

# UC Santa Barbara

## UC Santa Barbara Previously Published Works

### Title

Scientific Expertise and Natural Resource Decisions: Social Science Participation on Interdisciplinary Scientific Committees.

### Permalink

<https://escholarship.org/uc/item/5g2694fd>

### Journal

Social Science Quarterly. 83 (#1, March): 119-36., 83(1)

### Authors

Freudenburg, Wm R  
Gramling, Robert

### Publication Date

2002-03-01

Peer reviewed

# Scientific Expertise and Natural Resource Decisions: Social Science Participation on Interdisciplinary Scientific Committees\*

William R. Freudenburg, *University of Wisconsin–Madison; University of California at Santa Barbara*

Robert Gramling, *University of Louisiana–Lafayette*

Social scientists should seek greater involvement in interdisciplinary scientific committees, which often play important roles in natural resource management. In addition to our acknowledged areas of expertise, we have the ability to educate other disciplines about social sciences and, importantly, also about the realities of *biophysical* science input into policy processes. Two examples are worth noting. First, the asymmetry of scientific challenge can mean that biophysical science views/interpretations with favorable implications for organized groups' interests may have been "accepted" with little scrutiny, relative to work having unfavorable implications. Second and paradoxically, the structure of decision making can mean that preferences for "scientific caution" will result in resource management decisions that are anything but cautious. These and other observations need to be tested through participant observation by a greater number of social scientists on scientific committees in the future.

Natural resource management issues provide important new opportunities and challenges for social science contributions to the policy process. In the past, the roles open to social scientists have tended to be those associated with traditional disciplinary expertise, as in economists' involvement in economic policies and political scientists' work in analyzing political strategies. In the case of natural resources, by contrast, social scientists are likely to deal with issues that have generally been seen in the past as "belonging" to biophysical scientists, often doing so by serving as members of interdisciplinary scientific committees. Under this emerging model, it is increasingly common to find such real-world examples as a cultural anthropologist providing guidance on fisheries management, a social psychologist providing input to a wildlife management agency, or a sociologist participating in the technical evaluation of nuclear waste storage decisions.

\*Direct all correspondence to William R. Freudenburg, Professor of Rural Sociology & Environmental Studies, 1450 Linden Drive, Room 336A, University of Wisconsin, Madison, WI 53706 <freudenb@ssc.wisc.edu>. After July 1, 2002, Dr. Freudenburg's address will be: Dehlsen Professor of Environmental Studies and Sociology, Environmental Studies Program, University of California, Santa Barbara, CA 93106-4160.

When viewed from the perspectives of the past, such forms of involvement could well be seen as more than a little terrifying. In the past, after all, even in cases where social scientists have offered advice on *social* policy, where the relevance of social science expertise might have been expected to be relatively unproblematic, subsequent evaluations have often been quite negative, as suggested by titles such as *Maximum Feasible Misunderstanding* (Moynihan, 1969) or *Why Sociology Does Not Apply* (Scott and Shore, 1979). In the case of *natural resource* issues, the most common pattern to date appears to be one in which one or perhaps two social scientists will be added to an interdisciplinary scientific advisory committee—often well after the committee comes into being—on which all of the other members will be, as they sometimes put it, “*real scientists*,” at least some of whom will pride themselves on the precision of their predictions and who will view social scientists with something akin to scorn.

Although social scientists might well conclude that service in such interdisciplinary contexts ought therefore to be dreaded or avoided, this article will make precisely the opposite argument. As we spell out in the pages that follow, the experiences of the social scientists who have actually served on such interdisciplinary advisory committees—including but clearly not limited to our own experiences, which have included more than a dozen such committees—suggest that social scientists have much to contribute. Although social scientists often appear to be token members of such committees, most frequently comprising one or at the most two members, we will argue in this article that social scientific input has particular value for what tend to be seen as biophysical science deliberations.

One of the reasons why we encourage our colleagues to seek and accept such forms of service to society, however, is that social scientists have at least three broad types of expertise to offer, not just one. The first form of expertise will generally provide little surprise, given that it will have to do with the overt reason for adding social scientists to the committee in the first place—often involving the ways in which a “*natural*” resource element (be it fish or forest, water or waste) can influence and/or be influenced by human beings and social systems. The second and third forms of expertise, however, require greater discussion, and thus they will be our focus in the following two sections of this article.

### **We Know More Than We Realize**

The second form of expertise is one that growing numbers of social scientists have found themselves providing for interdisciplinary scientific committees: basic education on the nature of social scientific research. Although the point often comes as a surprise to those who have *not* yet served in such contexts, the experience often leads to the realization that social sci-

entists have reason to be far less apologetic about the nature of social science expertise than is commonly assumed. Still, we do need to acknowledge that this point is often highly surprising to biophysical scientists, as well, partly for reasons having to do with what is sometimes called the “hard science” issue.

The relevant contrast, of course, is not between the hard and *soft* sciences, but between the hard and *easy* sciences. If a geologist studies a rock, for example, that rock can often be broken into tiny pieces, all of which can be expected to lie passively on a workbench or under a microscope, many of which can readily be analyzed in terms of crystalline structure or chemical composition by off-the-shelf technologies, and none of which have ever been known to hire legal representation. A social scientist, by contrast, often needs to deal with institutional review boards or year-long reviews by the federal Office of Management and Budget even before doing interviews with human beings, many of whom may well have an interest in shaping the social scientist’s findings, all of whom may well object if they feel they are being “placed under a microscope” even figuratively, and any of whom may well change their behaviors or even decide to sue if they dislike the social scientist’s findings or conclusions. In the case of natural resource issues, moreover, the social scientist is often brought in only quite late in the process and then expected to analyze some of the most central and most complex issues of all, doing so with levels of funding that would be little more than rounding error for biophysical science research budgets. Clearly, then, the conclusion needs to be that the *social* sciences are the “hard sciences,” while the biophysical scientists have the luxury of pursuing relatively “easy” research.

