provides the best possible basis for public policy
73 on subjects involving the natural world.

74 This has been the historical justification for science ad-
75 vice in government, a tradition that in the United States
76 goes back at least as far as the US Fish Commission, estab-
77 lished in 1871 to determine the causes of declines in fish
78 catches in New England and suggest appropriate remedies
79 (Allard, 1978; McEvoy, 1986; Smith, 1994). It provided
80 the justification for the creation of the National Academy
81 of Sciences and the National Research Council, and for
82 the great post-war expansion of science and inclusion of
83 scientists as policy advisors in the US government (Dupree,
84 1957). It remains the justification today for offices and
85 organizations such as the President's Science Advisory
86 Committee and the White House Office of Science and
87 Technology (Kevles, 1978; Snow, 1960; Price, 1962, 1965;
88 Smith, 1990). This perspective has also underpinned vari-
89 ous scientific initiatives in support of policy development,
90 such as the US National Acid Precipitation Assessment
91 Program of the 1980s and the Intergovernmental Panel on
92 Climate Change (Herrick and Sarewitz, 2000; Miller and
93 Edwards, 2001). Nowadays a common political response to
94 an environmental problem is to be to establish a scientific
95 agency, program, or initiative to investigate it.

96 While there are other kinds of political responses, it cer-
97 tainly is the case that environmental problems *can* be for-
98 mulated as scientific questions. In part this is because often
99 science establishes the problem *qua* problem. Who among
100 us would know there was global warming without scien-
101 tific evidence to that effect? Who would know that atrazine
102 might affect amphibian sperm? Who would know there was
103 MTBE in groundwater? Even if we were octogenarian farm-
104 ers in New England keeping weather almanacs and notic-
105 ing that winters seemed to be getting milder, how would we

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.

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

118 These questions invite answers, and recalcitrant actors
119 may present themselves as skeptics demanding proof. Partly
120 for this reason, many scientists have concluded that their task
121 is to provide the proof that society needs, via better climate
122 models, better biodiversity indices, better estimates of ocean
123 temperature, and so on (see, for example, Levitus et al.,
124 2000; Jackson and Johnson, 2001; Canham et al., 2003).
125 Once we have these, then we will know what action to take
126 and when to take it. Of course, we all know that the sciences
127 never provide absolute proofs, but nevertheless we look to
128 scientific research to provide the nearest approximation to
129 proof that we can obtain. We look to science to tell us if a
130 problem is real, and if so what to do about it.

131 The difficulty is this: proof does not play the role in sci-
132 ence that most people think it does (or should), and there-
133 fore it cannot play the role in policy that skeptics demand. In
134 this paper, I explore three examples at the nexus of science,
135 proof, and/or policy: one, an example where scientists suc-
136 cessfully forged consensus despite the fact that earlier ex-
137 pressed standards of proof had not been met; two, an exam-
138 ple where policy-makers successfully forged consensus de-
139 spite acknowledged uncertainties and disagreement by some
140 experts; and three, an example of scientists who tried to pro-
141 vide a convincing demonstration of an environmental effect,
142 but were vilified by environmentalists for the attempt. By
143 examining examples of past disputes, we can perhaps gain a
144 more realistic appreciation of what science can and cannot
145 do in aid of public policy.

146 3. From continental drift to plate tectonics: the proof 147 of moving continents?

148 When Alfred Wegener proposed continental drift in 1912
149 as a unifying theory of earth sciences, he also provided
150 abundant evidence of it (Wegener, 1912, 1915, 1924, 1929).
151 Besides the obvious "jigsaw-puzzle" fit of the continents,
152 data from paleontology, stratigraphy, and paleoclimatology
153 strongly suggested that the continents had once been uni-
154 fied, then broken apart, and drifted into their present config-
155 urations. Despite cavils over the details of the data by some
156 specialists, most of this evidence was broadly accepted as
157 factual by earth scientists, and had been used by other scien-
158 tists to support alternative explanatory frameworks (Marvin, 158

159 1973; Le Grand, 1988; Oreskes, 1999). Despite widespread
160 acceptance of the bulk of the evidence and widespread dis-
161 cussion of the theory, continental drift was generally re-
162 garded as unproven. What would have constituted proof?

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

174 Wegener's conclusion was not idiosyncratic; others also
175 believed that direct measurement of continental motions
176 constituted the definitive test. In 1926, a group of interna-
177 tional scientists organized the Worldwide Longitude Oper-
178 ation to prove or disprove continental drift by measuring
179 inter-continental distances through radio wave transmission
180 times. While the scientists involved were admirably patient,
181 after a decade the results were still inconclusive (Oreskes,
182 1999; Dick, 2003). Then global political events made further
183 work impossible.

184 In the late 1950s, the question of crustal motions was
185 re-examined. In the mid 1960s plate tectonics became the
186 unifying theory of earth sciences, and moving continents
187 became established scientific fact. By the early 1970s, text-
188 books had been rewritten in the framework of plate tectonics,
189 and historical treatments were being published (Cox, 1973;
190 Le Pichon et al., 1973; Hallam, 1973; Frankel, 1979, 1982,
191 1987; Laudan, 1980). Plate tectonics was now accepted by
192 scientists as true, but was it proven? Not by the standard
193 demanded in the earlier debate.

