the Journal of Philosophy, Science & Law
www.psljournal.com

Volume 4, August 2004
www.psljournal.com/archives/papers/pfiesteria.cfm

Scientific Consensus and Public Policy: The Case of *Pfiesteria*[1]

Darrin W. Belousek*

* Department of Bible, Religion, and Philosophy, Goshen College

Abstract. This paper examines normative and political aspects of the peer review, scientific consensus and public policy processes related to harmful algal blooms of *Pfiesteria* in estuarine waters of North Carolina and Maryland in the 1990s. After laying out a brief science and policy case history, the tension between the scientific consensus and public policy processes in this case is analyzed in terms of conflicts between scientific norms, public values and political expediency. The relationship between scientific consensus and public policy in general is then questioned in light of this case.

1. Introduction

In 1991 North Carolina State University research scientist Dr. JoAnn Burkholder and her colleagues announced the discovery of a toxic dinoflagellate, a single-celled marine organism, which was named *Pfiesteria piscicida*, and concluded that it was responsible for major fish kills in estuarine waters of North Carolina. Their findings were published in *Nature* (Burkholder *et al.* 1992). She and some of her assistants also reported experiencing various negative health effects from working with the organism in the laboratory. In trying to bring the importance of these findings to the attention of both North Carolina state officials and scientific peers, Dr. Burkholder was met with both public and professional skepticism and soon found herself entangled in the politics of both the state bureaucracy and scientific community. This story was sensationalized in a popular book (Barker 1997). In the midst of this tangle of science and politics, no coherent state-level public policy was formed regarding either the human health hazard posed by or the environmental control of the organism.

After the release of the book in the popular press had brought national attention to the situation in North Carolina, the presence of *Pfiesteria* was found in conjunction with a minor fish kill in the summer of 1997 in a Maryland tributary of the Chesapeake Bay. Despite an ensuing media “feeding frenzy”, instead of a similarly messy tangle of science and politics, this event was followed by a fairly harmonious working relationship between Maryland's state public health and environmental management agencies and environmental science research community. The subsequent public policy-making process in Maryland quickly yielded two significant outcomes supported by scientific consensus—a policy to close rivers in which toxic *Pfiesteria* is found to be present in order to protect public health, and regulations mandating nutrient management practice for agricultural land use in order to enhance environmental control of the organism.

This study investigates how science and politics interacted to achieve or fail to achieve public policy supported by scientific consensus. The outcome of the interaction of science and politics is shaped by multiple, and often competing, values. Some of these values originate within science itself as a knowledge-seeking enterprise. Other pertinent values derive from the exigencies of the political process. Still other values are derived from neither science nor politics but rather reflect the human social ends that the public expects both science and politics to serve. The locus at which these values come into conflict usually occurs where uncertainties in the available scientific information bear upon competing interests. After developing briefly the respective case histories of *Pfiesteria* in North Carolina and Maryland, we'll proceed to analyze how such values functioned in the present case and then consider whether public policy ought to be based upon scientific consensus in the first place.

2. Case Histories

2.1 The science-policy background: estuarine nutrient pollution

Over the past thirty or more years, the international estuarine science community has developed a broad and deep consensus that increased loads of nutrients such as nitrogen (N) and phosphorus (P) into rivers and harbors is responsible for the pollution of many coastal waters around the globe, including the Abemarle-Pamlico (North Carolina) and Chesapeake Bay estuaries, with multiple measurable deleterious effects on local ecology and economy. In waters over-enriched by nutrients, growth of phytoplankton increases ('eutrophication'), and algae in nutrient over-enriched waters can bloom into dense populations that can have negative environmental and human health impacts, both direct effects to fish and humans via harmful toxins and several indirect effects, including loss of aquatic vegetation, depleted levels of dissolved oxygen and fish kills.[2] Because of its toxicity, *Pfiesteria piscicida* is classified as a 'harmful algae'.[3]

These deleterious effects of nutrient pollution comprise the primary public justification for a continuing coordinated process of research and policy in both the Albemarle-Pamlico and Chesapeake Bay estuaries that has altered the landscape of science and politics in the restoration and protection of coastal waters. For the purposes of this paper, it will be helpful to review briefly the history of environmental science and public policy regarding nutrient pollution in Chesapeake Bay, with a particular focus on Maryland.

From the late 1960s to early 1970s, the nutrient-reduction agenda of the environmental science and management communities focused primarily on controlling point-sources of P-inputs (e.g., municipal sewage treatment plants); here there was general congruence between the two, with management perhaps even somewhat out ahead of the scientific consensus. During the mid-1970s, in the wake of Tropical Storm Agnes, the scientific consensus moved ahead of management to emphasize the need to control N as well as P from point sources and to control non-point sources of N (e.g., suburban and rural land run-off) as well as point-sources; the management community lagged behind partly because of technical and economic factors in point-source N-removal. From the late-1970s to the mid-1980s the EPA's Chesapeake Bay Study synthesized and propelled the scientific consensus on nutrient reduction, especially regarding non-point sources, leaving a ten-year lag between science and management. In the mid- to late-1980s, the management community began to catch up with the scientific community following the Bay Study, leading to Maryland's 1985 phosphate ban and the multi-state 1987 Chesapeake Bay Agreement that called for a 40% reduction in the bay by 2000. By the early 1990s, the scientific and management communities were beginning to converge on the nutrient-reduction agenda, particularly in regard to controlling non-point sources.[4]

During the period 1991–1997 the environmental advocacy, science, and management communities were building a solid shared consensus that agricultural sources are the major contributor to nutrient pollution in the Bay and, hence, were converging rapidly on mandatory non-point source nutrient controls for agricultural land use practices as the next phase in the nutrient reduction agenda.[5] An advancing edge in the agricultural research community was pointing toward the need to rethink the contribution of agricultural land run-off to nutrient pollution and the need for mandatory nutrient management practices.[6] Lagging well behind this advancing consensus and agenda, the Maryland state agricultural research and management communities, however, with few exceptions, held to its conventional line that agriculture did not present a nutrient pollution problem and was largely unmoved by new scientific evidence that voluntary implementation of standard soil and water conservation measures and conservation tillage practices were ineffective in controlling farmland run-off.[7]

2.2 North Carolina

Within the environmental science research community in North Carolina there was considerable dispute over *Pfiesteria* concerning two main issues in particular—viz., whether massive fish kills occurring on the Neuse River were due chiefly to *Pfiesteria* or depleted oxygen; and, whether *Pfiesteria* posed a significant human health risk to those exposed to *Pfiesteria* in open water. The latter scientific dispute, because of its public health implications, embroiled the scientific research community (especially Dr. JoAnn Burkholder) in a highly politicized controversy aired publicly in the media.

Pfiesteria is by all accounts a rather bizarre, complicated organism, regarding its life cycle, nutrition and toxic-stage behavior.[8] This strangeness led to keen interest among some and appropriate skepticism among others when it was first reported to the scientific community in connection to estuarine fish kills. Dr. Burkholder's early laboratory research on fish bioassays seemed to show fairly conclusively that *Pfiesteria* in its toxic stage could cause fish mortality at sufficiently high concentrations of the organism. Whether *Pfiesteria* is the singular primary cause of fish kills in estuarine rivers, however, cannot be inferred with certainty from such laboratory results because in the field there are always multiple confounding factors. In connection with the fish kills on North Carolina's Neuse River in 1995, for instance, both high *Pfiesteria* concentrations and toxic *Pfiesteria* activity as well as low dissolved oxygen levels were variously observed. Dr. Hans Paerl, of the University of North Carolina Institute of Marine Science and Burkholder's chief local scientific rival, concedes that *Pfiesteria* may be a secondary factor as an opportunistic organism that strikes already-stressed fish populations; but, he claims, low dissolved oxygen is the primary stressor responsible for the fish kills.[9] This puts him at odds with Dr. Burkholder; and their scientific dispute eventually made its way onto the pages of a prominent marine biology journal, with Burkholder unabashedly charging Paerl with poor science and questionable professional ethics (Paerl *et al*. 1998, Burkholder *et al.* 1999, and Paerl *et al*. 1999).

This ongoing dispute between Burkholder and Paerl gained public attention in a *Pfiesteria* research peer-reviewed grant process administered by North Carolina Sea Grant for North Carolina Department of Environmental Health and Natural Resources (DEHNR) in 1995–96.[10] In 1994, Burkholder appealed to Gov. Hunt and other state officials to support her *Pfiesteria* research and persuaded North Carolina state Health Director Ron Levine to set aside some $600,000 of unearmarked government funds for *Pfiesteria* research. She claims she was not seeking direct state funding for her research, but she understood that there was a tacit agreement with Levine that all of the research funds would go to her. DEHNR, though, instead decided to distribute the funds through North Carolina Sea Grant, which routinely administers research funds for the state. This meant that Burkholder would have to compete with other researchers for the funds as part of a standard scientific peer-review process. She objected to this decision, claiming that it would unnecessarily delay crucial research and result in the funding being spread out among less qualified researchers inexperienced with *Pfiesteria*. Despite these objections, she submitted a research proposal seeking 75% of the available funds. Her research group received 75% of the funds she requested, which amounted to more than 50% of the total funds awarded to four different research groups including hers. In particular, she lost out on the funding of the key nutritional ecology study, which went to Paerl's research group. She objected to this outcome, claiming that Paerl's research proposal contained errors, that irregularities had occurred in the peer-review process and that she had been deliberately sidelined in *Pfiesteria* research by state officials and (then) Sea Grant Director Dr. B.J. Copeland. Consequently, in protest she returned to the state 80% of the funds awarded to her group, keeping only money funding projects already underway. In the wake of all this, the Governor demanded an accounting of the peer review process from Copeland, and both the North

Carolina State University Chancellor and the state legislature launched investigations into the dispute. In the end, Burkholder received the funding she had returned via a direct grant from DEHNR Secretary Jonathan Howes, who publicly apologized for how his agency had treated her, and Paerl was cleared by the legislative audit. In the political fallout, however, Sea Grant Director Copeland was dismissed by the NCSU Chancellor even though the executive-level investigation found no wrongdoing on his part.