Like most such distinctions, of course, the hard science/easy science dichotomy is not one that should be overstated or reified, particularly in the case of biological science colleagues who need to deal with complex real-world ecosystems that are too large to be studied within test tubes. In the case of lake or ocean ecosystems, to turn to a set of examples that will be considered in greater detail later in this article, there are only a few ways in which biologists can *directly* observe even a small fraction of the processes actually taking place well below the surface of the water. To examine a given species, a biologist may even need to remove it from its environment entirely, bringing into play an extreme form of the Hawthorne effect by killing the fish. To be fair, accordingly, we need to acknowledge that challenges such as this can cause a biologist to deserve nearly as much acknowledgment as “hard” scientists as would be the case if sociologists, anthropologists, economists, or historians could obtain knowledge only by “studying” humans in the morgue. Much the same needs to be acknowledged for astronomers who must observe the heavens from earth, resorting to relatively crude concepts as “black holes” and “ice volcanoes,” or the physicists who must “observe” atomic structure second- or third-hand, “discovering” new subatomic particles once or twice a decade. Still, the social sciences would

appear to be the hardest of the sciences, given that no economist or anthropologist who contributed to the accumulation of knowledge at the glacial pace of discovering one tiny particle every few years would be likely to be given tenure, let alone a Nobel Prize.

Many biophysical scientists, however, will not be as familiar with this state of affairs as might be desired, meaning that it creates the need for social scientists on interdisciplinary scientific advisory committees to carry out the first of two additional forms of “science education” that may not have been a formal part of their invitation to join such a committee; the need, to put it simply, is for social scientists to *educate other scientists on the committee* about the “hard science” issue, as well as educating them about even some of the most basic principles and findings from social science research. Although we see no reason to view this task with dread, we do see a need to acknowledge that it often requires explicit attention and a certain degree of hard work. Indeed, as one reviewer of this article noted, it is a virtual certainty that social scientists on such a committee will need to learn enough of the biophysical research findings (and terminology) to be reasonably conversant with other disciplines, while a significant fraction of the biophysical scientists will see no need to reciprocate. Instead—although additional research is required—the preliminary findings, based on our discussions with colleagues as well as our own experiences on such committees, suggest that perhaps two to three members of a dozen-person scientific committee, on average, will have an acute need for such an education, with the need often being greatest among committee members who received their training many decades ago.

The education process is important, however, because this lack of knowledge can be directly relevant to the issues being discussed. In discussions involving nuclear technologies or other “risk” issues, for example, it is still quite common to hear expressions of frustration about purportedly “irrational” or “ignorant” members of the public who “have no right” to influence policy decisions—expressions offered with no intended irony and no evident awareness that “the consent of the governed” is one of the basic expectations for policy decisions in a democracy, or that the members of Congress are elected by, and generally expected to represent, just such “irrational” constituents. Many biophysical science colleagues, accordingly, need to be provided with quite basic levels of education on fundamental issues, such as the fact that, although scientific input is often accorded considerable deference on “strictly technical” or factual matters, virtually any resource management issue will also inherently involve values and blind spots, and that, “at least in a democracy, when it comes to matters of values—and to questions such as ‘how safe is safe enough’—another word for ‘scientist’ is ‘voter’” (Freudenburg, 1996:15).

Frequently, the education needs to include another important fact: Most of the people we call “natural resource managers” actually do very little managing of natural resources. Instead, what many of them actually try to

“manage,” most of the time, are the behaviors of the one species that few resource managers have ever studied, namely, *Homo sapiens*. The pattern is perhaps the most obvious in the case of so-called nonrenewable resources, such as oil or coal. Despite the common use of phrases such as “oil production,” there is in fact very little that can be done to “produce” such fossil-fuel reserves in a human span of years, and in the absence of any human “management” or “production” initiatives, the resources are most likely to lie patiently in place. Even in the case of “renewable” resources such as forests or fish, what is actually most likely to be “managed” in most cases is the nature or rate of human harvest. Many of the fish species of the planet have proved eminently capable of growing, maturing, and reproducing in the absence of human intervention, and even trees commonly grow to maturity in decades or centuries, depending on the species and their locations, largely doing so without human intervention or help. As noted by Ludwig, Hilborn, and Waters (1993:17), although we speak of humans managing resources, it may be “more appropriate to speak of resources managing humans,” in the sense of inspiring ever-intensifying efforts to extract resources and profits. Whether “fisheries management” considerations focus on limiting the numbers or sizes of the fish species that can be taken by humans, these measures are not limitations on the behavior of fish, but restrictions on the behaviors of the humans who do the fishing. The strategies will be successful, or not, depending largely on human and organizational variables, such as how reasonable and enforceable the restrictions prove to be—a subject that cries for greater social science input—and on whether the restrictions limit fishing activities enough to make a difference in the ability of the species to grow and reproduce. The “fisheries management” efforts, themselves, are invisible to the fish.