194 Like the evidence of continental drift, the evidence of
195 plate tectonics was indirect. It consisted of terrestrial rock
196 magnetism, which showed that the continents had altered
197 their positions vis-à-vis the magnetic poles, marine magnetic
198 measurements, consistent with the creation of new oceanic
199 crust at mid-oceans ridges and its lateral displacement, and
200 seismic first-motion measurements, consistent with large
201 crustal slabs moving outward from the mid-ocean ridges and
202 downward under the continents in subduction zones. Again,
203 the relevant inferences were abductive: these phenomena
204 were things that would be observed if plate tectonics were
205 true, and would be very difficult to explain if it weren't. Fi-
206 nally, the data became so abundant and the patterns so clear
207 that no one doubted that it was true. But scientists in 1960s
208 had no more direct evidence of continental motions than
209 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.

210 When *did* earth scientists finally measure continental mo-
211 tion directly? Nearly 20 years later. In the mid 1980s, very
212 long baseline satellite interferometry made it possible to
213 measure the distances between points on Earth with great
214 accuracy, and to detect small changes in these distances over
215 time. In 1985–1986, a series of papers reported the results,
216 and the general conclusion was that the drift of the conti-
217 nents was now proven (Christodoulidis et al., 1985; Clark
218 et al., 1985; Kerr, 1985; Herring et al., 1986). Given this,
219 it could be argued that for 20 years, earth scientists used,
220 taught, and believed in the fundamental truth of plate tec-
221 tonics without “proof” that plates were moving. Were they
222 wrong to do so? Was this bad science? Of course not. The ev-
223 idence of plate tectonics was sufficiently overwhelming that
224 direct measurement of continental motion was not required.
225 Plate tectonics was not proven by the standard proposed by
226 the advocates of the Worldwide Longitude Operation, but
227 it nevertheless met the standards of earth scientists in the
228 1960s, who forged a consensus around it. Geodesists in the
229 1980s received relatively little attention for their work, be-
230 cause they had “proved” what by that time everyone already
231 knew (Oreskes and Le Grand, 2001, p. 406).

232 Given that earth scientists are nearly unanimous that the
233 formulation of plate tectonics was one of the great advances
234 of twentieth century earth science, it seems clear that sci-
235 ence does not require proof—neither in the sense of a direct
236 detection or measurement, nor in the sense of certainty or
237 unanimity—to advance. Science can and does proceed on
238 the basis of indirect evidence and abductive inference, so
239 long as the evidence and the inferences are acceptable to rel-
240 evant scientific experts. In the earth and environmental sci-
241 ences, in which controlled experiments are rarely possible,
242 this is generally how it does proceed. In the case of plate
243 tectonics, by the time direct measurements were obtained,
244 they were superfluous; the community had already achieved
245 consensus. The satellite data were interesting and satisfying,
246 but from the perspective of the advance of the science, they
247 were not especially important.

248 Now imagine that continental drift had been relevant to
249 a question of public policy. We can immediately see that
250 defenders of the status quo could have insisted that the data
251 were indirect and the theory was not proven. Moreover, they
252 could have found prominent scientists to support this view.
253 Even in the 1970s and 1980s, there were a few well-known
254 outliers, such as the distinguished geophysicists Sir Harold
255 Jeffreys and Gordon J.F. MacDonald, who rejected laterally
256 mobile continents outright, and the Tasmanian geologist S.
257 Warren Carey, who in the 1950s had organized research on
258 mobile continents based on the alternative framework of an
259 expanding Earth and continued to advocate that view until
260 his death (Oreskes and Le Grand, 2001; 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).

261 If one had looked hard enough, one might even have found
 262 someone who was specifically waiting for direct quantitative
 263 measures of plate motions; such skeptics could have been
 264 trotted out to demonstrate that the theory was uncertain. If
 265 money or celebrity had been at stake, it's likely that more
 266 skeptics would have been generated. After all, as Thomas
 267 Hobbes noted centuries ago, men will argue about the rules
 268 of geometry if they find it in their interest to do so (Hobbes,
 269 1969; see also Shapin, 1994, p. 224).

270 Should earth scientists have waited for these recalcitrant
 271 individuals to be convinced? Should implementation of our
 272 hypothetical policy been deferred? Of course not: scientific
 273 knowledge would not develop if such severe standards
 274 were enforced. Indeed, it was precisely this feature to which
 275 Thomas Kuhn credited the progressive nature of scientific
 276 inquiry: that scientists, unlike artists or humanists, forge stable
 277 consensus by ignoring outliers and moving on (Kuhn,
 278 1962; see also Latour, 1987).

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

295 Another point should be evident by now: There is no objective,
 296 irrefutable definition of what constitutes scientific proof,
 297 and no atemporal criteria upon which scientists have forged
 298 consensus. At different times, in different places, and among
 299 different communities of practitioners, scientists have
 300 adhered to differing standards of demonstration and
 301 argumentation and forged consensus by various means. What
 302 some earth scientists demanded in the 1920s, others were
 303 content to live without in the 1960s. Similar stories can
 304 be found throughout the history of science (Galison, 1997;
 305 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*. 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*, while 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

359 considered a military technology the relevant studies were
 360 mostly classified, and few in the public knew of their results.
 361 After the war, safety considerations were largely brushed
 362 aside as DDT was hailed as a miracle chemical, and its de-
 363 veloper, Paul Muller, awarded the Nobel Prize in medicine
 364 or physiology for its use in disease control (Russell, 1999).
 365 In any case, existing pesticide regulation was based on as-
 366 suring efficacy and controlling residues on food, not on envi-
 367 ronmental impact, and military studies of DDT did not deal
 368 with hazards to wildlife. Moreover, in any situation where a
 369 problem has not been widely recognized, the initial recog-
 370 nition will inevitably involve anecdotes, case reports, and
 371 circumstantial evidence.