This official controversy spilled over into the debate concerning whether exposure to *Pfiesteria* in open waters posed any serious human health risk. One of the studies funded by the Sea Grant peer-review process was an epidemiological study by Dr. David Griffith of Eastern Carolina University (Griffith *et al*. 1998). This study, which found no serious public health threat posed by *Pfiesteria*, was challenged publicly by Burkholder as scientifically inadequate when a preliminary report was released in April 1997 (Sheehan 1997). When the final report of both this study and a Maryland public health study appeared in 1998 bearing conflicting conclusions, the dispute erupted again in the media, with Burkholder siding publicly with the Maryland study.[11] Consequently, the Griffith study quickly became mired in the murky politics of *Pfiesteria* in North Carolina, buried beneath accusations by Griffith of risk exaggeration (Griffith 1999a, b) and counter-accusations by Burkholder of poor science and questionable professional ethics (Burkholder and Glasgow 1999; cf. Lewitus *et al.* 1999 and Oldach 1999). This dispute inevitably drew in the public health officials responsible for making public policy decisions, such as state Health Director Dr. Ron Levine, who found himself under fire from Burkholder and environmental activists in 1996–97, accused of doing too little, too late to protect public health from *Pfiesteria* (Ready and Sheehan 1997 and Clabby 1997). In the end, Levine resigned as state Health Director and two other state health officials lost their jobs over the controversy.

2.3 Maryland

During 1996 and 1997, there were several reports of strange lesions on fish found in rivers on Maryland's Lower Eastern Shore. Just prior to the fish kill in the Lower Pocomoke River in August 1997, the state convened a Technical Advisory Committee (TAC) consisting of experts in fish pathology, algal ecology and environmental toxicology to evaluate scientific evidence linking fish lesions, water quality and *Pfiesteria*. The TAC initially concluded in its Interim Report that there was no conclusive evidence linking the fish lesions to *Pfiesteria*, which by appearances could have had multiple causes, or *Pfiesteria* to specific forms of pollution or human activities (Center for Environmental Science 1997a). The TAC reconvened a month later after the fish kill to review new evidence. By contrast, its second report concluded, despite caveats about uncertainties, that *Pfiesteria* was the most probable cause of the fish kill and the observed lesions and that nutrient pollution from human activity was the most likely environmental factor promoting the growth of *Pfiesteria* (Center for Environmental Science 1997b). Regarding the policy implications of these findings, whereas the initial report recommended continuing with the current nutrient reduction policy as the only solution, the second report advised that the current policy was likely insufficient to improve water quality enough to significantly control *Pfiesteria* and its effects.

Along with the reports of fish lesions, anecdotes of watermen that work the waters with the lesioned fish complaining of various strange neurological symptoms had also been accumulating during the previous year. Facing a potential public health crisis in the wake of the fish kill, state health officials directed a medical team to conduct a field study of people complaining of symptoms. On the basis of their findings, the medical team recommended to state health officials that the river be closed, which the state did just prior to the Labor Day weekend. A follow-up, longer-term clinical study of the same subjects studied in the field confirmed the initial findings of acute neuropsychological dysfunction and concluded that such symptoms were correlated with open-water exposure to *Pfiesteria* (Grattan *et al.* 1998). This study left open the question of a causal explanation of the symptoms, leaving uncertain the degree of risk to public health posed by *Pfiesteria*, a point emphasized elsewhere by one of the study's authors (Oldach 1999). Subsequently, the state Department of Natural Resources in consultation with the state health department developed a policy to close rivers to public use where recent toxic *Pfiesteria* activity is found to be present, using observation of *Pfiesteria*-associated fish lesions as the sole environmental indicator for such activity based on the TAC consensus (Department of Natural Resources 1997).

In September 1997, Maryland Governor Glendening appointed a Citizens *Pfiesteria* Action Committee to make public policy recommendations about specific environmental regulations related to controlling *Pfiesteria* prior to the January 1998 legislative session. While this commission was in process, Dr. Donald Boesch, President of the University of Maryland Center for Environmental Science and ex officio member of the Governor's bay cabinet, after recommending to the chairman of the Commission that the process needed to be informed by the best scientific information available, convened a panel of scientists to review the available information and draw a consensus on the linkage between *Pfiesteria* and nutrients. This scientific panel, the Cambridge Forum, which included Dr. Burkholder and relied heavily on her research, concluded after its meeting in October 1997 that, "In the long term, decreases in nutrient loading will reduce eutrophication, thereby improving water quality, and in this context will likely reduce the risk of toxic outbreaks of *Pfiesteria*-like dinoflagellates and harmful algal blooms" (Center for Environmental Science 1997c). In other words, against the background of the scientifically fortified and politically entrenched environmental management agenda of nutrient reduction in Chesapeake Bay, the scientific consensus made the effectively conservative policy recommendation, "more of the same." The Citizens Commission then used this scientific consensus—in addition to the advancing edge of agricultural research on nutrients and farmland run-off that, due to prevailing political conditions, was forcing along the state agricultural community (College of Agriculture and Natural Resources 1997)—as the basis for recommending a policy mandating stringent nutrient management practices for nearly all agricultural operations in the state (Hughes 1997), a policy that was enacted into law in 1998 as the Water Quality Improvement Act (WQIA), which is described as "the most comprehensive farm nutrient control legislation in the country" (Maryland Cooperative Extension 1998).

3. Norms in Conflict

What we aim for in this section is not an "external" social-theoretical analysis of this case in terms of abstract sociological theories and concepts, but rather an "internal" analysis on the scientific community's own terms: Where did the scientific community fail to live up to its own norms in forming consensus on policy-relevant questions? And how, if at all, did those failures affect either the scientific consensus or the public policy based upon scientific consensus? By initially orienting such questions from the perspective of the scientific community, we do not intend to adopt a naïve 'realism' about science and policy (see next section). Nor do we suppose that an "internal" perspective by itself is, in the last analysis, sufficient for characterizing both actual scientific practice and the actual relationship between scientific consensus and public policy; indeed, a fully adequate characterization of science-policy interaction requires as well analysis from an "external" viewpoint, some aspects of which we will consider in the next section. Nonetheless, it would seem that a comprehensive accounting and assessment of the relationship between science and policy would be incomplete without an "internal" perspective; and taking seriously the self-understanding of scientists regarding their own practice need not presume a 'positivist' value-neutral epistemology, a matter to be taken up in a further study.

3.1 Normal v. Mandated Science

Science as it functions within the professional boundaries of the scientific research community itself and science as it functions under mandate from government officials/agencies can often be in considerable conflict because of the differing values and standards used to judge science within these respective contexts. 'Normal science' is that everyday science that may be described as being framed professionally and institutionally within a particular 'paradigm' of research or 'research program'. Merton (1942) articulated the 'ethos' of 'normal science' in terms of four norms of practice: universalism, communalism, disinterestedness and organized skepticism, which characterize the egalitarianism, economics, politics and sensibility of the scientific research community, respectively.[12] He did not understand these norms to be sociological descriptions, factually descriptive characterizations universally true of actual scientific practice; science, always and everywhere, surely does not actually function according to these norms. Nor are these norms to be taken as mere social conventions, representative of a power balance achieved by negotiation between competing factions within the research community; thus, they are not to be sacrificed whenever they interfere with individual advantage.[13] Instead, Merton understood these norms as "institutional imperatives (mores)" that are derived from the collective goal (viz., "extension of certified knowledge") and the technical methods (empirical evidence and experimentally confirmed laws) of science; together with the technical standards of logical consistency and accurate prediction, he claimed, these norms (ideally) promote progress toward the goal of science.[14]

For the scientific research community, such norms do express ideals to which scientists themselves for the most part subscribe, whether or not these are always followed in actual practice. From an "internal" perspective, these norms are primarily prescriptive, defining patterns and standards of behavior which scientists *expect* their colleagues to follow. The source of the prescriptive force of these norms is the research community itself, its aims, practices and institutions; and the scientific community observes itself regarding these norms (though imperfectly, for sure). There has been much discussion recently, for example, about scientists keeping data secret and patenting research techniques and products; and scientists describe a trend of "decline" from the norm of communalism due to "external" values such as pressure from corporate patrons and the lure of financial profit (Marshall 1990). Scientists themselves can observe an ethic to decline only once it has already been (perhaps tacitly) accepted as prescriptively normative for their community. Serious violation of these norms can be met with unofficial sanctions of various forms: denial of promotion, denial of funding, loss of position or pressure for resignation, loss of editorial positions, refusal from colleagues to cooperate in research, etc. These norms thus also function effectively as social indicators. Following these norms in one's research signals to other scientists that "you are one of us," a member of the research community, ready and willing to "play by the rules" of science; conversely, failure to follow these norms opens a question in the mind of the community as to the authenticity of one's credentials as a scientist.