There is also often still a need to remind biophysical scientists of some of the best-established principles from the social sciences, such as the fact that even “mere perceptions” can be quite real in their consequences, and conversely, that “blind spots,” or the effective *absence* of perceptions, can have real consequences, as well. One of the questions that needs to be asked, in other words, involves the degree to which the limitations of *biophysical* as well as social science knowledge will be perceived or recognized by the relevant scientists—and at least in our own experience, the actual degree of recognition is significantly lower than might be expected.

A particularly clear illustration of this point emerged during a conference supported by a large resource management agency within the Department of Interior, focusing on an important biological question about offshore oil platforms. Both the experience of fishers and the findings of fisheries research have pointed to the conclusion that fish populations are denser in the vicinity of offshore oil platforms; the session focused on the research question of whether the platforms simply concentrated the existing fish, or whether the structures actually produced additional fish. As the social scientists in the audience listened with mounting incredulity, a fisheries biolo-

gist described a million-dollar research project that had been carried out in the name of investigating this research question. The project was centered on a trip to one of the rigs off southern California with a research vessel and a submersible video camera that had been designed with agency funds, with the intent of capturing images of fish around the platform. Without discussing how these images would address the question of whether the fish were concentrated or produced by the platforms, the researcher described—more with humor than with embarrassment—how the incompetence of the boat captain and the nastiness of that trip's weather kept his team from stabilizing the camera and how the rough weather stirred up the water and reduced visibility. As the story unfolded, the researcher showed a series of slides that pictured blurry images of vaguely fishlike objects suspended in a medium that resembled diluted milk. The end result was that “due to circumstances beyond his control,” the million-dollar investment had cast absolutely no light on the question the project was “designed” to address. Although social scientists placed in such a quandary might consider ritual suicide as one of the few avenues of escape, both the presenter and the audience seemed to consider these results to be entirely unproblematic.

To be sure, experiences such as this are at least somewhat rare, but they are not so rare as is commonly assumed, and at least in our experience, they are clearly not rare enough to support what “everybody knows” about hard versus easy sciences. Both in the social and the biophysical sciences, in other words, at least a significant fraction of all active scientists appear to be less aware than might be desirable about the nature and limits of knowledge across disciplines. One of the key differences, in fact, may simply be the fact that social scientists tend, on average, to have thought more carefully about the nature of any such limitations.

Another relevant illustration can be drawn from another scientific advisory board; in that case, scientists were considering whether to help increase the total fishing industry harvest of a given fish species by decreasing the minimum size of the fish that could be kept. The committee's most knowledgeable expert for that particular species argued against the policy change, given that not much was known about the species in question. In this case, in other words, he showed clear awareness of the limitations of scientific understanding. As he noted, it was not clear how fast these fish grew, how large they normally got, what their reproductive potential might be, or even what the details were of when and how they reproduced. Swayed by the power of this expert's argument, the committee agreed to recommend that the minimum size not be changed and that an increased harvest of the species not be allowed. Later, during a break, this same committee member wondered aloud to one of the authors of this article why a social scientist should be on a fishery management committee, particularly in view of what he regarded as the “fact” that the social sciences knew nothing about fish and very little about humans and their behavior. He was initially taken aback and more than a little angry when he was told that, unlike the case in

his own discipline, social scientists actually know a good deal about the species we study; at a minimum, after all, we know how fast humans grow, where they live, and how large they commonly get, as well as knowing in exquisite detail how they reproduce.

### Decisions Will Be Made

In addition to whatever expertise we may possess that is directly “about” the interactions between humans and the other species of the ecosystem, and in addition to a broader awareness about the nature of social science findings and principles, social scientists will often need to provide a third form of “science education.” Social scientists, in essence, are often in a unique position to help clarify the ways in which *biophysical* as well as social science expertise is likely to be used within the policy process. What social scientists in such a position may often recognize, to be more specific, is a pattern that is as paradoxical as it is problematic.

We turn first to the paradox. Despite the fact that biophysical scientists may fail to realize how little they actually know about human behavior, or science in general, the very same scientists will often prove to be acutely aware of the limitations of their knowledge *within* their own subspecialties or areas of expertise. Like the fisheries scientist quoted above—and like perhaps the majority of our colleagues in the social sciences—they will argue against being “too hasty” in the face of acknowledged limitations of knowledge, being far more comfortable in arguing the need for caution than in arguing the need for action. What they often fail to recognize, however, is that the net result of their calls for caution can be management outcomes that are anything but conservative or cautious, particularly in the case of natural resources. The reasons have to do with characteristics of the *structure of decision making* that social scientists will often need to bring to the attention of biophysical science colleagues.

It is important to keep the following discussion in context; lest the matter not be clear enough already, accordingly, we wish to stress the relatively obvious point that we see the involvement of a full range of scientists in natural resource management as offering far more advantages than disadvantages. Despite the potential as well as the real advantages of such involvement, however, the actual track record of “scientific resource management” over the course of the 20th century has often been evaluated as having been far less successful than most of us would wish. To be sure, there have been at least a small number of success stories; “game” species such as deer have become more abundant during the course of the century, for example, and at least a small number of once-endangered species, including the national symbol, the bald eagle, are making significant comebacks. Weighing down the other side of the scale, however, are the many sobering cases in which even commercially valuable natural resources have been “managed” in a way



that seems almost impossible to describe in hindsight as having been enlightened or wise.

