
Title: Doubt and certainty in fishery science: Are we really headed for a global collapse of stocks?

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Viewpoint

# Doubt and certainty in fishery science: are we really headed for a global collapse of stocks?

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**Abstract.**

A recent article discusses the consequences for fisheries of the relationship between taxonomic diversity and the resistance of marine ecosystems to disturbance; this study is based on an examination of historical catch and abundance data, together with experimental studies and surveys. It leads the authors to suggest that all sea fisheries could collapse by the middle of this century unless action is taken to prevent this from happening: this extrapolation has been taken seriously by the news media and the general public, though not by fisheries scientists. The authors draw what appear to be inappropriate conclusions from experimental studies, and from public data bases of global catch and taxonomic diversity. Nevertheless, the doubtful conclusions reached by the authors are less important to the proper functioning of fisheries science than is the apparent failure of the peer review process in the influential journal in which the results were published.

*Keywords: overfishing; peer review; biodiversity.*

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There is a common perception among the public that fisheries in many seas have collapsed because of over-fishing, and it is generally supposed that the situation is not improving: dissent comes mostly from those sectors of the fishing industry that wish to avoid additional catch restrictions.

All recent studies of the problem concur that over-fishing has occurred and is still occurring: Garcia and Grainger (1997) suggested that 35% of global stocks were already 'senescent' or over-fished, while Mullon *et al.* (2005) showed that about 25% of 1519 stocks, selected to represent global fisheries, had collapsed during the period 1950-2000 when the rate of collapse appeared to be relatively constant over time. Neither study suggested that the problem of over-fishing could be terminal if extrapolated into the future.

But a recent article in 'Science' by Worm *et al*. (2006) was more alarmist: their analysis of a spatially interpolated version of the FAO archives led these authors to conclude that the rate of stock collapses had accelerated throughout the period 1950-2004. They extrapolate this trend in such a way that many readers understood that all exploited fish and invertebrate stocks could collapse before mid-21$^{st}$ century if remedial action was not taken: even more extreme interpretations of the text were widely reported in the news media.

These reports were received without surprise by the general public: a subsequent web-search for the phrase "*global fishery collapse*" yielded >200 hits, almost all of which accepted the predicted global collapse of fish stocks. The very few contrary statements were made by commercial fishermen and fishery scientists.

More formally, critical comments concerning the article have been offered to the Science editors by several fishery scientists: one of these suggested that the 'collapse' data had been obtained by Worm et al. from FAO archives using unreliable assumptions. But, at the time of writing, in January 2007, no critical comment has yet been published by Science; the only response that I have seen from the editorial staff concern my own (rejected) comments on this apparent failure of peer review, and are meaningless: *We are aware that fisheries science and ocean ecology are contentious subjects, and that areas of legitimate disagreement exist*" (Andrew Sugden, in litt.).

My reading of the paper reveals many errors, principally due to an emphasis on the beneficial consequences of 'biodiversity' for the stability, productivity and potential recovery of marine ecosystems: a central argument of the study is that the percentage of collapsed stocks is inversely related to regional biodiversity. This argument is based on experimental data from littoral ecosystems, and is then extended to the coastal ocean

Using metanalysis of experimental results obtained in littoral ecosystems, and published in 16 science journals from 1960 to 1995, the authors conclude that we may assume that taxonomic diversity enhances ecosystem stability, where this is defined as "*the ability to withstand recurrent perturbations*". The 20 papers used to support this conclusion report laboratory experiments or field manipulations of littoral species: the field data were dominated by manipulations of estuarine seagrass (*Zostera*) beds.

To extrapolate from these to the response of coastal ocean ecosystems is a big step, that appears to ignore the fundamental differences between the physical control of oceanic and littoral productivity: in any case, only one of the 12 coastal seas discussed, the northern Adriatic, is not merely a shallow embayment: the Wadden Sea, outer Bay of Fundy, Chesapeake Bay, San Francisco Bay and so on. These cannot represent the 'coastal ocean', as is suggested by one of their figures. It is also extremely difficult for the reader to evaluate the origin of the very large body of data used in the diagrammatic representations of imputed changes in each of the 12 coastal ecosystems: for these, the reader is directed to the

Supplementary Information of a companion paper by Lotze et al (2006) where one finds that to review the data on the percentage of collapsed taxa over the last millennium in , for instance, the Western Baltic ecosystem one is directed to an unpaginated list of 60 books and articles: this is a formidable task which few will undertake.