372 But anecdotes are not necessarily false, and Carson's work
 373 was also based on her reading of a growing scientific lit-
 374 erature. These studies documented accumulating evidence
 375 of harmful effects. Carson was reporting to the public what
 376 many scientists were seeing in their day-to-day work and re-
 377 porting in specialist journals. Much of her discussion drew
 378 on articles published by wildlife biologists who had wit-
 379 nessed the effects she now summarized. Perhaps for this
 380 reason, Carson was firmly supported by many in the sci-
 381 entific community, particularly biologists. Oceanographers
 382 who had come to know her through her earlier book, *The*
 383 *Sea Around Us*, were also generally supportive.

384 In 1962 President John F. Kennedy decided that the cir-
 385 cumstances warranted a review of government environmen-
 386 tal policy. The President's Science Advisory Committee
 387 (PSAC) reviewed the topic. According to historian Zuoyue
 388 Wang, the scientists on the committee undertook to examine
 389 the problem in a considered manner, and to "take real ac-
 390 tions to understand and control the effects of pesticide use"
 391 (Wang, 1997, p. 145).

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

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

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

423 The panelists noted that their charge was difficult: to con-
 424 trast obvious, rapid benefits with subtle, long-term risks.
 425 Moreover, their task was confounded by a multitude of un-
 426 certainties. These included the gap between data on acute ex-
 427 posure (whose risks were not disputed) and chronic effects;
 428 the lack of information on synergistic effects; and the fact
 429 that existing data probably under-reported adverse effects,
 430 as most doctors were ill-equipped to recognize sub-acute
 431 pesticide poisoning. Moreover, most of the available data
 432 involved animals, rather than humans. Experiments on lab-
 433 oratory animals showed that small doses could cause liver
 434 damage, but "the mechanisms leading to these effects are
 435 unknown" (1963, p. 12).

436 Despite these uncertainties, the panel broadly endorsed
 437 Carson's concerns, and called for greater control of pesticide
 438 use. The evidence of damage to wildlife was clear and com-
 439 pelling, they concluded, even in cases of "programs carried
 440 out exactly as planned" (1963, p. 10). Pest control programs
 441 had produced significant collateral damage to birds and fish,
 442 such as the loss of "[a]n entire year's production of young
 443 salmon . . . in the Miramichi River in New Brunswick in
 444 1954, and again in 1956," and the loss of robins "after Dutch
 445 elm disease spraying in certain communities in Wisconsin
 446 and Michigan" (1963, p. 11). Like Carson, the PSAC com-
 447 mittee accepted these case reports as legitimate evidence of
 448 harm, which might in time spread to human populations. In-
 449 deed, they noted that wild animals were likely to reveal the
 450 effects of bioaccumulation before humans did, because the
 451 human food supply was regulated.

452 While not dismissing the prospect for future scientific
 453 technological improvements, such as increased use of bio-
 454 logical pest control and improved breeds of resistant crops,
 455 the panel concluded in favor of immediate action to restrain
 456 pesticide use:

457 Precisely because pesticide chemicals are designed to kill
 458 or metabolically upset some living target organism, they
 459 are potentially dangerous to other living organisms . . .
 460 The Panel is convinced that we must understand more
 461 completely the properties of these chemicals and deter-
 462 mine their long-term impact on biological systems, in-
 463 cluding man. The Panel's recommendations are directed
 464 toward these needs, and toward more judicious use of
 465 pesticides or alternate methods of pest control, in an ef-
 466 fort to minimize risks and maximize gains. They are of-
 467 fered with the full recognition that pesticides constitute
 468 only one facet of the general problem of environmental
 469 pollution, but with the conviction that the hazards result-

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

473
474 The panel made numerous specific recommendations: the
475 re-evaluation of some pesticides already on the market and
476 increased stringency in the approval of new ones; increased
477 enforcement powers for the FDA; transfer of authority for
478 non-food pesticides out of the Department of Agriculture
479 and into the Department of Health, Education, and Welfare
480 (HEW); and the involvement of the Secretary of the Inter-
481 ior . . . in review of all registrations that may affect fish
482 and wildlife" (1963, p. 18). Once authority was transferred,
483 HEW should undertake comprehensive studies of occupa-
484 tional and environmental exposures, and implement moni-
485 toring programs for air, water, and soil. Finally, the entire
486 federal pesticide program should be reviewed with the view
487 that some federal pest control programs "should be modi-
488 fied or terminated." The "present mechanisms" for evaluat-
489 ing pest control programs, they concluded, "are inadequate"
490 (1963, p. 20).

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

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

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

535 PSAC never claimed that the hazards of persistent pes-
536 ticides were "proven," "demonstrated," "certain," or even
537 well understood; they simply concluded that the available
538 data were adequate to show that harms were occurring,
539 warranting changes in the pattern of pesticide use. They
540 also noted that environmental concerns other than pesti-
541 cides might actually be more serious, but they did not use
542 this to deflect attention from the issue with which they were
543 charged. They took seriously the idea that alternatives to
544 pesticides, such as biological pest control, might be effica-
545 cious. They were not dismissive of such alternatives, and
546 they did not accuse Carson and her supporters of harboring
547 hidden agenda. Finally, they did not let a lack of scienti-
548 fic understanding of the *mechanisms* of pesticide damage
549 stop them from accepting the *empirical* evidence of these
550 effects. They called for more study, but they did not use
551 uncertainty as justification for inaction. Policy was made;
552 action was taken. Whether the panel was "right" in all their
553 conclusions and whether the policy adopted was "correct"
554 is a matter for evaluative hindsight—and one's judgment
555 will depend in part on one's own value commitments—but
556 right or wrong, these actions stand in contrast to current
557 strategies of delay on grounds of scientific uncertainty.