One example involves one of the most important fisheries in the history of the world: the vast cod fishery of the Grand Banks, off the coast of Maine and Newfoundland. As noted by any number of authors (e.g., Hamilton and Seyfrit, 1994; Palmer and Sinclair, 1997; Kurlansky, 1997), the net result of a century of effort to "manage" this phenomenal resource was that even the prolific cod stocks of the area were overfished so heavily that the fishery ultimately collapsed, leading to an official U.S. federal "disaster" proclamation that Cohen (1995) has characterized as "turning fish into pork." So serious was the collapse that, even after the virtual elimination of the fishing industry for the better part of a decade, there is still little evidence of any significant recovery by the species (Haedrich and Hamilton, 2000).

Although the Grand Banks example is clearly a dramatic one, however, it is by no means unique; on the opposite coast of the United States, for example, the natural salmon runs in the Pacific Northwest were once so vast that they helped to make this region's Native Americans some of the wealthiest residents of the so-called new world prior to the arrival of Europeans and their descendants. Starting at about the time of the U.S. Civil War, however, salmon canneries began to process the plentiful populations of these anadromous fish. Peak production was reached on the Sacramento River just two decades later, in 1882, and the last area of the Northwest to reach its peak, involving coastal Washington rivers, did so in 1915. Despite the expenditure of many millions of dollars in studies, hatcheries, and habitat management efforts in recent decades, the salmon populations have continued to decline; seven identifiable populations of salmon have now been named officially as threatened or endangered in the Columbia River Basin alone, and today there are no longer any canneries operating in the Pacific Northwest (for extensive historical accounts, see McEvoy, 1986; Lichatowich, 1999). Around the world, in fact, even the "pelagic" or open-ocean species that were once seen by respected scientists as being "inexhaustible" (see the discussion in McEvoy, 1986) have proven to be anything but. According to the Food and Agricultural Organization of the United Nations (1995), more than two thirds of all commercial fish stocks are being fished at or beyond their levels of maximum productivity; about 25 percent of stocks for which data are available have been classified either as being depleted or in danger of depletion, while another 44 percent of fish stocks have been characterized as being fished at their biological limit, and hence as being in danger of overexploitation if fishing pressures continue to mount (see also Malakoff, 1997).

The problem, moreover, is not limited to fish. In the case of nonrenewable resources such as petroleum, for example, the net effect of more than 25 years of efforts to boost U.S. petroleum self-sufficiency, dating back at least to what President Richard Nixon termed "Project Independence" in

1974, has been that the proportion of U.S. petroleum consumption supplied by domestic sources has dropped from 74 percent in 1973 to 43 percent in 1999; the major effect of the “self-sufficiency” effort appears instead to have been a reduction of U.S. oil reserves, which now amount to just 3 percent of world proven petroleum reserves (U.S. Department of Energy, 2000; American Petroleum Institute, 1998; Gramling, 1996). In the case of other renewable resources, meanwhile, only a handful of the 877 species that have been officially listed as “endangered” or “threatened” under the Endangered Species Act have been removed from that status because of recovery, as opposed to extinction (World Wildlife Fund, 1994). In the concise summary of the on-line version of the *World Book Encyclopedia* (Myers, 1999), matters scarcely seem to have improved during the era of modern science:

One of the most notable differences [from earlier centuries] is the rapid pace of today’s extinctions. Ecologists estimate that we have lost hundreds of thousands of species in the past 50 years. The experts predict that if present trends continue, we are likely to lose one-half of all living species within the next century.

Even for tree species that are nowhere near being endangered, meanwhile, the once highly regarded U.S. Forest Service (see, e.g., Kaufman, 1960) has come under extensive criticism in recent years for policy decisions that biophysical scientists and historians (e.g., Franklin and Kohm, 1999; Policansky, 1999; Langston, 1995, 1999; and Hirt, 1994, 1999) see as having been badly mistaken, reflecting what Hirt (1994) has termed “a conspiracy of optimism.”

According to a number of analyses, some of the key sources of such problems have to do with the structure of interests, rather than with the structure of scientific input, and there is a good deal of value in these analyses. With respect to fisheries resources, in particular, Gordon (1954) appears to have been the first to identify what is sometimes called “the fisherman’s problem”—in essence, the fact that, when no one “owns” the fish, those who do the fishing will have incentives to catch all they possibly can, even though those same individuals have a *collective* interest in conserving a higher proportion of fish stocks. In subsequent work, Olson (1965) noted similar differences between what would be rational for an individual and what would be rational for a larger group, and in a still-later *Science* article, Hardin (1968) used the memorable phrase of “the tragedy of the commons” to describe a comparable phenomenon, although Hardin has since been criticized for his substantial anthropological errors as well as for the potentially problematic nature of his policy recommendations (see, e.g., McCay and Acheson, 1987; for an extensive analysis of the concept and its evolution, see McEvoy, 1986).

In addition, however, we believe it is important to recognize at least two characteristics of the structuring of scientific input—of the ways in which

scientific information and judgment are brought into the policy process—that appear to exert influences of their own. Although we would not necessarily concur with Hirt's view that the net result should be seen as a "conspiracy" of optimism, the pattern that we have too often witnessed is that, paradoxically, a preference for *scientific* conservatism winds up producing *policy actions* that are anything but conservative.