The final extrapolation of the initial assumption drawn from the experimental data is to the open ocean: for this, the Large Marine Ecosystem partition of the coastal ocean is used as a template. The authors incorrectly assume that individual LMEs are "*characterised by distinct bathymetry, hydrography, productivity and food webs*" as suggested long ago by Ken Sherman. For this reason, they assume that data from individual LMEs may be compared in the same way as data from standard experimental plots.

Unfortunately, the authors ignore the fact that LMEs vary greatly both in area and in relative proportions of shelf and deep ocean. Thus, the Humbolt Current and the West Central Australian LMEs both typify the western coast of a continent, but the former extends over 46$^{o}$ of latitude, encompassing three biogeographic provinces, while the latter extends over only 10$^{o}$ and comprises a single biogeographic entity. Neither extends seawards far beyond the shelf, but the LMEs of the Arabian Sea and the Bay of Bengal are oceanic, having only about 15% of their area at shelf depths. The Arabian Sea province includes two functionally different upwelling ecosystems on, respectively, the Somali and Indian coastlines (Longhurst, 2006).

Neither LMEs, nor any other partition of the oceans at similar scale such as biogeochemical provinces, may properly be used as standard units in comparative analysis as if standard plots were being compared in experimental manipulations of terrestrial vegetation. In fact, I would go so far as to suggest that inter-regional comparisons of the kind attempted by Worm et al. are not yet feasible at the global scale.

Then, the data concerning taxonomic diversity of fish (from www.fishbase.org of the UBC Fisheries Center) that were used for this analysis suggest diversities that are greatly in excess of the numbers of species relevant to fisheries ecology: 725 species are listed for the Gulf of Guinea LME although only about 200 fish species occur in standard taxonomic works describing the continental shelf fauna of this region, while only 17 species comprised >0.1% of catches in the near-pristine trawl fishery off Nigeria (Longhurst, 1964). The discrepancy appears to be caused by the inclusion in FishBase of the myriad meso- and bathypelagic myctophids and other species that live at considerable depths in the open ocean and are not relevant to fisheries ecology.

For such reasons, it is not possible to support the central conclusions of Worm et al. concerning the '*relative stability and productivity*' of regions with high taxonomic diversity which is due (they claim, using a term from economics) to the portfolio effect '*whereby a more diverse array of species provides a larger number of ecological functions and economic opportunities, leading to a more stable trajectory and a better performance over time*'. The authors do not cite previously-published analyses that derive a different causative mechanism: that the resistance of tropical species to fishing is a direct consequence of the between-year invariance of their annual recruitment, itself a consequence of relatively invariant physical forcing of their habitat. This is, for example, likely the mechanism by which tropical stocks of yellowfin and skipjack tuna remain remarkably resilient to fishing pressure and has been proposed as the a generic explanation of stock stability in warm seas compared with colder seas having strong inter-annual variance in the conditions encountered by newly-hatched larvae (e.g. Longhurst, 2002).

Although the authors do recognize the "*inherent problem of inferring causation from correlation in larger-scale studies*" they appear to make no effort to avoid this trap. They make extensive use of simple correlations and, without discussing any alternative explanations or of mechanisms for their interpretation, they assume causality in each case. The possibility that a third variable might explain the observed relationship is ignored.

Oceanic ecology is a subject in which one can achieve confidence in results only with very great difficulty, because so many of the standard methods of terrestrial ecology are inapplicable. This suggests that, in this environment, ecologists should be especially wary of correlations, no matter how statistically satisfactory; none should be treated with respect until or unless a mechanism is proposed to account for the relationship observed. 'Proof' is a concept foreign to oceanic ecology: the best that can be achieved is consensus, and we must expect that this will change as we evolve our as-yet very inadequate knowledge of the mechanisms that lie behind what we can observe increasingly acutely.

A case in point appears to be their central proposition that taxonomic diversity enhances ecosystem stability; this appears to be at odds with modern theory of ecological diversity in the ocean as discussed, for instance, by Ramón Margalef (1997). It also appears to ignore our common understanding of the evolution of pelagic ecosystems from juvenile, low diversity to mature, high-diversity systems. We usually assume that ecosystems of polar seas and upwelling regions are constrained to remain in the juvenile state by the strong seasonal "*recurrent perturbations*" that they experience, while the mature ecosystems of tropical seas are permitted to evolve their characteristic high diversity only because of the relatively low level of perturbation that they experience. We have come to associate ecological equilibrium in the ocean with oceanographic stability and ecological disequilibrium with strong seasonal changes in ocean conditions. It will take more than the analysis of Worm et al. to shake my confidence in this fundamental tenet of biological oceanography.