A brief discussion of these norms will assist in the analysis to follow. The norm of universalism expresses the idea that science as an institutionalized practice seeks knowledge characterized by an objectivity that transcends race, nationality, religion, class, gender and personal biases; there is to be no "particular" science, but one "universal" science that is true (or false) independently of the particularities that characterize individual practitioners of science and the research communities in which they practice (even though scientists and research communities cannot escape such particularities in actual practice). Communalism is the notion that in science there is to be a "common ownership of goods" that the scientific community produces, exhibited concretely in the practices of open sharing of data and materials; there are to be no individual proprietary rights in science except the right to recognition of claims of priority and originality and to institutional rewards such as prizes and promotions (i.e., scientists are never exclusive owners of their own discoveries). Disinterestedness, while not necessarily a character trait of individual scientists (each scientist has his or her own particular interests and motivations for doing science), signifies the idea that science is to have collectively neither a particular "clientele" (as the legal and medical professions do) nor a particular "patron" (as the arts might) whose interests foreign to science can compete with or take priority over the institutional goal of objective knowledge. Unlike literature and the performing arts today existing in symbiosis with professional critics, and unlike philosophy in medieval scholasticism being prized as a "handmaiden to theology," science is to be judge of itself as science (i.e., as a knowledge-seeking enterprise); and such autonomous judgment is to be realized through the multi-faceted activity of 'peer review.' Organized skepticism refers to the collective attitude with which the scientific community filters knowledge claims in order to winnow out a stable consensus widely shared within the community; this involves the skeptical scrutiny of knowledge claims according to logical and empirical criteria detached from implications of the moral value and social utility of science. This scientific sensibility, undisturbed by the siren calls of profits and fame and unharried by the pressures of funding and politics, tends to follow (ideally) Francis Bacon's dictum that "truth is the daughter of time" rather than of authority or tradition.

None of these norms stands by itself in the practice of science: if the scientific community fails to observe any one norm, then its ability to fulfill all the others, and hence achieve its institutional goal, is compromised. They operate socially and institutionally within what may be described as a 'knowledge filter' (Bauer 1992).[15] The knowledge filter functions over an extended, undetermined period of time to take initial speculations, hunches and ideas that are generally unreliable and subjective (influenced by human biases and fallibility) and transform them into relatively objective and reliable knowledge (viz., textbook science) via a series of (imperfect) 'filtering' mechanisms whereby "individual frailties or imperfections must run the gauntlet of communal scrutiny" before hypotheses are accepted as knowledge: peer review of research grant proposals, replication of research results in multiple trials, comment by colleagues in research seminar presentations, comments by editors and referees of professional journals, testing and use by other scientists that modifies and extends the original work, etc.

'Mandated science' is science done for public policy-making purposes and takes two forms: original investigations commissioned by government officials or regulators, and reviews (either by an individual or by a consensus committee) of peer-reviewed science done originally for 'academic' purposes (Salter 1988). Mandated science relies upon the ideal image of normal science as objective knowledge. Indeed, the very turn to science by government officials and agencies concerning public policy matters that bear on competing private interests is based upon the assumption that science can be a 'neutral arbiter' (or, at least, an 'honest broker') between those interests. The exigencies of the political process, however, often make demands upon science that compromise the norms of science. Chief among those demands is time: when government mandates science to provide information to aid in decision-making, science must conform to the time-cycle of politics if it is to be relevant to public policy. Such time constraints can result in science-based public policies that reflect an uncertainty-laden, quickly-formed consensus of a mandated report instead of the time-consuming, carefully-winnowed consensus of the 'knowledge filter.' Not only the pressures of politics but the social significance of the science can permit serious distortion of normal science by the media and invite interference by public officials, and the lure of public recognition in high profile cases (like this one) can lead scientists to circumvent or abandon the accepted norms.

There are several aspects in which this case exhibits significant tension between normal and mandated science as well as failures of the research community to observe the norms of scientific practice. Here we will focus on three episodes in particular, the North Carolina Sea Grant research funding process in 1995–96, the public policy-making process in Maryland in 1997-98, and the Maryland and North Carolina consensus committee processes in 1997.

3.2 Peer Review/Openness v. Politics/Interests

The North Carolina Sea Grant funding process was marked throughout by departures from the norms of disinterestedness, communalism and organized skepticism. The process began with a personal appeal by Dr. Burkholder to Gov. Hunt for what would have been (by all appearances) direct state funding for her research. Now, while government funding of research does not of course necessarily distort normal scientific practice, the standard mode by which the state's interest is mediated to the scientific community is the peer-review system so that it is the scientific community that judges according to its own standards, criteria, and values what or whose research is worth funding. When state officials decided to channel the *Pfiesteria* funding through a standard scientific peer-review process, Dr. Burkholder objected, claiming that doing so would cause unnecessary delays and end up funding less qualified researchers. When the peer-review process did not fund all the research she had proposed, she not only rejected the process as lacking integrity and withdrew, again seeking (and this time obtaining) direct funding from the state, but subsequently she refused to openly share data and materials (specifically, assistance with organism identification, maps of *Pfiesteria* blooms, and *Pfiesteria* culture samples) with those researchers whose proposed projects were funded. And as soon as preliminary reports of some of those research projects were released by Sea Grant, she criticized that research through the media rather than through the standard modes of scientific communication. Such a personality-driven, media-exposed affair in science invited politicization of and interference with the peer-review process, which resulted in science being subjected to the standards of public rationality and political process.

All this helps make sense of the (hyperbolic) remark by Dr. Joe Ramus of the Duke University Marine Biology Laboratory that this chain of events amounted to "the total failure of a principle of science. The peer-review process has been circumvented...it has collapsed."[16] From her own perspective, Dr. Burkholder agrees that the peer-review process has fallen apart, but she locates the problem with others in the scientific community whose criticism of her work, she claims, "has gone beyond healthy scientific skepticism."[17] Nonetheless, she candidly asserts that the standard peer-review process shouldn't even apply in this case, which in her view presents a fundamentally different situation because, as she sees it, most of her peers are simply unable to adequately assess her unique expertise in *Pfiesteria* research: scientific peers who lack experience with *Pfiesteria*, she says, can't appreciate her and her collaborators' competence to work on *Pfiesteria* and, hence, should not be permitted to evaluate her research proposals.[18] This way of thinking carried over directly into her decision not to supply cultures to her competitors for the Sea Grant funding—viz., because in her eyes they lacked the requisite technical competence and professional integrity. An additional, non-trivial issue in the sharing of *Pfiesteria* cultures is the time and expense involved in producing and maintaining the cultures. Dr. Burkholder (understandably) does not want her laboratory to be reduced to a culture-producing factory serving other researchers (which, she says, would happen if she directed the efforts of her assistants to fulfilling every request for cultures). She is clear to say that if given funding for culture production, she is willing to share her cultures with other researchers, subject (she emphasizes) to her private assessment of their competence and character.[19]

Dr. Burkholder's decision to provide data, materials, and cooperation on a qualified, private-assessment-only basis to scientists whose research proposals had been approved by a standard peer-review process raises the question of who decides who is worthy and competent to perform research. Her response to the peer-review process shifted that question concerning her own research from the arena of scientific evaluation to the arena of politics and advocacy, in which she enjoyed "patronage" relationships (viz., favor from both the Governor's office and environmental groups). She rationalized this response by a claim of unique qualification. This in effect created an exception for her and her research from the standard practice of the research community, which runs contrary to the accepted scientific norm of open sharing of data and materials. This norm has been recently re-articulated and re-affirmed unequivocally by the National Research Council:

> …[M]ost arguments for making exceptions to standards could not be rationalized without sacrificing the integrity of the principles of publication…[E]xceptions unfairly penalize the community, which would have otherwise had access to the data, information or material being withheld. Furthermore, granting a special exception to certain categories or particular researchers is problematic for a variety of reasons, including the difficulty of deciding who qualifies for the exception…Universal adherence, without exception, to a principle of full disclosure and unrestricted access to data and materials that are central or integral to published findings will promote cooperation and prevent divisiveness in the scientific community, maintain the value and prestige of publication, and promote the progress of science. (National Research Council 2003, pp. 13-14)

This norm of science is not, of course, a sacrosanct or inviolable imperative. There may be times and circumstances in which overriding this norm for the sake of protecting and preserving a higher good is warranted. A case in point might be the situation in 1939, in which British and American atomic scientists found themselves facing the uncertain potential for a ghastly weapon of mass destructive capacity latent in the discovery of uranium fission in Germany at the end of 1938. At that time, in the midst of mutual suspicion and a breakdown of communication between Continental and Anglo-American scientists under the stress of a looming European war, a number of scientists, especially a group of recent émigré scientists who were political refugees from fascism, sacrificed the norm of openness and sharing with the aim of preventing that destructive potential being actualized and exploited by the Nazi German regime for immoral purposes. Whether or not they were warranted in doing so, these scientists tacitly acknowledged that one is *un*warranted in sacrificing this norm *unless* one can argue that it is overridden by the strongest of reasons (cf. Jungk 1958, chap. 5). Such strong overriding reasons are not apparent in the present case regarding the norms of peer review and openness.