558 An interesting question to consider is where the PSAC
559 members placed the burden of proof—and why. The com-
560 mittee report explicitly invoked the rhetorical standard of
561 reasonable doubt (their words), and placed the burden of
562 proof on those who argued that persistent pesticides were
563 safe. In their conclusion, a pesticide should be restricted or
564 disapproved for use if there was "reasonable doubt" of its
565 safety (1963, p. 20). In making this choice, they implicitly
566 invoked a normative standard: denying privilege to the sta-
567 tus quo, and placing responsibility on pesticides manufact-
568 urers. Without more detailed research it is not possible to
569 say what arguments guided their thinking, but the use of the
570 legal phrase "reasonable doubt" suggests that they may have
571 been guided by existing legal frameworks, such the land-
572 mark federal Food, Drug and Cosmetic Act (1938), which
573 placed the burden of proof on manufacturers to demonstrate
574 the safety of their products, and the Miller Amendment to
575 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.

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

587 Whether this was the “right” or “wrong” decision, it is
588 a clear example of public policy implemented on the basis
589 of scientific knowledge that was neither proven nor certain,
590 but that reflected a consensus of expert scientific opinion.
591 PSAC was composed of nine prominent scientists in the
592 United States, who listened to testimony from leading ex-
593 perts. While these experts were not unanimous in their views,
594 the PSAC report reflected the committee’s assessment of the
595 weight of relevant scientific opinion. That weight supported
596 the banning of DDT. This is not to say that no one opposed
597 the ban—quite the contrary—or that there aren’t some indi-
598 viduals today who continue to question its wisdom—there
599 are. It simply shows that informed public policy was imple-
600 mented based on a consensus of relevant scientific experts, a
601 consensus that was accepted by politicians with the author-
602 ity to act upon it, and with which the public by and large
603 appears to have been content.

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

618 Even without such a deeper historical analysis, we can
619 make the point that PSAC made a recommendation, based
620 on what was both accepted then and still appears in hind-
621 sight as the consensus of expert scientific opinion, despite
622 some open dissent and acknowledged uncertainty. The pol-
623 icy that resulted was successful in addressing public con-
624 cerns, and it was based neither on an abstract notion of proof,
625 nor on a demand for certainty, but on the weight of con-
626 sidered scientific opinion and public concern. This may not
627 be the hard and fast principle that some people want, but it
628 is the reality of how things can work in practice—at least
629 under the right conditions. How to create and sustain such
630 conditions is a topic for another paper (and perhaps another
631 scholar).

5. Different experts weigh evidence differently

632

633 The scientific community was divided about Carson’s
634 claims, just as earlier earth scientists were divided over
635 Wegener’s. In the case of continental drift, the divide was
636 partly geographic: scientists who lived, worked, or traveled
637 in the southern hemisphere, where the empirical evidence
638 was strongest, were more likely to accept it than those who
639 had not. In the case of DDT, the divide was weighted along
640 disciplinary and institutional divides: biologists, oceanogra-
641 phers, the Department of Interior and PSAC generally af-
642 firmed Carson’s concerns, chemists, food scientists, and US
643 Department of Agriculture scientists generally did not.

644 This is a common pattern in scientific debates: special-
645 ists from different locales and in different fields weigh evi-
646 dence differently (see also Sarewitz, 2004). Elsewhere I
647 have argued that scientists have epistemological affinities
648 and chauvinisms, based on education and training, personal
649 affiliations and loyalties, and their philosophies of science
650 (Oreskes, 1999, pp. 51–53). These preferences and prej-
651 udices affect how scientists weigh evidence, with a ten-
652 dency to give greater weight to evidence that is near to
653 hand, with ‘nearness’ being experienced physically, socially,
654 and epistemologically.⁷ As the seismologist Charles Richter
655 once put it, “We are all best impressed by evidence of the
656 type with which we are most familiar” (Richter, 1958; see
657 also Oreskes, 1999). Epistemological affinities can be found
658 in any debate—not merely politically charged ones—but
659 they take on added fervor when scientific debate spills over

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 life 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. And 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.

660 into the public arena, and an added dimension when finan-
661 cial or political interests are at stake.

662 In hindsight, the extra-epistemic interests of Carson's crit-
663 ics are obvious—many had ties to the pesticide industry—so
664 we might dismiss them as handmaidens of that industry.
665 That would be a mistake, for it would obscure the fact that
666 *all* debates involve underlying commitments, and clarity re-
667 quires addressing those commitments. Like Mrak, many of
668 Carson's critics were food scientists dedicated to a large, in-
669 expensive food supply. Like Bjørn Lomborg they were not
670 ashamed to value immediate human needs over long-term
671 ecological concerns. Carson's historical critics thus lie in
672 conceptual alignment with Lomborg today, who is explicit
673 that his focus is on "[c]ounting [human] lives lost from dif-
674 ferent problems (Lomborg, 2001, p. 11).

675 Indeed, Carson's critics accused her of indifference to
676 human fate, writing to the tune of the folk song, "Reuben,
677 Reuben,"

678 Hunger, hunger, are you listening,

679 To the words from Rachel's pen?

680 Words which taken at face value,

681 place lives of birds 'bove those of men" (Lear, 1992).