Then, this paper inadvertently raises another important issue: to what extent are the FAO data archives of fishery landings competent to answer questions of the kind addressed in this article? They are, of course, the only tool to hand even though we all know that catches are a very poor proxy for stock biomass in the ocean. The archive is available in two formats: (i) from FAO, as catches in each statistical area as reported by each nation, and (ii) from the University of British Columbia, as catch data that have been extrapolated to values representing global half-degree latitude and longitude spatial cells for all reported taxa (Watson et al., 2004). It was these extrapolations that were used by Worm et al. but, in what follows, the basic FAO data archive is used for greater certainty of input.

Mullon et al. (*op. cit.*) pointed out some of the precautions that must be taken when interpreting either version of the FAO archive. We must exclude: (i) catches by nations with centrally-managed economies whose catch rates reflect economic and political decisions, (ii) catches from distant-water fleets subject to UNLOSC conditions after 1973, (iii) catches not attributed to Linnean species, (iv) catch data with significant missing sets of years, and (v) catches from very small fisheries that are probably improperly reported. Worm et al. took only the first and last of these precautions. Similarly, it is also very difficult estimate what are the consequences for the veracity of the FAO data of illegal, unreported and unregulated (IUU) catches: what is landed and what is reported represents an unknown fraction of the true fishing mortality suffered by each stock.

It is also necessary to approach the FAO archive remembering that changes in stock biomass are forced not only by changes in catch rates but also by changes in both the physical environment and in population size of other species in the same region, whether these are natural or fishery-induced. Unfortunately, the natural responses of stock biomass to environmental changes are usually ignored in studies of the evolution of global catches.

Despite all this, what is surprising is that a very simple examination of the FAO archive appears to reveal patterns in the evolution of global catches that differ very significantly from what is suggested by the studies of the progressive collapse of species-specific fisheries discussed above. A general increase in global catches was sustained from the beginning of record-keeping until about 1990, when they reached a plateau of around 67-75 x $10^6$ tonnes. The change of slope at this time was almost entirely due to the notorious fishing-induced collapse of the gadoids stocks of the NW Atlantic region and some largely natural changes in the dynamics of pelagic stocks of the southeast Pacific. In all other regions, either the general increase has been maintained, or else there has been a more complex pattern of decrease and recovery which requires examination of regional cause and effect before it can be interpreted correctly.

What is most surprising, if one were to subscribe to the general opinion concerning collapsing fisheries, is that the FAO archive appears (and I use the word carefully) to contradict some of our deeply-held preconceptions. Consider the case of the SW Pacific tropical shelves, where we are very familiar with the reportedly collapsed state of the trawl fisheries of the Gulf of Thailand; in fact, this region '*has been used often as a model case for demonstration of how fisheries resources are depleted through human interaction*' (Christensen, 1998).

We would naturally assume that the other, similar trawling grounds in that region would by now have suffered a similar fate. But the FAO archives suggest that the catches of coastal demersal fish in the southwest Pacific (Philippines to Malaysia) continue to sustain the steady annual increase in catches that was initiated at the start of the FAO series. Moreover, the relative partition of the total among 19 taxonomic categories at family and generic level has remained constant throughout the period.

The same is observed in the Gulf of Guinea, with which I am personally very familiar: what is especially remarkable here is how reported increases in total catches have been sustained in all coastal states from Guinea-Bissau to Cameroun since the 1950s; overall, this increase is largely due to increases in the weight of clupeids landed, especially of the diatom-feeding 'bonga', *Ethmalosa fimbricata*, but it is also evident in other groups, as in the catches from trawlers working on the inner continental shelf.

(Insert Fig 1 near here)

It is surprising how closely the species composition of the recent catches resembles that of the same community that I observed in scientific trawl surveys performed off Nigeria during the early 1960s (Longhurst, 1964), at a time when some concern was already expressed for the falling CPUE on the grounds within easy access of Lagos (Bayagbona, 1965). Not only are all the major species still present, but these take approximately the same relative proportions as in 1961:

| | Resource surveys off Nigeria 1961 | FAO: 1999-2004 |
|---|---|---|
| Croakers (*Pseudotolithus*) | 25.6% | 20.6% |
| Threadfins (*Galeoides, Pentanemus*) | 10.7 | 7.5 |
| Catfish (*Arius*) | 13.0 | 14.8 |
| Bigeye (*Brachydeuterus*) | 8.9 | 9.7 |
| Soles (*Cynoglossus*) | 3.8 | 4.9 |
| Grunters (*Pomadasys*) | 1.9 | 0.5 |
| Spadefish (*Drepane*) | 2.0 | 1.2 |

It is even more remarkable that the shallow shelf community of the Gulf of Guinea should so completely have recovered its pristine state after the population explosion of tilefish (*Balistes forcipatus*) that dominated this community on at least the central part of this coast 1970-1990, the effects of which are visible in the data series. Finally, it is also surprising that some of the larger and apparently more vulnerable species, such as the giant threadfin *Polydactylus quadrifilis* should still figure significantly in the landings from this community.