It thus seems plausible to conclude here that there occurred in this case a significant compromise of normal science in the midst of a public-policy relevant scientific dispute that pitted political and bureaucratic values (viz., efficiency of process and inscrutability of expertise) and techniques of advocacy (e.g., use of media and exposure of opponents) against the scientific norms of independence of peer review and openness of data and sharing of materials. This compromise was engendered by the elevation of one scientist's expertise above peer criticism and, correspondingly, (at least a temporary) exemption of one scientist's research from the skeptical scrutiny of the locally relevant research community. The resulting dysfunction of the research community on its own terms consequently undermined potential for a scientific consensus unmixed with local politics that could inform policy with at least some veneer of objectivity that would be convincing to the public (see below).

3.3 Skepticism v. Time

In the Maryland public policy-making process in 1997, one sees a tension between the normal scientific sensibility of organized skepticism and the time-pressures of politics. Although the initial TAC consensus confirming the *Pfiesteria*-fish lesion linkage was hedged with caveats about uncertainty, politics could not wait for pathology. At stake were certain public values—including cultural,[20] economic[21] and, as already mentioned, public health factors—that, compounded by media attention[22] and unfounded public fear[23] that were dubbed "*Pfiesteria* hysteria" (Greer and Leffler 1997), put the policy-making process under pressure to produce politically effective results. The chief question here is not what political *motivations* beyond concern for public health and protection of natural resources Governor Glendening might have had (say, re-election in 1998) in appointing the *Pfiesteria* Commission and setting its timeline for action, or whether such political motivations may have conflicted with the epistemological pursuits of the scientific research community. The primary issue here is the *structural-institutional* mismatch between science and politics—viz., that public policy-making inevitably conforms to legislative patterns and election cycles that necessitate timely compromise accountable to public values, while normal science (except indirectly via periodic funding requests) does not tend to do so.

Thus, Maryland's Governor-appointed Citizens *Pfiesteria* Action Commission could not wait in the fall of 1997 for the lesion question to be resolved by further research prior to addressing the more legislatively salient issues of nutrient pollution and land use regulation in time for the start of the legislative session in January 1998. In meeting its mandated time constraint, the Citizens Commission, confident in the early TAC consensus, by-passed the lesion question and went straight to the nutrients issue. Meanwhile, laboratory fish pathology investigations were conducted on the etiology of the fish lesions associated with *Pfiesteria* blooms in Maryland's Lower Eastern Shore rivers, with preliminary results in 1998 showing multiple possible causes (Blazer *et al.* 1998 and Kane *et al.* 1998). Thus, during the course of the year since the TAC concluded that *Pfiesteria* was the most probable cause of the lesions, the scientific uncertainty over lesion etiology was only magnified by the organized skepticism of the research community.[24] As a result of the research results released in 1998, the TAC subsequently vacillated on whether *Pfiesteria* was even a primary cause of the lesions—"*Pfiesteria* toxins...cannot be ruled in or out as initiators of fresh lesions or deep ulcers" (Center for Environmental Science 1999)—and Maryland Department of Natural Resources officials revised the river closure policy accordingly.

Guided by the early TAC consensus on lesions based on available information, the public policy-making process eagerly leapt ahead in the chain of causation to those linkages—*Pfiesteria*-nutrients and nutrients-land use—having the greatest political currency for leveraging change in current agricultural nutrient control policy. Consequently, the scientists of the Cambridge Forum, convened in order to inform the public policy-making process, operated under the constraints of the Citizens Commission agenda—viz., to reach consensus solely on the *Pfiesteria*-nutrients linkage— and therefore effectively bracketed the *Pfiesteria*-lesions linkage, which was presumed by the Commission. As further scientific evidence on lesion etiology became available, the TAC appropriately revised its consensus to loosen the *Pfiesteria*-lesion linkage, but too late to have any effect on a nutrient management policy-making process that had already been completed several months earlier. Now, even had the public policy-making process waited on the peer-review process, it cannot be said with certainty what impact this might have had on the politics of *Pfiesteria*. As the TAC was clear to point out in its latter report, regardless of lesion etiology, *Pfiesteria* remains both a fish and human health concern (Center for Environmental Science 1999). Nonetheless, the effect of this time-lag between mandated and normal science was that environmental interests were able to use the early consensus as leverage for their political agenda of stronger nutrient control, playing public resource values against private property interests, a strategy that would surely have been confounded by the later consensus on lesions despite the TAC caveat regarding public health.[25] Moreover, considering sentiment among Lower Eastern Shore voters just prior to the 1998 election and the reaction of one member of the state Agricultural Advisory Board to the further research on lesions (Smith 1998 and Shelsby 2000), it seems safe to say that agricultural interests would have played up the increasing scientific uncertainty as calling into question the whole of the *Pfiesteria* hypothesis as an insufficient scientific basis for the nutrient control legislation.

Thus we see that the mismatch in time-scale between the public policy-making process and the peer-review process likely benefited those political interests in favor of stronger regulation of agricultural land use. Because of this mismatch, mandated science in this case was effectively biased toward environmental interests, and thus the ideal image of normal science as objective was compromised. Does this imply that the scientists involved failed to be 'honest brokers' of information? Answering that question may depend upon what type of risk assessment strategy one thinks scientists should use to evaluate hypotheses in the face of uncertainties, a topic beyond the scope of this paper.[26] For sure, though, the upshot here is that mandated science cannot always achieve a timely consensus on new and limited information that is neutral between competing political interests.

3.4 Universality v. Personality

The norm of universalism was seriously compromised in the consensus committee processes in North Carolina and Maryland, which suffered from personality conflicts within the scientific community stemming from the public controversy in North Carolina. The norm of universalism regarding scientific consensus concerns chiefly who is, and who is not, invited to the consensus table, and who decides. [27] In this case, Dr. Burkholder was in effect given power to determine (at least in part) who was and who was not invited to both the TAC and the Cambridge Forum; and that power was granted (in part, at least) for sake of political expediency.

Dr. Burkholder was invited to serve on the TAC at the request of Maryland State Secretary of Natural Resources John R. Griffin, from whose department the TAC received its mandate. Dr. Burkholder initially hesitated to attend the first TAC meeting, however, because public health officials from North Carolina had also been invited. She agreed to attend only on the condition that they not attend; and so they were uninvited.[28] Thus, the political situation in North Carolina effectively limited the scientific consensus in Maryland.

The convening of the Cambridge Forum presents a similar scenario. After the TAC's second meeting, Gov. Glendening appointed the Citizens *Pfiesteria* Action Commission. The Cambridge Forum, while not officially mandated by the *Pfiesteria* Commission, was convened by Dr. Boesch for the purpose of informing the policy-making process. Dr. Burkholder gave testimony by invitation at the Commission's first meeting and then testified on *Pfiesteria* before a U.S. House of Representatives subcommittee just a few days later. By this point, she was *the* recognized expert on *Pfiesteria* as far as the Commission was concerned; hence, if the Cambridge Consensus were to carry weight with the Commission, Dr. Burkholder would have to be included. Above politics, scientific expertise was also at stake; at the time, Dr. Burkholder had authored most of the peer-reviewed published literature on *Pfiesteria*. Now, Dr. Boesch had initially wanted to invite both Dr. Burkholder and her North Carolina rivals Dr. Paerl and Dr. Pat Tester to be on the Forum panel, even though he was well aware from the book

And the Waters Turned to Blood that there were serious tensions between them. Dr. Burkholder, however, balked: it was either her or them; she would not come if they were invited. So, it would have to be Dr. Burkholder. Again, to be fair, it should be said that Dr. Boesch's decision itself was based at least as much on the question of scientific expertise as on politics; Dr. Paerl's research was as yet unfinished and unpublished (Paerl and Pinckney 1998).[29] Nonetheless, that there was an "either-or" choice to be made was a matter of personality conflict stemming from the political situation in North Carolina; and that conflict effectively diminished the universality of the scientific consensus committee. According to Dr. Thomas Malone, member of the Cambridge Forum, there was not an optimum mix of scientists on the Cambridge Forum, because of both time constraints on drawing the committee together as well as the impact of Dr. Burkholder on who could and couldn't be on the committee.[30]

Similar problems were faced in North Carolina by those who in December 1997 convened the scientific panel that produced the Raleigh Report (Water Resources Research Institute 1998). The responsibility for convening the panel fell to Marion Smith, then assistant to the North Carolina Secretary of Environment and Natural Resources, and Dr. Kenneth Reckhow, Director of the Water Resources Research Institute at North Carolina State University. In Smith's view, dissension between Dr. Burkholder and other North Carolina scientists was inhibiting effective influence of science on policy; thus there was a need for independent evaluation of *Pfiesteria* research to help the policy process along.[31] When Dr. Reckhow initially contacted Dr. Burkholder to discuss what areas of expertise should be represented on the panel, which scientists should be invited, and which research papers should be consulted, however, he soon found himself mired in just the personal controversies and animosities they wanted to avoid.[32] Subsequently, to rise above the politics of personality, Smith and Dr. Reckhow decided not to invite *any* researchers from North Carolina; neither Dr. Burkholder nor Dr. Paerl were invited, nor were any of their close collaborators. Instead, they gathered an impressive array of both government and university researchers from both the eastern U.S. coast and Canada. Those scientists most intimate with *Pfiesteria* research and closest to the public policy-making process in North Carolina, however, were necessarily excluded because of ties to Burkholder or Paerl. As a result, the Raleigh Report proved to have little impact on water quality policy in North Carolina. Thus, whereas avoiding personality conflict in North Carolina perhaps allowed for a scientific panel representing a broader national-international cross-section of the research community (and, hence, greater universality), doing so necessarily diminished the local expertise present at the consensus table (and, consequently, the local political relevance of the consensus).