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

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

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

710 Indeed, the very word service reveals a kind of con-
711 sumerist bias—as if life were a matter purchasing services
712 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 environmental-
ists 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 nor-
mal part of geological history (Houghton et al., 1995, 2001).

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

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

785 On the other hand, the problem of how to measure the
786 average temperature must be addressed in the hydrosphere
787 equally as in the atmosphere; one can no more stick a ther-
788 mometer into the ocean to get a global average than into the
789 air. Here the oceans present a second advantage: the speed
790 of sound in water is directly dependent on the water tem-
791 perature. A long-range transmission, say from La Jolla to
792 Honolulu, provides an integrated assessment of the thermal
793 conditions of the water between those two points. In this
794 way, acoustics can provide information on the large-scale
795 thermal structure of the oceans, without being overly af-
796 fected by temporally or geographically local fluctuations.
797 (In particular, the integrating effect of tomography damp-
798 ens the 10–100 km scale of ocean “weather,” that dominates
799 the temperature variability spectrum.) Take measurements
800 at strategic locations across the world's oceans, and you
801 come close to measuring the whole world ocean tempera-
802 ture. Do this repeatedly over the course of several decades,
803 and you may have an answer to the question of whether
804 Earth's oceans—and therefore Earth—is warming up, inde-
805 pendent of noisy and perhaps unreliable instrumental tem-
806 perature records and unverifiable climate models.

807 The scientists involved originally dubbed this the “ocean
808 acoustic thermometer” (Spiesberger et al., 1983); in time
809 it became known as Acoustic Thermometry of Ocean Cli-
810 mate (ATOC). Like the sea-floor magnetic stripes that re-
811 vealed plate motions, the acoustic thermometer was admit-
812 tedly indirect—measure sound velocity and from that cal-
813 culate water temperature—so one's conclusions could be
814 only as good as the science of underwater acoustics. But
815 that science was very, very good. Besides nuclear physics,
816 few subjects in 20th century physical science had been stud-
817 ied in as great detail. Since World War II, and throughout
818 the Cold War, the US (and other countries) had put enor-
819 mous resources into the understanding of underwater sound
820 transmission for its use in pro- and anti-submarine war-
821 fare. During World War II, the study of underwater sound

822 transmission had been a major initiative of the National
823 Defense Research Committee (National Defense Research
824 Committee, 1944; Ewing and Worzel, 1945; Eckart, 1968;
825 Research Analysis Group, 1969) for its use in submarine
826 hiding and tracking. With the Cold War development of SO-
827 SUS (the SOund SURveillance System)—the secret US un-
828 derwater ocean acoustic system that tracked the activities of
829 Soviet submarines—and submarine launched ballistic mis-
830 siles, these research programs continued to flourish through-
831 out the 1950s, 1960s and 1970s (Frosch, 1964; Urick, 1979;
832 Spiess, 1997). Over the course of nearly half a century, phys-
833 ical oceanographers had become intimately familiar with the
834 physics of underwater sound. While salinity and currents
835 also affect ocean temperature, it was well established that
836 these effects were secondary (Munk and Wunsch, 1979).
837 Because of the high heat capacity of water, the ocean is a
838 significantly larger reservoir of global heat storage than the
839 atmosphere, and so one might reasonably say that ΔT_{Ocean}
840 $= \Delta T_{\text{Earth}}$.

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

855 From the start, the scientists involved recognized the rel-
856 evance of their proposal to the “big question” of global
857 warming. As Woods Hole oceanographer John Spiesberger
858 wrote to oceanographer Henry Stommel in 1989, “our in-
859 tention [was] to set up acoustic observations to detect hypo-
860 thetical greenhouse effects on climate change” (Woods Hole
861 Oceanographic Institution MC6, 1989). Answers would not
862 be obtained quickly, but would require persistent measure-
863 ments over decades. “One can imagine measurements ex-
864 tending for 100 years or more where perhaps the gradual
865 heating of the oceans due to the increase of CO₂ could be
866 detected. Just as astronomers have established observatories
867 where measurements have been taken for hundreds of years,
868 the oceanographers might establish an acoustic observatory
869 of the type described herein” (Spiesberger et al., 1983). Like
870 the proponents of the Worldwide Longitude Operation, these
871 scientists took the long view, envisaging a research program
872 in which oceanographers would answer fundamental ques-
873 tions about the Earth, just as astronomers over the centuries
874 had answered fundamental questions about the heavens. For
875 the less patient, Walter Munk pointed out that it was not
876 necessary to wait for centuries; a decade of measurements
877 would be sufficient to detect the predicted warming effect

878 (Munk and Forbes, 1989). In 1991, the Heard Island Fea-
879 sibility Test demonstrated that the transmissions could in-
880 deed be detected at global ranges and a meaningful signal
881 obtained (Munk et al., 1994, 1995).

882 ATOC was a clever, creative, and insightful proposal to
883 apply basic scientific knowledge to answer a significant en-
884 vironmental question, but this promising avenue of inquiry
885 hit a wall of controversy when biologists suggested that the
886 sound signals might injure marine mammals. The ATOC
887 permit requested permission for a “take”—defined as any
888 injury or harm—of a variety of marine mammals, including
889 whales, dolphins, seals and marine turtles, and encompass-
890 ing several threatened or endangered species. Potentially,
891 several hundred thousand marine mammals could have been
892 affected (Potter, 1994). While the word “take” in this context
893 meant any effect, no matter how small, and the application
894 insisted that any effects would be transitory and minor, some
895 biologists disputed the grounds for this optimistic assess-
896 ment. Louis Herman, Director of the Kewalo Basin Marine
897 Mammal Laboratory, Honolulu, noted that the ATOC signal
898 fell within the frequency band of the humpback whale song,
899 which might render the songs less detectable or even un-
900 recognizable (Herman, 1994). The ATOC permit included a
901 plan to monitor possible effects on humpback whales, but
902 acknowledged that long-term effects would be difficult to
903 detect. Yet it was precisely such long-term effects, Herman
904 noted, that “are of the greatest concern” (1994, p. 65).