By discussing these examples, I do not mean to suggest that all fisheries in warm seas are in good shape. In fact, off Senegal, at the southern end of the Mauretanian upwelling region and the northerly neighbour to the Gulf of Guinea region, the coastal fisheries for sparids and sciaenids are in crisis (anecdotal evidence, S. Garcia, in litt). Does this mean that we may not trust the FAO data? Does it mean that the apparent difference between adjacent upwelling and oceanographically-stable regimes differ strikingly in their response to fishing?

It would be easy to use the two examples discussed above, and the many similar ones that may be extracted from the FAO archives, to suggest quite incorrectly that, far from failing, global fisheries are performing sufficiently well as to suggest that catches at levels not far from those we now enjoy may be sustained indefinitely. But those who have read my previous Viewpoint articles will know that this is not my opinion.

Nor is this the place to expand on how we should interpret the regional catches from the many other FAO regions; this would entail discussing the complex natural dynamics of coastal pelagic species that dominate the catch data of regions such as the Humboldt Current or off southwestern Africa, the interaction between environmental forcing and fishing mortality in gadoids off northwest Europe, and many other issues. Because many such complexities are hidden within the apparently simple data lying in the FAO archives, these archives should be used only with a reasonably secure knowledge of what they conceal: complex interactions between predator and prey fish species, between fish species and environmental forcing, between native and intrusive species, between fishery scientists, administrators and politicians, and finally – and perhaps as important as any other – between fishermen and their bankers.

The controversial paper of Worm et al. is close to what has been described as '*faith-based fishery science*' (Hilborn, 2006). A general assumption that fish stocks are everywhere in terminal decline has, Hilborn suggests, engendered among many fishery scientists a '*community of belief that fisheries management has failed, and that we need to abandon the* *old approaches and use marine protected areas and ecosystem based management*'. I have myself commented in different terms on the existence of this community of belief, with which may be associated (as in other such cases) the use of dialect: insistence on the neologism '*biodiversity'* and the use of terms associated with economics rather than ecology, such as the provision of '*ecosystem goods and services*'.

The use of such terms is, I believe, misleading and potentially dangerous to rational discussion, but it is clear that the editors of our most influential journals may be receptive to such language: a Concepts article by one Jeff Harvey on the '*The Natural Economy*' that appeared in Nature several years ago included the statement that '*Every human being lives within an ecosystem: towns, cities, farms, villages are freely leased to us by one or more ecosystems*'! It is hard to understand how such an astonishing statement could be printed in a periodical to which we look for the publication of the most fundamental new results in all scientific disciplines. My comments on it were, unurprisingly, rejected.

The results reported by Worm et al, and the very public interpretation of their text, must raise in our minds the issue of doubt and certainty in science (Young, 1951), and the increasing difficulty of certifying that important scientific results have a reasonable probability of being correct. The managing editor of Science, whose response I quoted above, had it completely wrong: if fisheries science and ocean ecology are contentious subjects, it is perhaps only because the process by which contributions are assessed by disinterested reviewers is starting to fail us.

Inevitably we are drawn to consider the apparent failure of peer reviewing in cases like this, and to the essential question: how should we resolve doubt and certainty in science now that individual scientists appear more likely than in the past to espouse social issues that are related to their research? If this is really the case, how may we now 'certify' that a scientific conclusion is the result of unbiased investigations and has received a disinterested review? More specifically, what are the consequences of part of the scientific community losing confidence in the review processes in what the press refers to as 'influential scientific periodicals'?

This is not a new concern, and doubts have already been raised concerning the reliability of peer-reviewing research on fisheries in Science and Nature: Polacheck (2006) noted that the practice of attracting media attention to forthcoming papers in these journals, and the consequent advocacy of marine policy change, is tending to *"undermine the trust placed in such journals to provide accurate and well-balanced review of the most important new scientific findings and their role in informing policy decisions based on those findings"*.