Thus, in the two cases one sees an anti-correlation between the universality of the mandated scientific consensus and the political relevance of that consensus. In Maryland, ensuring the political relevance of the scientific consensus by including Dr. Burkholder came at the price of excluding scientific colleagues who would have broadened the universality of the consensus. In North Carolina, the attempt to avoid local politics resulted in a more universal consensus that was relatively politically irrelevant because the state scientific community had little stake in it. Did this tension between normal and mandated science undermine the objectivity of the scientific consensus? While that is difficult to assess, one can point to the fact that the Cambridge Consensus and the Raleigh Report reach virtually the same conclusion on the *Pfiesteria*-nutrients linkage as evidence in favor of the scientific community having filtered political geography out of its consensus in this case. Insofar as the scientific community was able to do so, it was likely due to its reliance in both processes not so much on the specific expertise of any one particular scientist regarding *Pfiesteria* but rather on the *prior* shared consensus regarding nutrients, eutrophication and algal blooms, which was developed independently of, and hence was largely immune to, the twinned politics of personalities and *Pfiesteria*.

4. Scientific Consensus and Public Policy

4.1. Science and policy—naïve 'realism'

One view of the relationship of science and policy (often considered to be naïve) may be called 'realism.' The term 'realism' here is taken from Jasanoff's terminology concerning the relationship between science and policy and refers to the view that

> truths about the natural world arise without meaningful human agency or intervention, in an autonomous domain of endeavor that is cleanly separated from the uses of political power. Facts, the results of scientific inquiry, are assumed in this standard account of science in public policy to be distinct from values, which are seen as the primary medium of exchange in the political realm. Values [distinct from the scientific community's "internal" normative commitments] are thought to play no significant role in the creation of scientific facts. Realists believe that productive discussion of norms and values stops at the point where public choices come to depend chiefly on experts' objective assessments of the facts. By extension it is the duty of expert policy advisers to bring facts to bear on the processes of political evaluation and judgment, and so to keep public actions from falling prey to passion and irrationality. (Jasanoff 1997, p. 229)

From such a 'realist' perspective, one would expect the policy-relevance of scientific consensus to correlate (more or less) with the "internal" normativity of the scientific consensus: the closer the scientific-consensus process conforms to the norms of scientific practice, the more 'objective' will be the scientific consensus reached and, hence, the greater will be the relevance of that consensus in any 'rational' public policy-making process.

Our study of this case has shown mixed results for the 'realist' position. In the case of North Carolina, the failures of the norms of disinterestedness, communalism and organized skepticism, due to the influence of politics on science, did significantly compromise the ability of scientific consensus to inform public policy, in accord with the 'realist' view. Concerning the river closure policy in Maryland, however, the initial short-term policy-relevance of scientific consensus came at the cost of the organized skepticism of the scientific community, although in the longer term further research did help refine public policy. And comparing the cases of Maryland and North Carolina, we find an anti-correlation between the universality of the scientific consensus and the policy-relevance of that consensus, in contrast to the 'realist' view. Thus, on the basis of this study, one would agree that 'realism' is at least somewhat naïve in its understanding of the relationship between science and policy and for sure inadequate to comprehend the science-policy interaction in the present case.

4.2. Science irrelevant to policy—radical 'skepticism'

Given the mismatch between science and politics as evident in this particular case, one might conclude that not only was science in this case actually irrelevant to policy but further that science generally is, in fact, and ought to be, irrelevant to policy. Such is the radically 'skeptical' view of Collingridge and Reeve (1986), who reject the rationalism of the 'realist' view. They make two central claims: first, that "relevance to policy, by itself, is sufficient to completely destroy the delicate mechanisms by which scientists normally ensure that their work leads to agreement" and, second, that "this failure of science to operate as smoothly as mythology would have it does not in effect matter for policymaking" (Collingridge and Reeve 1986, pp. ix-x). In their view, science can relate to the policy making process in either of two ways, but in both ways science not only fails to live up to its best ideals but also has little real effect on policy decisions. In the first, science is called in to settle a policy debate by reaching consensus on new evidence, but in attempting to do so becomes dysfunctionally mired in hyper-critical technical disputes that provide no guidance for policy makers who end up reaching decisions largely independently of the scientific debate. In the second, an existing 'under-critical' scientific consensus ends up merely confirming a prior policy consensus by failing to give equal weight to alternative hypotheses. They thus draw the conclusion that "the corrigibility of all scientific claims means that policy ought to be insensitive to any conjecture of science" (p. 29).

There is no doubt that the case of North Carolina provides ample evidence supporting their first claim that political relevance can indeed distort and disrupt the normal functioning of science to the extent of undercutting the ability of scientific consensus to objectively inform public policy. Is the present case also evidence in favor of their second claim that science is and ought to be irrelevant to policy? That depends on the inevitability of such irrelevance of science to policy. We will test their claim against the case of Maryland.

Regarding river closures, the early policy followed by state officials was motivated by a medically warranted concern over the potential human health risk from *Pfiesteria* toxicity and was keyed to an assumed linkage between toxic *Pfiesteria* activity and observable fish lesions. This latter assumption was supported by the early TAC consensus, which, despite noted uncertainties, gave scientific objectivity to a policy that was publicly controversial because of its impact on private economic and recreational activities. During the next two years, however, the *Pfiesteria*-lesion linkage came under considerable scrutiny within the scientific community, resulting in a fair amount of dispute among researchers concerning the etiology of the fish lesions that had been previously attributed to *Pfiesteria* as the primary cause. The TAC reconvened in late 1998 and in light of the new research significantly qualified its earlier conclusions: "This uncertainty does mean…that the prevalence of fish lesions alone should not be considered a reliable indicator of toxic *Pfiesteria* outbreaks" (Center for Environmental Science 1999). Consequently, the state's river closure policy came under both scientific and public criticism as the results of these further fish pathology studies were released. State officials, acknowledging that "since many factors may cause lesions in fish" subsequently revised its river closure policy by abandoning lesions as the primary and exclusive environmental indicator of recent toxic *Pfiesteria* activity in favor of multiple indicators, including fish behavior and visual identification of the organism in addition to lesions (Department of Natural Resources 1999). For sure, the *Pfiesteria*-lesion linkage question has generated hyper-critical technical disputes among scientists as in the first scenario depicted above.

Instead of this dispute hindering public policy or rendering science irrelevant to policy, however, it rather prompted a revision of public policy to bring it more in line with the limits of present knowledge; and this was so because the original policy was explicitly left open to "continuous evaluation" and revision "in light of improvements to our knowledge" (Maryland Department of Natural Rescources 1997). Now, the science in dispute was not the ultimate basis of the river closure policy, which was motivated primarily by political risk assessment and would likely have been made by state officials on the basis of the available medical evidence and standard public health precaution irrespective of the question of lesion etiology, the latter of which effectively mattered regarding only under what circumstances the policy should be invoked. So, given that this policy was justified based on public values more than scientific evidence to begin with, it is perhaps not the best issue on which to test the Collingridge and Reeve claim.

Looking on the face of things regarding water quality and nutrient control, one might be led to conclude that, in fact, science was irrelevant to policy in that area. The policy-making process on water quality, bolstered politically by an early (uncertainty-laden) scientific consensus in favor of the *Pfiesteria*-lesion linkage, moved quickly out ahead of the peer-review process on lesion etiology in the direction of stronger nutrient control measures, in favor of which there was already a substantial consensus among environmental advocates, scientists, and managers. A second expert scientific committee, practically restricted by the mandate of the policy-making process, drew up a scientific consensus that effectively validated the prior policy consensus on nutrient control and consciously framed the *Pfiesteria*-nutrients issue within this prior consensus. The policy-making process then ran with this conclusion on its way toward new regulations on agricultural land use. Moreover, the public policy finally enacted doesn't appear to be based upon *Pfiesteria* itself in that it reflects neither the extent of the original *Pfiesteria*-related event (a relatively minor fish kill that affected a small, remote Bay tributary), nor the science of *Pfiesteria* (which suggested that *Pfiesteria* thrives in only nutrient rich waters), nor even the actual public health risk associated with *Pfiesteria* (which is effectively limited to direct contact with those specific waters where recent toxic *Pfiesteria* activity is suspected). Instead, the WQIA mandates nutrient management planning on (nearly) every farm in every tributary basin regardless of the susceptibility to toxic *Pfiesteria* blooms (which appear restricted to shallow, slow moving brackish waters), likelihood of fish kills (which chiefly affect juvenile Atlantic menhaden), relative contribution of agricultural run-off to nutrient loads, and trends in nutrient levels (which were much higher on the Eastern Shore tributaries) in the respective rivers. The major features of the policy appear rather insensitive to *Pfiesteria* itself, reflecting rather a prior policy agenda on controlling non-point source nutrient pollution from agricultural activities and a political process that must seek equity by treating all farmers alike regardless of scientific reasons for a more "geographically-logical" policy. All of this would seem to fit the second scenario above, the 'under-critical' model.

