A LITTLE KNOWLEDGE (PLUS A LOT OF INFORMATION) MAY BE A VERY DANGEROUS COMBINATION.

A recent Research Article[i] raises critical concerns for the reputation of Science (and, incidentally, of science). This article stated that fishery data may be extrapolated to predict the total collapse, by the year 2048, of all stocks that are currently fished; I understand that comments concerning their interpretation of the global catch data have already been made.

Here, I address two other problems raised by this article. A review of published experimental results[ii] led the authors to conclude that "diversity enhanced ecosystem stability", where this is defined as "the ability to withstand recurrent perturbations". This curious statement reverses modern theory of ecological diversity in the ocean as discussed, for instance, by Ramón Margalef[iii]. Ecosystems of polar seas and upwelling regions are constrained to remain low-diversity, juvenile systems by the strong seasonal "recurrent perturbations" that they experience, while ecosystems of tropical seas are permitted to evolve their characteristic high diversity only because of the relatively low level of "recurrent perturbation" that they experience. Here is a classical case of the danger of assuming that correlation indicates causality.

Their conclusion is then interpreted in the context of the numbers of fish species in each of 64 large marine ecosystems. These LMEs collectively encompass the entire coastal ocean[iv] and are described by Worm et al. as "characterised by distinct bathymetry, hydrography, productivity and food webs". The FishBase data[v] used for this analysis are the cumulative numbers of species reported (1950-2003) in each LME, and are greatly in excess of the numbers of species relevant to fisheries ecology. For instance, 725 species are listed for the Gulf of Guinea LME where only about 200 fish species, including both demersal and pelagic teleosts and elasmobranchs, occur in standard taxonomic works describing the continental shelf fauna. Unfortunately, the FishBase data include also (for instance) the myriads of meso- and bathypelagic myctophids and other species that live at considerable depths in the open ocean and are not relevant to fisheries ecology; judicious, knowledgeable editing of this data base was required before it could be used for this analysis.

Then, individual LMEs are highly variable in size and in their relative proportions of shelf and deep ocean areas, so that they cannot be treated as standard units in comparative analysis. Consider the case of LMEs #13 (Humbolt Current) and #44 (West Central Australia); both typify the western coast of a continent, but the former extends over 46° of latitude from Ecuador to Cape Horn, encompassing three quite distinct biogeographic provinces, while the latter extends over only about 10° and comprises a single biogeographic entity. Neither extends seawards more than about twice the width of the continental shelf, but LMEs #32 and #34 (Arabian Sea and Bay of Bengal respectively) are oceanic provinces, with only about 15% of their total areas at shelf depths. The Arabian

Sea province, furthermore, includes two functionally different upwelling ecosystems on, respectively, the Somali and Indian coastlines[vi].

How, then, can we take seriously the central conclusions of Worm et al.? These are illustrated in their now-much-copied Fig. 3, that relates the regional percentages of collapses and recoveries of fish stocks, of catch stability and of catch productivity against a "relative species richness" derived, as described above, from the apparent diversity in individual LMEs.

Of course, all this raises the increasingly worrying problem of the certification of science publications, because the critical refereeing process appears to be progressively eroding. It has recently been shown that "two journals with the highest profile, Science and Nature, clearly publish articles on fisheries not for their scientific content but for their publicity value…these papers are being published only because the editors and selected reviewers (emphasis added) believe in the message" [vii]. Why, in the case of the paper by Worms et al., did none of the reviewers notice their lack of general ecological knowledge and their casual data interpretation? This problem is becoming widespread: a critical examination of the recently-fashionable concept that 'iron-from-the-skies' controls the global pattern of oceanic productivity demonstrated that this concept is largely mythic[viii]. Here, the socially-correct message was that iron fertilisation of the oceans might enable us to draw down excess CO_2 from the atmosphere.

All this must be interpreted as the abandonment of scholarity in favour of societal concern for environmental problems; in the case of Worm et al., the socially-acceptable message that "biodiversity is good for us" appears to have been espoused with a striking lack of critical thought.

Furthermore, the on-line availability of large data archives may perhaps not encourage careful thought about how the individual numbers should be interpreted: I suggest, therefore, that a little knowledge and a lot of information may be a dangerous combination. This is especially the case for scientists more concerned with social problems than with careful, old-fashioned analysis of what we know (or what we do not know) about our world.

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[i] B. Worm et al. (2006) "Impacts of biodiversity loss on ocean ecosystem services", Science 314, 787-790.

[ii] Their analysis was based on metanalysis of experimental results published in 16 science journals from 1960 to 1995.

[iii] Margalef, R. (1997) "Our Biosphere". Inter-Research, Oldendorf, Germany.

[iv] Sherman, K. and A. Duda (1999) Marine Ecology Progress Series 190, 271.

[v] FishBase at www. fishbase.org (Fisheries Center, U. British Columbia).

[vi] A. Longhurst (2006) "Ecological Geography of the Sea", Academic-Elsevier (pp. 291-308)

[vii] R. Hilborn (2006) Faith-based fisheries. Fisheries 31, 554-555.

[viii] A. Longhurst (vide supra), pp. 71-87.