The first of the two characteristics has received at least some attention in the peer-reviewed literature; it involves what Freudenburg and Youn (1999) have called "the asymmetry of scientific challenge." This phenomenon has to do with generally unintended yet cumulatively significant biasing pressures within what is taken to be "known" (or alternatively, "controversial") about likely impacts on natural resources and the humans who care about them.

Particularly in cases in which certain forms of resource use could lead to profits for a small segment of the public—a group having what Wilson (1980) might term a "concentrated interest," as contrasted against the broader public, which has a more "diffuse interest" in sound resource management—the parties having concentrated interests will often be organized in ways that are useful in exerting systematic pressures on scientific deliberations. At a minimum, research hypotheses and findings that have the potential to challenge the concentrated interests can be subjected to vigorous dispute and attack, while findings and interpretations that are more congenial to the concentrated interests may instead enjoy rapid acceptance. If fishing interests feel a scientist's estimates of a fish population are too low, for example, those estimates may well be subjected to a withering attack by fishing organizations that seek to avoid reductions in the quantities of fish that are allowed to be caught; if the estimates are too high, on the other hand, representatives of the fishing industry are unlikely to complain about being allowed to catch a few more fish, and other groups are unlikely to have the degree of interest or expertise that would be needed to carry out the potentially expensive research needed to correct the error. Particularly in cases in which the scientific data are less than definitive, accordingly, or in which it can be especially complex or expensive to learn "the true facts" about a resource, it is highly unlikely that "scientific findings" having negative implications for the near-term interests of a powerful resource-using industry will escape scrutiny and correction for an extensive period of time, while excessively favorable interpretations are far more likely to escape unchallenged.

If this first structural problem can be called one of "knowing" less than we appear to know—and thus of favoring actions that appear to be scientifically justified but that ultimately prove to have been ill-advised—the second problem is one of knowing more than may at first be apparent, but where the nature of scientific knowledge is sometimes obscured by the ways in which discussions are framed. This second problem can be observed directly through the "participant observation" that can take place through working

on interdisciplinary scientific committees, and thus it is worthy of additional attention here.

It is rare, after all, for species collapses or resource depletion problems to come as complete surprises. In many if not most such cases, respectable scientists issue warnings, often doing so in quite clear and distinct terms (see, for example, Hirt, 1994; Lichatowich, 1999; McEvoy, 1986). The problem is not that scientists fail to express concerns, but rather that the concerns go largely unheeded by the relevant resource management agencies and other decisionmakers—often until the damage to resources has become undeniable or, in some cases, irreversible. One obvious temptation, of course, is to conclude that such a pattern results from the tendency for economic interests to exert greater influence within the policy process than do scientific experts. Realistically, of course, we see the need to recognize the importance of such differences in political power, but to be equally realistic, we see it as important to recognize that such differences do not provide a full and satisfactory explanation. Instead, scientists are often complicit in their own lack of influence over policy decisions, albeit through patterns of behavior that are not evident to those same scientists until they are pointed out.

In the interest of simplicity, that pattern can be summarized in terms of a single question: Do we know enough to act? Most scientists, of course—whether biological, physical, or social scientists—have learned to avoid overstating the strength of scientific conclusions. One of the most common patterns in scientific articles, after all, involves an exhortation about the need for “caution,” followed by a conclusion that “further research is needed.” The problem is not the scientific penchant for conservatism—our own writings, after all, are heavily studded with the same warnings—but the specific *way* in which such conservatism is often factored into specific resource management decisions. Based on available research findings, for example, one or more scientists will raise expressions of concern; based on those concerns, either that scientist or some other actor(s) will argue for additional policy measures—reductions in the rate of resource harvest, for example, or the imposition of new pollution control regulations. It is often at that stage that another person—a resource manager, another scientist, or an interested party—will raise some variation on a simple but powerful question: *Are you sure?*

In many ways, of course, science is not “designed” to address the complex sets of questions involved in resource management. As Kuhn (1962) has pointed out, save for paradigm shifts, most of science is relatively mundane, narrowly focused, and incremental in nature; by contrast, questions of resource use are complex, broad, and often of great consequence. Still, the potential mismatch between information and “information needs” is by no means limited to natural resource management. The explosion of the space shuttle *Challenger*, for example, occurred only after the chief engineer initially refused to go along with the launching of the shuttle under such cold-weather conditions; ultimately he reversed himself, since he could not be

completely sure that the launch would be dangerous, while many other actors were intensely interested in hearing the opposite conclusion, given that the space agency was badly in need of political support and that the U.S. president wanted to refer proudly to the launch in his upcoming State of the Union address (see, e.g., Vaughan, 1997; Davis, 1989).

Yet the mismatch often proves to be particularly significant in resource management contexts. One illustration is provided by a recent case that was witnessed by one of the authors of the present article, when a federal fisheries management scientific committee was asked for advice about a proposed management option involving a specific species. The existing data showed that this prized species had been overfished, that it continued to experience overfishing, that roughly two thirds of the fish were being “taken” by tourist fishing operations known as charter boats, and that charter operations had increased dramatically over the previous decade. The agency had no interest in taking draconian measures, but the committee was asked if it would be scientifically prudent, under the circumstances, at least to refrain from issuing any *new* licenses for charter boat operations. Even though the scientists on this committee had shown themselves by deed as well as action to be deeply committed to conservation and species preservation, at least as individuals, one scientist after another dutifully raised issues of scientific conservatism. There could be no certainty, one of them noted, that such a measure would lead to a net reduction in rates of catch. Another observed that existing charter boat operations might well increase the number of trips if demand increased. In short, the data did not exist to demonstrate definitively that a moratorium on new licenses would stop the increasing fishing rate. The litany of cautions ended only after a committee member finally noted, in obvious frustration, that no decision of this committee in the past, and probably no decision of any fishery management advisory committee in history, had ever been based on complete knowledge. After that, other members chipped in that, although the proposed action might not be perfect, it was far more likely to help than to hurt, and ultimately the committee voted to endorse the proposal.