While on the face of it the peculiar role played by *Pfiesteria* in the policy-making process in Maryland does indeed fit this scenario, it would be hasty to conclude that science was irrelevant to the WQIA. For the primary scientific basis for the policy was not so much *Pfiesteria* as the three decades of prior research on nutrient pollution, estuarine eutrophication and algal blooms. Now, the nutrient reduction agenda surely did constitute a prior policy consensus, but such consensus was motivated by that prior research independently of *Pfiesteria*; and that prior research consensus is hardly what one would consider 'under-critical.' Rather than the 'under-critical' model of Collingridge and Reeve, the role of *Pfiesteria* better fits what Dr. Michael Orbach, Director of the Duke Marine Biology Laboratory, refers to as the 'lever.' Sometimes in environmental advocacy, one issue is used as a 'lever' for another issue. In such cases "the lever can be scientifically weak, but it might have other factors that make it usable for advocacy purposes."[33] The human health, economic and cultural impacts of *Pfiesteria* were used by advocates for stronger nutrient control to leverage mandatory nutrient management planning even though the scientific linkages of *Pfiesteria* to lesions and fish health were significantly uncertain. So, science entered into the water quality policy-making process in Maryland at two levels—*Pfiesteria* as the short-term lever for a policy warranted by long-term nutrient research. There was science, then, that was relevant to the water quality policy-making process in an epistemologically defensible way that did not necessarily betray the ideals of science; and without that long-term research program, such water quality policy would likely not have been on the table in the first place.

From the perspective of Collingridge and Reeve, one would say that the case of the river closure policy fits the 'over-critical' scenario, while the case of the nutrient management regulations fits the 'under-critical' scenario. In both cases, one would infer, science was irrelevant to policy. Such an analysis, however, overlooks a crucial difference between the two cases. Using fish lesions as the environmental indicator for the river closure policy, on the one hand, was motivated by a quickly-formed scientific consensus that was based upon a relatively thin layer of primary literature supported by only limited laboratory and field observations that precipitated hypotheses that were unconfirmed by pathological studies and had not been replicated by other researchers. In terms of Bauer's 'knowledge filter', this consensus was based upon scientific information that had passed only the first filter; and that it should prove the subject of controversy and would have to be revised later is unsurprising. The nutrient management regulations, on the other hand, were motivated by repeatedly confirmed and extensively peer-reviewed water quality models derived from a broad range of observations that form the basis of a long-standing and widely-shared consensus that has been well-described in the secondary literature and is already a standard topic in textbooks on ecology, oceanography and estuarine science (cf., e.g., Kennish 1998). This consensus, by contrast with the first, was based upon scientific information that had passed through several filters in the scientific community prior to entering into the current policy-making process; and that it should prove rather uncontroversial is also hardly surprising. The crucial difference here is not that controversy or lack thereof is unsurprising, but that the scientific basis of the nutrient management regulations was far more epistemologically sound (in the scientific community's own terms) than was the basis of the river closure policy.

It would appear that Collingridge and Reeve's two-scenario dichotomy is exhaustive, and the irrelevance of science to policy is inevitable, *only if* the ostensible policy-relevant scientific consensus is to be based exclusively either on new research and primary literature or on the empirically unfounded prejudices of the research community, i.e., only if there is no *prior*, *well-confirmed* scientific consensus relevant to the policy question at hand, which, as shown here, need not be the case. One might conclude instead, then, that the relevance of science to policy is (at least in part) a function of the epistemological status of the relevant scientific information within the research community: an independent and 'well-filtered' or carefully-winnowed scientific consensus can have potential relevance to the outcome of a public policy-making process in a manner that need not grossly compromise the norms of science. Thus, while one should not be naïve to the less than epistemologically defensible ways in which science can be used in policy debates, one should hesitate to endorse the view that science cannot be relevant to policy and, hence, that policy ought to be insensitive to science.

4.3. The policy-relevance of science—'moral knowledge'

Although in this case it can be cogently argued against radical 'skepticism' that science can be relevant to policy in a manner that need not egregiously sacrifice the "internal" norms of science, it is also clear that the policy-relevance of science in this case cannot be understood simply on the basis of naïve 'realism.' Jasanoff has articulated a position on the relevance of science to policy that moves beyond both naïve 'realism' and radical 'skepticism.' She argues that "in order to influence public policy, science must achieve moral as well as epistemological authority—indeed, that the latter cannot be attained except in conjunction with the former" (Jasanoff 1997, p. 232).[34] In particular,

> scientific ideas may prove influential because they (1) converge with prevailing cultural ideas about responsibility and fault; (2) support politically accepted forms of discourse and reasoning; or (3) are ratified by communities that have established, within well-defined boundaries, a privileged right to formulate policy. (Jasanoff 1997, p. 232).

We will analyze the present case from two of the angles she suggests—viz., the convergence of knowledge claims with moral claims and the role of institutions in validating a joint moral-epistemological order.

Regarding a case of water pollution in the Great Lakes connected with discharges from iron ore mining, Jasanoff concluded that public agreement to cease such discharges was reached because "there was convergence between an epistemic order that gave credibility to claims of future health risks…and a moral order that validated, in the name of environmental stewardship, precautionary actions even in the absence of definite proof of harm" (Jasanoff 1997, p. 239). A similar convergence between knowledge claims and moral claims can be observed in case of the nutrient management regulations enacted in Maryland.

As mentioned in the case history above and recalled in the previous sub-section, environmental research in the Chesapeake Bay watershed during the 1960s-1990s established practically indisputable linkages between various sources of excess nutrients (nitrogen and phosphorus) being released into the Bay and its tributaries and various deleterious effects of such nutrient pollution upon water quality, fish health and ecosystem cycles in the estuary, research conclusions that gradually converged with the responsibilities of state environmental management and regulatory agencies for protecting natural resources. From the 1960s to the early 1970s, in parallel with the mandates of the federal Clean Water Act (1965), the initial focus was on point sources of phosphorus, especially urban industrial and municipal sewage outfalls. Following Tropical Storm Agnes in 1972, scientific awareness of nutrient pollution expanded to encompass the entire watershed, a perspective that recognized nutrient pollution as the primary pollution problem in the Bay and brought into view non-point source phosphorus and nitrogen pollution resulting from both sub-urban run-off linked to human land use (e.g., parking lots and golf courses) and rural run-off linked to agricultural activity (e.g., tillage practices). This new perspective was confirmed by research done under the auspices of the federally-mandated Environmental Protection Agency's Chesapeake Bay Study (1977-1983), which synthesized the emerging scientific consensus on nutrient pollution into a coherent picture. This coherent scientific picture formed the basis for the landmark multi-state, multi-agency Chesapeake Bay Agreement (1983), which led four years later to a basin-wide environmental management strategy to achieve a 40% reduction in nutrient inputs into the Bay, and the Maryland state ban on phosphates in detergents

(1985), which eventually spread across the nation. During the 1990s, the basin-wide perspective on non-point source nutrient pollution generated increasingly focused attention on agriculture, by far the largest land-use activity in the Bay watershed, with particular attention being paid to the previously unacknowledged role of fertilizer and manure application practices as major factors contributing to farmland nutrient run-off (especially phosphorus) as well as the empirically demonstrable inadequacy of existing standard agricultural land-management practices for controlling nutrient run-off.

The developing scientific understanding of nutrient pollution in the Bay proved useful and relevant for public policy to ameliorate the negative environmental effects of excess nutrients because it linked specific human practices and activities with observable and quantifiable ecological phenomena. Such linkages made possible discernment of human causes and harmful effects that allowed the identification of responsible parties and manageable activities, which is a necessary prerequisite for public bodies to take defensible legislative action and formulate enforceable management regulations. A publicly supported moral framework holding industry legally responsible for the harmful effects of pollution was already well established in the Chesapeake prior to the forging of the science-management consensus on nutrient pollution from a watershed-wide perspective (Capper *et al.* 1983). While urban industries had long been regarded in the eyes of public policy as ‘polluters’ responsible for the harmful effects of their activities upon public goods such as natural resources and human health, the new epistemological order substantiated by the recent environmental science research sanctioned an expansion of the existing public framework of responsibility and regulation such that the legal-moral category ‘polluter’ was extended to include municipalities, sub-urban land owners, consumers, and even farmers. It is this latter extension of responsibility, which has met with resistance by the agricultural community (cf. Paolisso and Maloney 2000), that is crucial for understanding the formation of the nutrient management regulations mandated by the Water Quality Improvement Act. The new scientific evidence regarding *Pfiesteria* itself did not suggest any new linkages between agricultural activity and water quality; rather, the *Pfiesteria* evidence (along with the recent agricultural research on phosphorus application practices) was evaluated against the background of the existing epistemic-moral framework, within which it seemed reasonable and defensible, despite acknowledged scientific uncertainties in the nutrients-*Pfiesteria*-fish health linkages, to assign (partial) responsibility to certain manageable agricultural activities for the nutrient-pollution-related water quality conditions that appeared to promote *Pfiesteria*. This framing of the *Pfiesteria* problem within the broader perspective of non-point source nutrient pollution is evident in the Final Report of the Citizens Pfiesteria Action Commission (Hughes 1997), which recommended the polices enacted into law through the Water Quality Improvement Act. After acknowledging multiple responsible parties for the nutrient pollution problem (p. 3) and discussing specific evidence for the *Pfiesteria*-nutrients linkage (pp. 9-13), the Commission report sets that evidence against the background of the comprehensive land-based nutrient pollution problem (pp. 15-17) before going on to address at length the various linkages between agricultural practices and nutrient pollution (pp. 19ff.). It was thus an already existing epistemic-moral order that (in Jasanoff's phrase) "repaired the uncertainty" of the nutrients-*Pfiesteria*-fish health linkages and legitimated the public policy warranted by those linkages.