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

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

miss 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 judg-
ment. 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, establish-
ing 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 En-
vironmental Impact Statement (DEIS) was released for pub-
lic 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 contin-
ued to oppose the project. In September 2000, after expira-
tion of the DEIS public comment period, Canadian biolo-
gist 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 pre-
tend 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 demon-
strate long-term effects, but . . . it failed to adequately in-
vestigate short-term responses. The proposal for contin-
uation of ATOC is based on false [premises] (Anderson,
2000).

While scientists continued to try to address the environ-
mental issues throughout the late 1990s, by the end of the
decade the project was grinding to a halt. In 1999, the ini-
tial permits were not renewed, and the scientists were re-
quired 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 died at its end: 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 (Worchester, 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

987 exaggerated, and that the news media had misrepresented
988 the permit language of “taking” to mean “killing” (Potter,
989 1994; Munk, 2003). Most felt that environmentalists should
990 welcome the project, because it was motivated by an envi-
991 ronmental concern. Why didn’t environmentalists see it that
992 way?

993 One reason is clear: most environmentalists already ac-
994 cepted that global warming was real. They did not need
995 more information to be convinced, and therefore were not
996 interested in accepting risks to get that information (Potter,
997 1994). Moreover, while scientists were proud of the “swords
998 to plowshares” aspect of the projects, for many environmen-
999 talists the military association was grounds for suspicion. In
1000 the words of oceanographer Stanley Flatte, “folks thought it
1001 was some kind of secret Navy project.” (Flatte, 2000). Even
1002 if the project were what it claimed to be, the US military
1003 has not been not known for its history of environmental sen-
1004 sitivity, and in the past has been exempted from much en-
1005 vironmental regulation and indemnified from litigation. To
1006 environmental activists, the US Navy as steward of the en-
1007 vironment was simply not plausible.

1008 One argument in defense of the project was that the US
1009 Navy had been using this sort of acoustic transmissions for
1010 decades, but this did little to satisfy environmentalists for
1011 whom such an argument merely proved the point: that the
1012 Navy was used to operating without environmental over-
1013 sight. Naomi Rose, a biologist with the Humane Society, put
1014 it this way. “The oceanographers asked: ‘Why would you
1015 even think we would hurt the environment?’ and environ-
1016 mentalists responded, ‘Why would we think you wouldn’t?’”
1017 (Rose, 2001). From the perspective of environmentalists,
1018 the scientists were aligned with a Goliath who had run
1019 rough-shod over the environment in the past, and would
1020 likely do so again in the future.

1021 And what if ATOC “proved” that there was no climate
1022 signal? Then what? Put another way, why should anyone
1023 accept any particular line of evidence as a scientific trump
1024 card? After all, is there really such a thing as direct scientific
1025 evidence, or is it simply that the ambiguities inherent in
1026 some forms of evidence are more evident than in others?
1027 That some forms of data production are more transparent
1028 than others?

1029 Consider again the Worldwide Longitude Operation. The
1030 latter was promoted as the direct measurement of continental
1031 motion, and therefore less ambiguous than the various indi-
1032 rect, largely historical, arguments that supporters of drift had
1033 used. But if the project had continued uninterrupted by world
1034 war, yet failed to detect the continental motion, would sci-
1035 entists have concluded? That drift had not occurred? Would
1036 they then have dismissed the other evidence of drift as dis-
1037 proved. Or might they have questioned the experiment, won-
1038 dering if it there were a mistake in it somewhere? Both
1039 options would have been possible, because the Longitude
1040 Operation, like all scientific experiments, was based on cer-
1041 tain premises, certain background assumptions. In this case,
1042 those assumptions included, among other matters, premises

1043 about how radio waves travel through the atmosphere: for
1044 the experiment to have worked, those travel paths would
1045 have had to have been unaffected by ionospheric fluctuation.
1046 Today we hold that radio waves travel paths are affected by
1047 ionospheric fluctuations. Moreover, the Operation was based
1048 on assumptions about the rate of drift—tens of meters per
1049 year—that turned out to be much too high. In hindsight, the
1050 Longitude Operation was doomed to failure—World War II
1051 or not.

1052 ATOC was similarly touted as a direct measurement of
1053 changing ocean temperature, more reliable than historical
1054 climate records. But, as in the Worldwide Longitude Opera-
1055 tion, the proposal involved various assumptions: about sound
1056 travel paths, about the accuracy of signal processing, and
1057 about the reliability and consistency of instrumentation. No
1058 matter how good the science, there are always uncertainties.

1059 Experimental premises may be faulty, limited, or incom-
1060 plete. Instruments may not be sensitive enough to detect faint
1061 signals. Theoretical understandings may turn out to be erro-
1062 neous. One independent paper evaluating the feasibility of
1063 the ATOC approach concluded that, when all the uncertain-
1064 ties were considered, there was a “realistic chance of detect-
1065 ing the expected greenhouse-induced warming in the World
1066 Ocean” (Mikolajewicz et al., 1993). One could equally con-
1067 clude from such language that the prospect of not detecting
1068 the expected signal was also ‘realistic.’ The scientists in-
1069 volved in ATOC emphasized how well understood the basic
1070 physics were, but a project like this is never simply a matter
1071 of basic physics. Environmentalists never said so explicitly,
1072 but they might reasonably have viewed ATOC as a Trojan
1073 horse—trouble masquerading as a gift.