The frustrated scientist’s point is an important one. In real-world contexts of resource management, after all, decisions *will* be made. Those decisions will respond to legal and other pressures imposed on managers—not on the amount of information available—meaning that decisions will virtually always be made on the basis of less than perfect knowledge.

Given that agencies do not have the luxury of postponing decisions until all relevant research has been completed, most observers would conclude that it is generally better to rely on the best expert input available, no matter how incomplete, than to continue with “business as usual” approaches until true certitude emerges. This is particularly true given that scientific certitude is often a moving target, rather than an obtainable goal, while the failure to make a decision is often itself a decision, if only the decision to allow current practices to continue.

An awareness of this problem, of course, is one of the factors that has contributed to support for what has come to be known as “the precautionary principle” (for discussions, see Cameron and Abouchar, 1991; O’Riordan, 1995; see also Interorganizational Committee on Guidelines and Principles for Social Impact Assessment, 1993; Gramling and Freudenburg, 1996). As noted by Fisher (1999:184), a “standard definition” is provided by Principle 15 of the 1992 Rio Declaration on Environment and Development: “Where there are threats of serious or irreversible environmental damage, lack of full scientific certainty should not be used as a reason for postponing measures to prevent environmental degradation.”

This principle is indeed receiving increased attention throughout the industrialized world, but “Australia is one of the few places in the world where the principle has been put widely into legislation and considered in case law” (Fisher, 1999:186). Unfortunately, as pointed out by what may be the most careful analysis of the Australian case to have been published to date, the courts have consistently interpreted the principle in a way that “has not resulted in any fundamental legal reform in the way in which decisions are made or judged” but that instead has “reaffirmed” earlier approaches to environmental management (Fisher, 1999:184). A key part of the reason, remarkably, has been the apparent tendency for judges to assume that science provides certitude, meaning that they have characterized the precautionary principle *not* as being a common form of scientific conservatism, but as being the virtual opposite—akin to making decisions by flipping a coin or by consulting an astrologer (Fisher, 1999).

As in the case of risk management, in short, it may well be that the uneasy relationship between a search for certitude and a reality of ambiguity needs to be recognized as *one of the defining characteristics* of the decision-making process. This characteristic, in turn, can lead to yet another structural feature of the resource management process that works against cautious resource management. As noted by Freudenburg and Pastor (1992:401), even though health, safety, and environmental regulations are made and enforced in the context of just such uncertainties and ambiguities, “regulated industries may press for regulators to demonstrate that a regulation is *unambiguously* justified” (emphasis in original). Although the U.S. criminal justice system assumes that an accused person should be “presumed innocent until proven guilty,” in other words, the same is not true for a nonhuman resource in the civil justice system. Instead, as noted in the classic article by Galanter (1974), “repeat players” such as the organized interests of a resource-using industry often come to enjoy a position of privilege in the legal system, such that the burden of proof can come to be placed on those who oppose an industry’s plans or who argue for greater levels of precaution. Even where an agency has a statutory requirement to protect the biological health of a species or an ecosystem, in other words, we should perhaps not be surprised if the agency shows at least an equally keen interest in protecting the financial health of a resource-using industry.

## Discussion

The first main section of this article has made the point that we social scientists actually do know more—relative to biophysical scientists—than we sometimes give ourselves credit for knowing. The second has made the point that our biophysical science colleagues sometimes know either less or more than they give themselves credit for knowing—knowing less about certain assumptions or interpretations that happen to favor the near-term interests of resource-using industries, while knowing more about natural resources than they give themselves credit for knowing, particularly in the face of pressures for “scientific caution” (although not *resource management* caution) from the interest groups that try to influence agency decisions. These two points, when considered in conjunction with other salient aspects of the natural resource policy-making process, suggest that there may be value in viewing resource policy from a different perspective than has been common in the past.

As a useful simplification, although few of the biophysical scientists who become active in policy-making issues appear to have reflected extensively on the nature of their roles, a relatively typical view among the few who have done so would be that their role, as scientists, is to provide input on matters of fact, while the role of policymakers and the broader public is to provide input on matters of values. In the words of a scientist on one such panel who explained his thinking to one of us, “The agency’s job is to ask the questions, and our job is to provide the answers.” Clearly, these views reflect a good deal of logic, but the arguments put forth in this article suggest that it may be time to consider a different form of logic, as well. The point is not merely that scientists are not always so careful to keep their values out of their evaluations, but that they are often not even aware of the many ways in which scientific evaluations can be influenced by factors other than facts—starting with the importance of realizing that, if certain interests are able to frame the asking of questions, those interests can have exceptional influence on the development of answers, and hence of what are taken to be the relevant facts.

Part of the reason for suggesting such a perspective has to do with a characteristic that we would not want yet to claim to be universal, but that has been a part of every issue of resource policy that we have encountered in our combined experience, which now includes some 50 person-years of involvement. In all of the cases we have witnessed, the questions being raised by interested parties were beyond the capability of present-day science to answer. It may not be too soon to ask, in other words, whether at least some degree of controversy over “the” facts may be close to being an *inherent* characteristic of resource management controversies.