This epistemic-moral order did not exist only intellectually, of course, but was embodied institutionally; and without that institutional backing for the epistemic-moral order, it is unlikely that the new scientific information linking water quality and agricultural activity to *Pfiesteria* would have won sufficient public assent to justify new public policy. Jasanoff writes,

> scientific knowledge needs a sustaining and supporting social order…in order to reassure skeptical publics and serve as a compelling basis for policy decisions…Science, in other words, has to be produced and interpreted within a pre-existing epistemic community—a community already committed to the joint production of epistemological and moral order—in order to have meaning for policy. (Jasanoff 1997, p. 247)

In the present case, one can observe the roles of various institutions in the scientific consensus and public policy-making processes, some of which sought to validate and extend the existing epistemic-moral order to agriculture and others of which sought to entrench against just such an extension.

In one sense, the story of the Water Quality Improvement Act is a story of four Maryland state institutions.[35] In the role of science were the Center for Environmental Science (UMCES) and the College of Agriculture and Natural Resources (CANR) of the University of Maryland; in the role of environmental management and regulation were the Maryland Departments of Natural Resources (DNR) and Agriculture (MDA). As the public debate over *Pfiesteria* shifted to a policy negotiation over agriculture and nutrients, these institutions lined up on different sides of the issue, with UMCES and DNR leading a "progressive" movement to introduce mandatory regulation of agriculture and CANR and MDA leading a "reactionary" response to mitigate the impact of new regulation on agriculture.

The Center for Environmental Science, especially through its oldest and leading component, the Chesapeake Biological Laboratory (CBL), has had a long history of involvement in public policy issues in the Bay and its President is ex officio a member of the Bay Cabinet, which advises the Governor on issues concerning the Bay. Some of the major public policy impacts of research by CBL over the years include management of oyster harvesting (1940s), environmentally-sensitive siting of power plants (1960s), and a temporary moratorium on commercial fishing of striped bass (1980s) (Mihursky 2000). By the time that nutrient pollution became a major public policy issue in the 1980s, UMCES was already a significant institutional component of the epistemic-moral order concerning the environmental protection and restoration of the Bay; and during the 1980-90s UMCES research contributed substantially to the development of the nutrient reduction agenda and its related framework of responsibility and regulation under the Bay Agreement (cf. Smith et al. 1992, Malone et al. 1993, and Boynton 1997). The authority of UMCES within that order was evident in the weight given by the Citizens Commission to the report of the Cambridge Forum on *Pfiesteria* and nutrients, which was convened at the request of Citizens Commission Chairman Hughes by UMCES President Dr. Donald Boesch (who named five UMCES scientists to the panel of nine participants) and the conclusions of which were adopted by the Commission (Hughes 1997, p. 9).

The authority of the Cambridge Consensus was reinforced by the construction of a plausible model by DNR's Dr. Robert Magnien to interpret and connect both the evidence concerning *Pfiesteria* in relation to the fish lesions and mortality on the Pocomoke and the evidence concerning nutrient pollution in relation to eutrophication and low dissolved oxygen levels, a model that was presented to both the Cambridge Forum and the Citizens Commission (included as Appendix 5 of Hughes 1997). During the 1990s, DNR played a major role in Maryland's Tributary Strategies, a tributary-specific approach to reduce nutrient pollution via non-point source management, with a special emphasis on agriculture, in accordance with the goals of the Bay Agreement. By 1997, DNR was convinced that the existing approach to reducing the agricultural contribution to nutrient loads via voluntary implementation of standard conservation methods was ineffective in meeting the nutrient reduction goal and, hence, was beginning to favor a mandatory regulatory approach to non-point source nutrient pollution from agricultural activity.[36]

Such a shift to mandatory regulation had long been resisted by MDA, which shared responsibility for oversight and implementation of the Tributary Strategies. According to Dr. Tom Simpson, Coordinator of the Chesapeake Bay Agricultural Programs with MDA and CANR Cooperative Extension Office, MDA's resistance to mandatory agricultural management regulations reflected its self-perceived role as an advocate for agriculture, promoting economic development of agriculture rather than imposing mandatory programs on farmers; indeed, in persuading the agricultural community to cooperate with the Tributary Strategies, MDA entered into a tacit agreement with farmers that cooperation would protect them from mandatory nutrient management.[37] MDA was backed up in its entrenched position by the research of CANR. According to Dr. Russell Brinsfield, CANR Professor and Director of the University of Maryland's Wye Research and Education Center, CANR continued to support the existing methods of nutrient run-off control and fertilizer/manure application for several years after clear and convincing scientific evidence emerged that existing methods of conservation tillage were demonstrably ineffective in controlling (and might even increase) nutrient run-off given the conventional practices of fertilizer/manure application.[38]

The joint authority of MDA-CANR within the epistemic-moral order pertaining to nutrient pollution and Bay restoration was checked—and correlatively their institutional ability to shield agriculture from full inclusion within the framework of responsibility and regulation sanctioned by that epistemic-moral order was diminished—when the scientific evidence undermining the existing agricultural management practices certified by MDA-CANR became known publicly during the hearings of the Citizens Commission. Reluctantly, CANR produced a report for the Commission acknowledging the inadequacy of the received wisdom on agricultural practices and nutrient control (College of Agriculture and Natural Resources 1997, included as Appendix 10 of Hughes 1997), an epistemic admission that effectively conceded the moral position to those advocating mandatory nutrient management regulations for agriculture.

The moral authority of the Water Quality Improvement Act to command public assent and the relevance of scientific information to that moral authority was thus (in part) a function of the shifting weights of public institutional authority within the existing epistemic-moral order, a shift occasioned by the *Pfiesteria* episode. Although both UMCES-DNR and MDA-CANR carried public institutional authority prior to the *Pfiesteria* episode, the converging scientific evidence from both environmental and agricultural research that was brought to bear on the *Pfiesteria*-nutrients linkage significantly qualified the epistemic basis of the moral authority of MDA-CANR while enhancing that of UMCES-DNR. So, UMCES-DNR were in a privileged moral position before the Citizens Commission to interpret the scientific information concerning *Pfiesteria*, nutrients, and agriculture; consequently, the Commission took the import of the scientific evidence, considered within the existing framework of responsibility and regulation pertaining to nutrient pollution and Bay restoration, to favor mandatory nutrient management regulations for agricultural practice.

5. Conclusion

Two broad conclusions may be drawn from this case study regarding the relationship between scientific consensus and public policy.

First, this case illustrates the continuing relevance of socially shared and institutionally sanctioned norms of scientific practice—e.g., (1) the openness of data and sharing of materials related to the conduct and reporting of research and (2) the independence of peer review—for understanding (a) how actual scientists view their own scientific activity, (b) how they interpret and evaluate the scientific activities of their colleagues, and (c) how scientific research interacts with the making of public policy. Although this case shows that compromising or making exceptions to such norms does not necessarily (in the short term, at least) undermine a responsible relationship between science and policy or prohibit the formation of scientific consensus relevant to public policy, it is apparent from this case that sacrificing accepted norms of scientific practice (whether to political values or personal criteria) without compelling justification convincing to scientific colleagues does weaken significantly the fragile practices that maintain constructive and cooperative relationships among researchers within the scientific community, which could in the longer term undermine the capacity of science to inform public policy in a relatively objective and reliable manner.

Second, this case shows further that, contrary to what some have argued, the relationship between scientific consensus and public policy need not be dysfunctional for either science or policy. At the same time, it is apparent from this case that there is no necessary linkage between the “internal” normativity of scientific consensus and its relevance to public policy: justifying public policy in the political process requires more than simple appeal to the relative objectivity of the relevant consensus and the integrity of the consensus process on science’s own terms. It is here that this case demonstrates both the need for scientific consensus to carry a certain moral authority (in addition to epistemic authority) in order to be relevant to public policy and the indispensable role that research and regulatory institutions play in constituting and contesting the public moral authority of scientific knowledge.

