Faith-based Fisheries

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The scientific community gave a collective sigh of relief just before Christmas 2005 when Judge John E. Jones III ruled that intelligent design is not a scientific theory and cannot be taught alongside evolution as an alternative scientific hypothesis. There is no better way to unite any group of fractious scientists than to bring up creationism and intelligent design as alternative scientific hypotheses—scientists know that these faith-based ideas are not scientific and have no place in a scientific course. The court's ruling is a triumph for the scientific method of hypotheses being confronted by data and a setback for those with a political agenda masquerading as science.

However, before we congratulate ourselves too much for the triumph of the scientific method over belief, I suggest the fisheries community needs to look at itself and question whether there is not a within our own field a strong movement of faith-based acceptance of ideas, and a search for data that support these ideas, rather than critical and skeptical analysis of the evidence.

This faith-based fisheries movement has emerged in the last decade, and it threatens the very heart of the scientific process—peer review and publication in the top journals. Two journals with the highest profile, *Science* and *Nature*, clearly publish articles on fisheries not for their scientific merit, but for their publicity value. Beginning in at least 1993 with an article I co-authored (Ludwig et al. 1993), *Science* and *Nature* have published a long string of papers on the decline and collapse of fisheries that have attracted considerable public attention, and occasionally gaining coverage in the *New York Times* and the *Washington Post*. I assert that the peer review process has now totally failed and many of these papers are being published only because the editors and selected reviewers believe in the message, or because of their potential newsworthiness.

As examples, let me choose papers by well-established professionals who have long records of significant work beyond the papers discussed below and I emphasize the problem is with the peer review and editorial system, not the authors of the papers. Casey and Myers' paper on barndoor skate (Casey and Myers 1998) argued that these skates were headed towards extinction. Analysis by others more familiar with the data showed that the survey data came from areas that are not the core of the range of the species (Kulka et al. 2002), and subsequent evaluation of the status of barndoor skate in New England by the National Marine Fisheries Service concluded they were not overfished (Boelke et al. 2005), hardly headed towards extinction. A review by the Canadian Department of Fisheries and Oceans removed barndoor skates from a list of species that are threatened or endangered. The original paper has not been withdrawn and continues to frequently cited as an example of near extinction of marine species.

Myers and Worm published a paper in *Nature* (Myers and Worm 2003) which made the front page of major national newspapers, purporting to show that large pelagic fish stocks around the world had declined rapidly and by the 1980s were at less than 10% of their historic abundance. Widely cited in the scientific and popular literature, this paper raised a furor among many scientists specializing in pelagic fisheries who knew the same data, knew it was being misinterpreted, and knew there was a large body of other data that contradicted Myers and Worm's results. At least three independent critiques of the paper subsequently have been published: Walters (2003), Hampton et al. (2005), and Polacheck (2006). The critics are not the "old guard defending their turf," because it is not as if no one had noticed that the catch-per-unit effort data Myers and Worm used had declined. Rather these critics have themselves long been arguing that some of these fisheries are now depleted and overfished. What they criticized was Myers and Worm's analysis, their highly selective use of data, and specific conclusions about the extent and timing of depletion of these stocks, not their concern about overexploitation.

Conover and Munch (2002) published a highly cited paper in *Science* showing experimental evidence that size-selective fishing could induce growth changes in fish stocks and suggested this was a mechanism that could lead to collapse of fish stocks. The article never looked at actual fisheries data to ask if the laboratory selection regime imposed resembled what happens in fisheries, nor did they look at the vast body of fisheries data which shows that fish more commonly grow faster, not slower, when fishing pressure is high.

A paper in *Science* (Roberts et al. 2001) purported to show an example of how a marine protected area (MPA) increased yields outside the protected area, when in fact the abundance of fish outside the protected area increased within one year of the establishment of the MPA. Any competent peer reviewer would have seen the flaw in this logic—the theory of MPA impacts on adjoining areas requires at least a generation for abundance to build inside reserves and recruitment to spill out (Hilborn 2002). The displacement of fishing effort from inside to outside the reserve should initially cause abundance outside to decrease, so the increasing abundance outside the reserve after MPA establishment must have been due to an uncontrolled effect.

These four examples illustrate a failure of the peer review system and lack of the basic skepticism needed in science, and are unfortunately but a few of the many papers now appearing with similar sensational but unsubstantiated headlines. The people who knew the data used in the Casey and Myers paper and the Myers and Worm paper clearly were not involved in the review process, or the editors chose to ignore their opinions. The complete absence of skepticism by the reviewers of

the Roberts paper is a concern. The Conover and Munch paper demonstrated that growth is a heritable trait, but failed to demonstrate anything about how commercial fisheries operate. It did pose a testable hypothesis, but the paper did not include the real fisheries data to see if there was support for the hypothesis.

A community of belief has arisen whose credo has become "fisheries management has failed, we need to abandon the old approaches and use marine protected areas and ecosystem-based management." I fear that this belief has shaded the peer review process so badly that almost any paper showing a significant decline in fish abundance or benefits of marine protected areas has a high probability of getting favorable reviews in some journals regardless of the quality of the analysis. Critical peer review has been replaced by faith-based support for ideas and too many scientists have become advocates. An advocate knows the answer and looks for evidence to support it; a scientist asks nature how much support there is for competing hypotheses.

Much of the problem lies in the kind of journals *Science* and *Nature* have become: commercial enterprises covering a broad range of scientific issues. In a spoof of a *Science* article published on the *New York Times* web site, one of the fictitious authors is quoted as saying "journal editors favor bold claims, because these attract press attention and help recruit further bold papers, which in turn is a tonic for circulation and advertising" (www.nytimes.com/2006/01/17/science/17frau.html?ex=1295154000&en=9ca2921bc88fe0e3&ei=5090&partner=rssuserland&emc=rss). Given the high prestige of *Science* and *Nature* and the impact publication in these journals has on promotion and grants, one cannot blame authors for making bold claims. Perhaps those of us in fisheries should simply not give articles in these journals the prestige they now enjoy. Because of their general coverage, *Science* and *Nature* must have problems identifying appropriate reviewers for an individual paper. While there is no easy solution to this, a good step would be for journals to publish the names of reviewers who recommend publication. That would at least make it clear if these journals are relying on a small group of like-minded reviewers who have little expertise relevant to technical papers. Finally, the fact that discredited papers continue to be widely cited is aggravated by the fact the rebuttals frequently are not published by the original journal and may appear in gray literature or technical journals. The high-profile journals need to be especially sensitive to critiques of articles they have published and to formally withdraw discredited papers.

Although the scientific community was unanimous in its condemnation of faith-based teachings in evolution, we need to also reject agenda-driven, faith-based publication in fisheries and revive the peer review and publication process within our own community. Let's go back to testable hypotheses and evidence, and make sure that the peer reviewers know the data and the problem, and are not chosen because of their faith.

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