If so, then one of the implications may be that something resembling what Hirt called “a conspiracy of optimism” may in fact extend well beyond

the U.S. Forest Service. As Hirt has spelled out, the Forest Service has long had a policy of promoting a “sustained yield” of wood from the national forests, but for decades, even the best scientists had little way of identifying what the actual maximum sustained yield might be. As in the disputes in which we have been involved, the ability to ask a scientific-sounding question did not create an ability to answer the question scientifically. Although the matter may have been empirically underdetermined, however, it was clearly not “politically underdetermined.” Instead, lumber-producing interests and their allies in Congress were able to apply consistent and effective pressure to ensure that the numbers would not be too low; when seen from the perspective of hindsight, it appears, the net result may well have been that the numbers were significantly too high. For years, the actual “harvest levels” were in the range of 12 billion board-feet per year, and particularly during the Reagan administration, the official agency position came to be that the “true” maximum sustained yield levels would be in excess of 25 billion board-feet per year. One characteristic of *former* resource management controversies, unfortunately, is that the real-world experiment of resource management often does provide clearer evidence, including the evidence that an earlier estimate may have been too optimistic. As noted by historians such as Hirt (1994) and Langston (1995), the vast majority of today’s biophysical scientists have concluded that even the 12 billion board-foot harvest level was far too high to be sustainable and that it reflected not a sound or dispassionate science, but “an unjustified technological optimism” (Hirt, 1999:235).

Although Hirt’s focus was on the Forest Service, similar patterns have characterized all of the disputes in which our input has been sought. Even apparent questions of fact are often inextricably intertwined with problems of blind spots and scientific limitations, with the net result being that—even when the scientists in question were not merely committed to doing solid and balanced science, but were convinced that they were doing it—their involvement often worked to confer significant short-term advantages on the groups having a concentrated interest in the exploitation of the resource, often at the expense of the broader public and the resource itself. Few of those scientists have had any difficulty in recognizing this pattern in retrospect; equally few of them, unfortunately, appear to have been able to recognize it in advance. Perhaps matters would have been different if those scientists had come into more frequent contact with social scientists—particularly those social scientists who are familiar with the types of often-unseen, structural biasing pressures that have been identified in this article.

To note the obvious caveat, this article’s identification of such pressures should be considered preliminary, not definitive. To be sure, what we have observed to date is a pervasive pattern of pressures—one that has often gone unnoted even by the types of scientists who would be the most resistant to pressures that were visible or overt. Still, although our experience has been reasonably extensive, it should clearly not be considered the final word. Lest



the point not be clear enough already, accordingly, we wish to stress that no article such as this one should be said to “prove” the existence of a universal bias in the scientific work that is expected to provide the factual basis for resource policy decisions; indeed, only if an increased number of our social science colleagues gain experience in just such settings will it be possible to decide with greater confidence whether this pattern is indeed as pervasive as it currently appears to be. It is thus not just in the hope of increasing social science input into policy deliberations, but also in the hope that more of our social science colleagues will take advantage of the unique opportunities for *studying* policy deliberations through such participant observation, that we conclude by repeating our exhortation for more of our colleagues to seek and accept positions on the types of scientific committees that we have been discussing.

You can do so with confidence. Given the residual skepticism toward the social sciences by those who appoint committee members, the social scientists who are asked to join scientific committees can expect to have had their credentials scrutinized carefully—often more carefully than the more traditional physical scientist members. As a social scientist, accordingly, you can have a reasonable level of assurance that you are indeed qualified. More broadly, social scientists are in a position to bring unique contributions to such committees—and service on such committees, in turn, can offer unique opportunities for bringing important data to the social sciences.

## REFERENCES

- American Petroleum Institute (API). 1998. *Basic Data Book*. Washington, D.C.: API.
- Cameron, Janes, and Juli Abochar. 1991. “The Precautionary Principle: A Fundamental Principle of Law and Policy for the Protection of the Global Environment.” *Boston College International and Comparative Law Review* 14:1–27.
- Cohen, Maurie J. 1995. “How to Turn Fish into Pork.” *Journal of Commerce* (April 20):8A.
- Davis, Michael. 1989. “Explaining Wrongdoing.” *Journal of Social Philosophy* 20:74–90.
- Fisher, Elizabeth. 1999. “Changing the Mindset? Implementing the Precautionary Principle in Australia.” *Research in Social Problems and Public Policy* 7:183–98.
- Food and Agriculture Organization of the United Nations (FAO). 1995. *The State of World Fisheries and Aquaculture*. Rome: FAO.
- Franklin, Jerry F., and Kathryn A. Kohm. 1999. “How Science Altered One View of the Forest.” *Research in Social Problems and Public Policy* 7:243–51.
- Freudenburg, William R. 1996. “Strange Chemistry: Environmental Risk Conflicts in a World of Science, Values and Blind Spots.” Pp. 11–36 in C. R. Cothorn, ed., *Handbook of Environmental Risk Decision Making: Values, Perceptions, and Ethics*. Boca Raton, Fla.: CRC.
- Freudenburg, William R., and Susan K. Pastor. 1992. “Public Responses to Technological Risks: Toward a Sociological Perspective.” *Sociological Quarterly* 33(3):389–412.