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[1] Research for this paper was funded by Maryland Sea Grant and was conducted in the summers of 1998 and 2000 while in residence at Chesapeake Biological Laboratory (University of Maryland Center for Environmental Science) in Solomons, MD. A more extensive and detailed discussion of the science, policy and social issues involved in this case can be found in my report, "Harmful Algal Blooms of *Pfiesteria* in Maryland and North Carolina: A Study in Science, Policy and Values," available from Maryland Sea Grant. Special thanks to Dr. Ken Tenore, Director of Chesapeake Biological Laboratory, for his assistance and counsel on this project. Thanks to the nearly three dozen scientists, managers and advocates who participated in this study via personal interviews. Thanks also to Dr. Donald Boesch, President of UMCES, for reading the final Sea Grant report and making several critical comments.
[2] Regarding such indirect effects of nutrient pollution and algal blooms, cf. Harding *et al.* (1992) and Paerl *et al.* (1998)
[3] To put *Pfiesteria* in perspective with other harmful algae, see Mlot (1997) and Tibbetts (1998).
[4] This brief review follows Cronin (1982) and Malone *et al.* (1993).
[5] For the perspective of environmental advocates, see Horton and Eichbaum (1991). For the perspective of environmental science, see Boynton (1997). For the perspective of environmental management, see Shuyler (1993) and Favero (1997). For an overview and synthesis of these perspectives, see Boesch *et al.* (2001).
[6] For this advancing edge in agricultural research, see Sharpley (1998).

[7] For the conventional agricultural perspective, see Brodie and Powell (1995). For such new scientific evidence, see Staver and Brinsfield (1995). For an exception to the conventional perspective, see Simpson (1991)

[8] For a technical review of Dr. Burkholder's research on *Pfiesteria*, see Burkholder and Glasgow (1997); for a non-technical overview, see Burkholder (1999). For a comprehensive look at the current state of knowledge and outstanding questions concerning *Pfiesteria*, consult National Institute of Environmental Health Sciences (2001).

[9] Personal interview with Dr. Paerl, 19 July 2000.

[10] The following account of the funding dispute is derived from newspaper investigations in the *Raleigh News & Observer* and the *New Bern Sun Journal*. Cf. Leavenworth (1996a, b) and Jones (1996 a, b, c).

[11] "Two views of toxic threat in rivers," *Raleigh News & Observer*, 15 July 1998.

[12] For many contemporary sociologists of science, Merton's "CUDOS" norms are yesterday's news at best, poor sociology at worst, and in any case representative of a naïve view of science as a "pure" activity. Merton, to the contrary, was anything but naïve about the actual functioning of science within the social order (cf. Merton 1938); in fact, that science had proved quite susceptible to social pressure and was being judged by "external" utilitarian criteria in his contemporary situation (especially under both fascism and communism) is precisely what occasioned Merton's normative analysis of scientific practice in the first place. Indeed, this case, I think, shows the continuing relevance of such norms to contemporary scientific practice.

[13] Whether or not scientists *should* always follow such norms is a legitimate question, one aspect of which we will consider below.

[14] Clearly, Merton himself was influenced by a 'positivist' epistemology that viewed science as generally progressing inductively toward the goal of complete knowledge. Such an epistemology is surely an inadequate basis for understanding actual scientific practice. Nonetheless, I would contend, the CUDOS norms have a relevance to actual scientific practice independent of positivist epistemology, as explained below.

[15] Although Bauer himself would want to emphasize the workings of the knowledge filter over any particular set of epistemic norms as the key to science achieving relatively reliable and objective consensus, one could say that Merton's norms define the optimal social conditions within which the knowledge filter works.

[16] Personal interview with Dr. Ramus, 14 July 2000.

[17] Personal interview with Dr. Burkholder, 18 July 2000.

[18] Personal interview, 18 July 2000.

[19] Personal interview, 18 July 2000.

[20] Relevant here was sensitivity to both the local significance of the Chesapeake Bay in Maryland's self-image and public discourse as well as the national sensation generated by the *Pfiesteria* episode in North Carolina through the early 1997 publication of Barker's *And the Waters Turned to Blood*. The former was emphasized by Maryland State Secretary of Natural Resources John R. Griffin in testimony before the US House of Representatives Subcommittee on Fisheries Conservation, Wildlife and Oceans, 9 October 1997, regarding the *Pfiesteria* episode in Maryland; the latter was emphasized by Dr. Rob Magnien, MDNR's principal scientist advising the scientific consensus process, during a personal interview, 10 July 2000.

[21] Relevant here was the impact of the *Pfiesteria* episode on seafood sales in Maryland during the fall of 1997. Cf. Meyer (1997) and Lipton (1998).

[22] Data compiled by Dr. Donald Boesch and Alexis Henderson of UMCES show a strong correlation between newspaper coverage of *Pfiesteria* by the Baltimore Sun and the Washington Post and state public policy response to *Pfiesteria* in the summer and fall of 1997. These data were presented by Tim Wheeler of the Baltimore Sun at the Ecological Society of America symposium, "*Pfiesteria* in Maryland," in Baltimore, 3 August 1998.

[23] Relevant here was serious public misunderstanding regarding the public health risks associated with *Pfiesteria*, especially a completely unfounded fear of *Pfiesteria* toxin exposure via consumption of seafood. Cf. Falk (1999).

[24] The scientific uncertainty of the initial Burkholder hypothesis regarding the *Pfiesteria*-lesion linkage, including even whether or not *Pfiesteria* affects fish by producing toxins, has only

continued to be magnified by further pathological studies and controlled laboratory experiments. Cf. Law (2001), Vogelbein et al. (2001), Berry et al. (2002) and Vogelbein et al. (2002).

[25] In a letter to the author (18 August 2002), Dr. Donald Boesch contends that the primary scientific factor driving both the scientific consensus and public policy processes on nutrients and land use in the fall of 1997 was human health and that the lesion/fish health issue was therefore irrelevant by this time. Thus, in his view, the above analysis misses the point from the scientific standpoint by focusing on the lesion/fish health question. I find this argument to be unconvincing for the following reason. Increased nutrient control was simply unnecessary for protecting the public from any potential *Pfiesteria* human health risk, at least in the short term. Because any potential public risk to human health from *Pfiesteria* is limited to only direct exposure to toxic *Pfiesteria* during an ongoing fish kill (a matter on which there already existed a consensus within the scientific research community at this time even though there remained considerable uncertainty regarding how serious a risk such open exposure posed to individuals), restricting public access to waters where a fish kill is in progress or has recently occurred and warning the public to avoid *all* fish kills, whether *Pfiesteria*-related or not (a common sense precaution in any case), is adequate as an interim measure in satisfying public health concerns. Hence, a strong river closure policy by itself would have proved sufficient to protect public health while the organized skepticism of the scientific research community did its work on the question of lesion etiology. While I readily grant that public health was a major political motivation in the public policy process (due in part, I would add, to serious public misunderstanding about the human health risks associated with *Pfiesteria*), and that public health provided a relatively objective basis for the river closure policy, I maintain that from a scientific standpoint it would appear that, although in the long term public health is served by improving water quality for a number of reasons unrelated to *Pfiesteria*, the *Pfiesteria*-human health linkage was substantially irrelevant to the new agricultural land use regulations, which were objectively justified on the prior evidence cited above. *Pfiesteria*, with its attendant potential public health risk (as well as economic and cultural factors), created a favorable political milieu in which advocates for stronger nutrient control were able to seize the moment, an opportunity that may well have passed by the time the organized skepticism of the scientific community had done its work.

[26] The question here concerns whether scientists should employ what may be called an 'epistemologically conservative' or an 'ethically conservative' risk assessment strategy, i.e., whether Type-I or Type-II error should be minimized in cases of statistical uncertainty. Cf. Shrader-Frechette (1996).

[27] In researching this case, I encountered claims of gender bias regarding the way in which Dr. Burkholder and her research have been treated by the scientific community. Gender bias is, for sure, inconsistent with the norm of universalism. While I cannot say that no instances of gender bias have occurred in relation to *Pfiesteria* research, there seems to be no basis for a charge of gender bias regarding the consensus committees in particular. Out of the eighteen members of the TAC, five were women. Three out of nine people on the Cambridge Forum were women. And four of the fourteen comprising the Raleigh panel were women.

[28] Personal interview with Dr. Burkholder, 18 July 2000.

[29] Personal interview with Dr. Boesch, 14 June 2000, and letter from Dr. Boesch to the author, 18 August 2002.

[30] Personal interview with Dr. Malone, 20 August 1998.

[31] Personal interview with Marion Smith, 19 July 2000.

[32] Personal interview with Dr. Reckhow, 24 July 2000.

[33] Personal interview with Dr. Orbach, 14 July 2000.

[34] My thanks to Dr. Donald Boesch for bringing Jasanoff's work to my attention. Cf. Boesch (1999).

[35] In the following analysis, we will focus on the roles of public state institutions, ignoring the roles of both private state institutions such as the Chesapeake Bay Foundation, federal institutions such as the Environmental Protection Agency, and multi-state, multi-agency organizations such as the Chesapeake Bay Program. Concerning the roles of these institutions in generating the epistemic-moral order, refer to my Maryland Sea Grant report, pp. 10-12, 16-17, 25-26.

[36] Personal interview with Dr. Rob Magnien, 10 July 2000.

[37] Personal interview with Dr. Tom Simpson, 2 August 2000.

[38] Personal interview with Dr. Russell Brinsfield, 27 June 2000.