During the course of the 20$^{th}$ century we came to trust formal peer review as a necessary step in the publication of the results of our scientific investigations. Earlier, it was the direct responsibility of editors and of more experienced scientists to certify the papers of younger, less well-known contributors; this was famously performed by the Committee on Papers established in the 18$^{th}$ century by the Royal Society of London. Such formal approval was a public and personal affirmation that the author was competent, and worthy of being taken seriously in the public debate that represented the progress of science. Peer review today is almost always a private affair, and more specifically concerned with locating inadvertent errors or fraud.

But, does it work? To date, something like half-a dozen international symposia have been held to evaluate the success of peer review without reaching any clear opinion, for or against, so the problem is not simple. In discussing the apparent problem of peer review with fisheries scientists, I have encountered a strongly-held opinion that in the major 'influential scientific journals', so often quoted by journalists, the process is failing: one is certainly led to wonder why the peer reviewers of the Worm et al. paper failed so signally to locate many apparent errors.

Unfortunately, all judgments will to some extent reflect any strongly-held opinion of the judge concerning the case before him, and it would be naïve to think that peer review is not subject to similar bias. Unfortunately, today, we do not lack for social issues on which peer reviewers are likely to hold strong opinions: biodiversity, global warming, genetically-modified food, intelligent design and all the rest. The quality of contributions to the scientific debate risks being compromised not only by opinions held by authors on such sensitive topics, but also those held by their peer reviewers.

Perhaps the most rapidly-effective change of policy for editors would be to establish some simple rules: for instance, not to invite anybody whose name appears in the citation list under very recent date. And, of course, to prohibit the very dangerous practice (which I believe is associated with some journals) of inviting suggestions from the author for the names of suitable referees. I believe (without any very firm evidence, I admit) that our current problems with peer review stem largely from what I have called elsewhere '*in-group peer-reviewing*'.

In the end, it may not be faith-based fishery science that is the real danger to our subject, as Ray Hilborn has suggested, but rather our own faith-based reviewing of that science at the request of journal editors.

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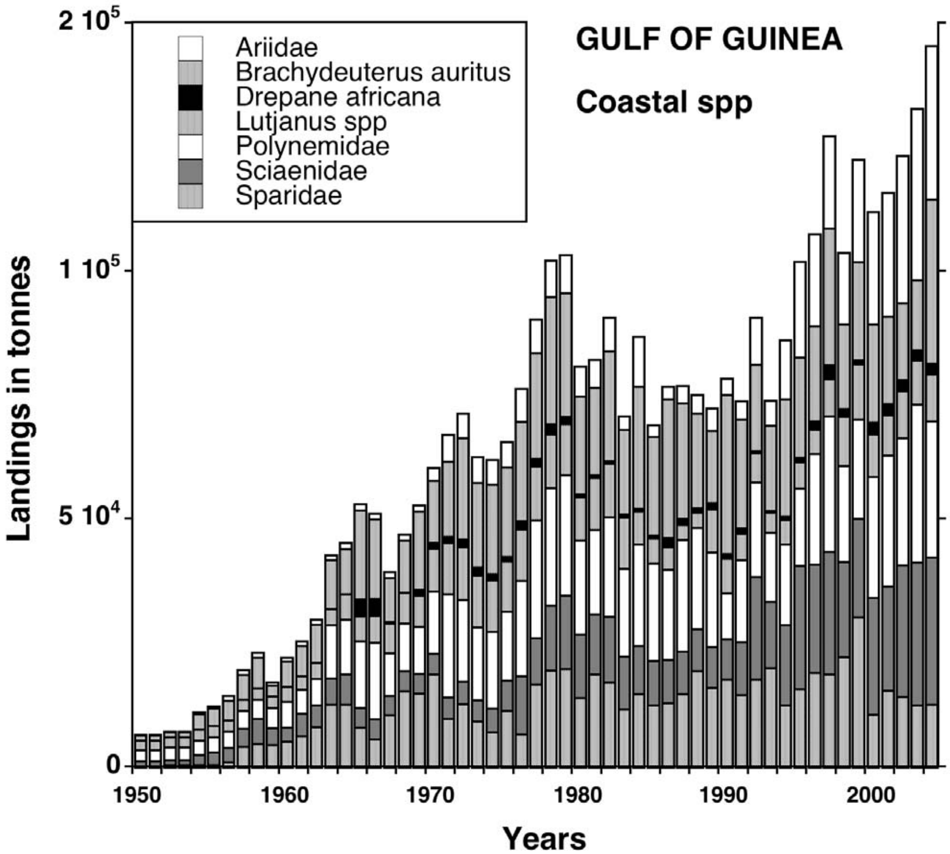


Legend for figure:

The progression of catches in the Nigerian fishery, 1950-2005, partitioned between some of the characteristic groups of inner continental shelf species of teleosts.