- Freudenburg, William R., and Ted Youn. 1999. "Institutional Failure in Environmental Management: Toward a Fuller Understanding of Social Problems and Public Policy." *Research in Social Problems and Public Policy* 7:3–18.
- Galanter, Marc. 1974. "Why the 'Haves' Come Out Ahead: Speculations on the Limits of Legal Change." *Law and Society Review* 9:95–160.
- Gordon, H. Scott. 1954. "The Economic Theory of a Common-Property Resource: The Fishery." *Journal of Political Economy* 61:124–42.
- Gramling, Robert. 1996. *Oil on the Edge: Offshore Development, Conflict, Gridlock*. New York: State University of New York Press.
- Gramling, Robert, and William R. Freudenburg. 1996. "Environmental Sociology: Toward a Paradigm for the 21st Century." *Sociological Spectrum* 16:347–70.
- Haedrich, Richard L., and Lawrence C. Hamilton. 2000. "The Fall and Future of Newfoundland's Cod Fishery." *Society and Natural Resources* 13:359–72.
- Hamilton, Lawrence C., and Carole L. Seyfrit. 1994. "Resources and Hopes in Newfoundland." *Society and Natural Resources* 7(6):561–78.
- Hardin, Garrett. 1968. "The Tragedy of the Commons." *Science* 162(December 13):1243–48.
- Hirt, Paul W. 1994. *A Conspiracy of Optimism: Management of the National Forests since World War Two*. Lincoln: University of Nebraska Press.
- . 1999. "Institutional Failure in the U.S. Forest Service: A Historical Perspective." *Research in Social Problems and Public Policy* 7:217–39.
- Interorganizational Committee on Guidelines and Principles for Social Impact Assessment. 1993. *Guidelines and Principles for Social Impact Assessment*. National Oceanic and Atmospheric Administration Technical Memorandum NMFS-F/SPO-16. Washington, D.C.: U.S. Department of Commerce.
- Kaufman, Herbert. 1960. *The Forest Ranger: A Study in Administrative Behavior*. Baltimore: Johns Hopkins University Press.
- Kuhn, Thomas S. 1962. *The Structure of Scientific Revolutions*. Chicago: University of Chicago Press.
- Kurlansky, Mark. 1997. *Cod: Biography of the Fish that Changed the World*. New York: Walker.
- Langston, Nancy. 1995. *Forest Dreams, Forest Nightmares: The Paradox of Old Growth in the Inland West*. Seattle: University of Washington Press.
- . 1999. "Environmental and Human Change in Old-Growth Forests." *Research in Social Problems and Public Policy* 7:253–69.
- Lichatowich, Jim. 1999. *Salmon without Rivers: A History of the Pacific Salmon Crisis*. Washington, D.C.: Island.
- Ludwig, Donald, Ray Hilborn, and Carl Waters. 1993. "Uncertainty, Resource Exploitation, and Conservation: Lessons from History." *Science* 260(April 2):17, 36.
- Malakoff, David. 1997. "Extinction on the High Seas." *Science* 277 (July 25):486–88.
- McCay, Bonnie J., and James M. Acheson, eds. 1987. *The Question of the Commons: The Culture and Ecology of Communal Resources*. Tucson: University of Arizona Press.
- McEvoy, Arthur F. 1986. *The Fisherman's Problem: Ecology and Law in the California Fisheries, 1850–1980*. New York: Cambridge University Press.

- Moynihan, Daniel P. 1969. *Maximum Feasible Misunderstanding: Community Action in the War on Poverty*. New York: Free Press.
- Myers, Norman. 1999. "Extinctions Past and Present." *World Book Online*. Available at <<http://www.worldbook.com/fun/wbla/earth/html/ed12.htm>>.
- Olson, Mancur. 1965. *The Logic of Collective Action*. Cambridge: Harvard University Press.
- O'Riordan, Tim. 1995. "The Application of the Precautionary Principle in the United Kingdom." *Environment and Planning* 27:1534–38.
- Palmer, Craig, and Peter Sinclair. 1997. *When the Fish Are Gone: Ecological Disaster and the Fishers of Northwest Newfoundland*. Halifax, Nova Scotia: Fernwood.
- Policansky, David. 1999. "Natural Science, Social Science, and Policy in Resource Management." *Research in Social Problems and Public Policy* 7:311–22.
- Scott, Robert A., and Arnold R. Shore. 1979. *Why Sociology Does Not Apply: A Study of the Use of Sociology in Public Policy*. New York: Elsevier.
- U.S. Department of Energy. 2000. Crude Oil and Petroleum Products Overview, 1973–Present. Available at <[http://www.eia.doe.gov/pub/oil\\_gas/petroleum/data\\_publications/petroleum\\_supply\\_monthly/current/txt/table\\_s01.txt](http://www.eia.doe.gov/pub/oil_gas/petroleum/data_publications/petroleum_supply_monthly/current/txt/table_s01.txt)>.
- Vaughan, Diane. 1997. *The Challenger Launch Decision: Risky Technology, Culture, and Deviance at NASA*. Chicago: University of Chicago Press.
- Wilson, James Q. 1980. "The Politics of Regulation." Pp. 357–94 in James Q. Wilson, ed., *The Politics of Regulation*. New York: Basic.
- World Wildlife Fund. 1994. *The Official World Wildlife Fund Guide to Endangered Species of North America*. Washington, D.C.: Walton Beacham.