1074 Finally ATOC ran aground simply because people will
1075 go to great lengths to protect the things they love. As Paul
1076 Anderson put it, “It is the misfortune of physical oceanog-
1077 raphers that the sea contains organisms that are culturally
1078 valued, and ecosystems and populations of ecological and
1079 economic importance” (Anderson, 2000). From the perspec-
1080 tive of oceanographers, the objections to ATOC may have
1081 seemed irrational, but consider a mother bear who charges a
1082 solitary hiker. The hiker has no gun and no intention of hurt-
1083 ing her cubs, but she does not know that. From her perspec-
1084 tive, she’s not taking any chances. What may be presented
1085 as a scientific problem—a matter of technical facts—reveals
1086 itself to be a question about which particular chances we are
1087 prepared to take.⁸

7. What happens when scientists don’t agree? 1088

1089 Most of us realize that proof—at least in an abso-
1090 lute sense—is a theoretical ideal, available in geometry 1090

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).

1091 class but not in real life. Nevertheless, many of us still
1092 cling to the idea that some set of facts—some body of
1093 knowledge—will resolve our problems and make clear
1094 how we should proceed. History suggests otherwise: ear-
1095 lier scientific wisdom has been overturned, earlier gen-
1096 erations of experts have made mistakes. This is as true
1097 in physics and chemistry as in biology and geology. The
1098 criteria that are typically invoked in defense of the reli-
1099 ability of scientific knowledge—quantification, replicability,
1100 falsifiability—have proved no guarantee.

1101 Moreover, experts do not always agree. Even when there
1102 is no transparent political, social, or religious dimension to
1103 a debate, experts may weigh evidence differently based on
1104 intellectual, social, and personal affinities. Honest and intel-
1105 ligent people may come to different conclusions in the face
1106 of the “same” evidence, because they have focused their
1107 sights of different dimensions of that evidence, emphasizing
1108 different elements of the evidentiary landscape. Even
1109 when a scientific community reaches consensus on a previ-
1110 ously contested issue—as earth scientists did in the 1960s
1111 over moving continents—there are always dimensions that
1112 remain unexplained. In the future, plate tectonics no doubt
1113 will be modified, perhaps overturned entirely. Indeed, there
1114 are a handful of scientists today who advocate Earth expan-
1115 sion to explain continental separation, and they are of course
1116 eager to detail the limitations of plate tectonics theory (e.g.
1117 Shieds, 2003). Nevertheless, for now plate tectonics remains
1118 the consensus of most Earth scientists: our best basis for
1119 understanding the Earth.

1120 Contrary to the Thomas Kuhn’s widely accepted theo-
1121 ry, anomalies are always hovering about, even in ‘nor-
1122 mal science.’ Scientific consensus is a complex process—
1123 involving a matrix of social, political, economic, histori-
1124 cal considerations along with the epistemic—and history
1125 shows that its achievement typically requires a long time:
1126 years, decades, even centuries. But even when a stable con-
1127 sensus is achieved, scientific uncertainty is not eliminated.
1128 Rather, once we have deemed the remaining problems as
1129 “minor”—which is to say, insufficiently great as to war-
1130 rant further concern—we simply live with them (Engelhardt
1131 and Caplan, 1987). Moreover, the grounds on which sci-
1132 entific communities have concluded that evidence is “good
1133 enough” to warrant living with the uncertainties have var-
1134 ied enormously throughout the course of history. A deter-
1135 mined individual may choose to pursue these uncertainties,
1136 and that determination may successfully destabilize the prior
1137 consensus. In a “purely” scientific debate, that determina-
1138 tion would, ideally, arise solely from the demands of em-
1139 pirical evidence, but no debate is ever “purely” scientific,
1140 given that, at minimum, credibility, reputation, and, perhaps
1141 future funding are at stake.

1142 When there is a policy dimension to a scientific debate,
1143 we can expect such determination to be common, as scien-
1144 tists pursue issues whose importance is measured against a
1145 backdrop of political significance, as the media focus atten-
1146 tion on ‘mavericks,’ and as money flows into scientific re-

1147 search from parties with stakes in the outcomes. Louis Pas-
1148 teur noted this phenomenon long ago, writing in the 19th
1149 century about impassioned debates in the 18th over the re-
1150 ality of spontaneous generation: “Very animated controver-
1151 sies arose between scientists then [in the 18th century] as
1152 now [in the 19th]—controversies the more lively and pas-
1153 sionate because they have their counterpart in public opin-
1154 ion, divided always, as you know, between . . . great intel-
1155 lectual currents.” (Geison, 1995). Conversely, when there
1156 is a scientific dimension to a policy debate, we can expect
1157 that science may be used as a basis for competing political
1158 or moral claims (Nelkin, 1995; Herrick and Sarewitz, 2000;
1159 Jamieson, 1996; Sarewitz, 2004).

1160 How can we evaluate scientific claims when scientists
1161 themselves don’t agree? There is no good answer to this
1162 question, but certain perspectives may help us to judge the
1163 information we are receiving. First we may ask: Who are
1164 the relevant experts? Or better, what is their expertise rele-
1165 vant *to*? In the case of DDT, food scientists were qualified
1166 to speak to the agricultural benefits of DDT, and wildlife bi-
1167 ologists were better placed to speak to the ecological harms.
1168 In this sense both sides were right in what they affirmed but
1169 wrong in what they denied, and ultimately the question was
1170 not so much who was ‘right,’ but which set of concerns—an
1171 enhanced food supply for humans or greater protection of
1172 wildlife—would be viewed as more pressing.

1173 A similar point can be made about the ATOC case. Phys-
1174 ical oceanographers are experts in the make-up, behavior,
1175 and properties of the ocean as a body of water, but not as
1176 an abode for life. They could discuss the potential results of
1177 the ATOC experiment, but cetacean biologists were better
1178 qualified to consider its potential effects on whales. If the
1179 question at stake was—will the ATOC experiment provide
1180 useful information about global climate?—oceanographers
1181 were the relevant experts to answer that question. If the ques-
1182 tion was—what will the effect of the ATOC experiment be
1183 on whales?—then biologists were the relevant experts.

1184 Biologists do not map the distribution of earthquake
1185 hazard, endocrinologists do not forecast the weather,
1186 and chemists are not permitted to perform heart surgery.
1187 There are good reasons why. Traditional markers of
1188 expertise—training, experience, academic appointments and
1189 honors—are no guarantee of an expert’s honesty, integrity,
1190 or wisdom, but, *ceteris paribus*, it makes sense to trust
1191 those persons whose competence is closest to the question
1192 at hand. Why else do we support colleges, universities, and
1193 research institutes if not to develop and sustain expertise
1194 that we believe to be of value?

1195 Expertise can of course be compromised and even bought
1196 outright, so we also need to ask: what are the non-epistemic
1197 interests of the experts? How might they be affecting the
1198 scientific results? All parties to debates have interests that
1199 condition their responses to evidence and arguments, so it is
1200 legitimate to inquire into those interests (Etzkowitz, 1996).
1201 The recent growth of corporate sponsorship of scientific re-
1202 search on university campuses has raised the question of

1203 how financial interests are currently shaping not merely the
1204 subject of scientific research, but also the outcomes.

1205 An obvious and well-studied area is tobacco research,
1206 which, through its obviousness, drives home a point that
1207 may be less obvious yet still relevant elsewhere. Research
1208 by scientists at the University of California, San Francisco,
1209 has shown the ways in which the tobacco industry has tried
1210 to generate uncertainty over the issue of second-hand smoke
1211 by directly sponsoring scientific studies whose purpose is
1212 to destabilize the existing consensus. These studies are far
1213 more likely to find no evidence of ill effect than studies
1214 not funded by the tobacco industry (Hong and Bero, 2002;
1215 Shamasunder and Bero, 2002; Montini et al., 2002; Bero,
1216 2003; Bryan-Jones and Bero, 2003). In an overall review of
1217 the effects of industry-sponsored research, Boyd and Bero
1218 (2000) conclude that research clearly documents “an associ-
1219 ation between single-source sponsorship of clinical research
1220 and publication of results favoring the sponsor’s product”
1221 (see also Stelfox et al., 1998; Angell, 2000). The critical
1222 point here is not that the fact the research was funded by
1223 industry, because all science is funded by some institution,
1224 group or individual, and it’s not clear that industrial pa-
1225 tronage is intrinsically more problematic than support from
1226 a prince, a foundation, an armed service, or a government
1227 agency. Rather, the issue is that the research is supported by
1228 a sponsor who wants a *particular* result—a particular *epis-*
1229 *temic* outcome—and the researchers know what that out-
1230 come is, producing an explicit conflict of interest that un-
1231 dermines the integrity of the research performed.

1232 This point brings us to what may be the most impor-
1233 tant point of this paper: scientific proof is rarely what is at
1234 stake in a contested environmental or health issue. Bjørn
1235 Lomborg’s focus is on humans—on quantitative measures
1236 of the conditions of life for the majority of persons on the
1237 planet—and given this perspective, many of his claims are
1238 surely right. More people do live longer, eat more calories,
1239 and have thicker roofs over their heads than was generally
1240 the case in the past. But many environmental claims are
1241 not so much about life’s quantities as its qualities. They are
1242 about aesthetic and moral choices. They are about equity and
1243 ethics. To be sure, we humans have enhanced our lives by
1244 controlling, diminishing, and even eradicating certain forms
1245 of non-human life, and few people would defend viral or
1246 bacterial rights. But increasingly our actions are impacting
1247 the Earth in ways that will affect future generations, who
1248 will have had no say in those choices and may be unable
1249 to undo them. In the past, human actions tended to be lo-
1250 cal and reversible, but increasingly our actions appear to be
1251 global and irreversible. As Roger Revelle astutely pointed
1252 out nearly 50 years ago, speaking of the human contribu-
1253 tion of CO₂ to the atmosphere, we are performing a “a great
1254 geophysical experiment” on our planet without the consent
1255 or the knowledge of future generations, and which cannot
1256 be undone (Revelle and Suess, 1957).

1257 Scientists debate unresolved epistemic and methodolog-
1258 ical issues in their own specialties all the time, but these

1259 rarely receive public scrutiny. Lack of consensus becomes
1260 a public issue when there is a public stake, which means
1261 a moral, political, or economic stake. In such cases, natu-
1262 ral science can play a role by providing informed opinions
1263 about the plausible consequences of our actions (or inac-
1264 tions), and by monitoring the effects of our choices (Herrick
1265 and Sarewitz, 2000). Social science can do the same. But
1266 there is no need to wait for proof, no need to demand it, and
1267 no basis to expect it.

1268 Uncited reference

1269 Carson (2002).

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