

REJOINDER :
Towards Consilience in Small-Scale Fisheries Research

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The five responses to my keynote address range from the mostly positive ('observations about our work as social scientists, which I imagine most of us would appreciate') to the mostly negative ('unsubstantiated guesswork'), with one even orthogonal to what I said and wrote. In this rejoinder, their content is best addressed by author, rather than thematically.

The first contribution, by Svein Jentoft, agrees with the thrust of my argument, and I, unsurprisingly, agree with the thrust of his. It reiterates positions of which I first read in Jentoft (1998), and which greatly influenced my thinking. Agreements do not generate sparks, however, and even less light, so I will amplify one little point of disagreement, or at least non-overlap, pertaining to the 'Malthusian' part of the 'Malthusian overfishing' concept. Malthus (1798) wrote about human population growth leading to conflict because 'demand' (assumed proportional to population size) would always, in the long term, outgrow supply (for example, productivity of cultivated land). However one might feel about this with regards to agriculture (the jury may be out, still), this is certainly true in fisheries, whose productivity (= 'supply') has an upper limit set by natural processes. Thus, any fishery can get into a Malthusian squeeze, if 'demand' (via effective fishing effort) is allowed to increase beyond a certain level, set by the local environment. The driver of increasing 'demand' need not be the growth of the fishing population. It can be, for example, the use of a more effective technology. Indeed, Fitzpatrick (1966, cited in Garcia and Newton 1997) published data from which one can calculate that the effective effort of fishing crafts, ranging from motorised pirogues to large trawlers, increased by an average of 4.4 percent per year between 1965 and 1995. In other words, every fifteen years or so, the effective fishing effort around a small port, in both the developing and the developed world, is doubling, even when the number and size of its fishing boats remain the same (and they usually do not). This, ultimately, is what undermines sustainability, even in relatively well-managed fisheries, and even in countries where fisher populations stagnate.

I should now turn to the fact that Svein and I basically agree. Agreement between practitioners of different disciplines is not saying the same thing in different disciplinary languages. It is rather a matter of saying things that are mutually compatible: one set of observations and the intellectual constructs that give them meaning 'jump together', that is, are consilient with one another, though they may be of different disciplines, and address different issues (Wilson 1998). Such consilience is to be expected if, and only if, the observations and their interpretative frameworks reflect the same underlying reality. Consilience thus provides, in the form of what may be called 'multi-dimensional' tests, an added benefit of interdisciplinarity.

Finally, I appreciate Svein's point about lack of interest in the ledger sheets of the multinational companies that now dominate large-scale fisheries. They pose, indeed, few questions of scientific interest, except perhaps by providing exemplars of corporate behaviour for students of applied ethics (Barkan 2004).

This last point is the reason why I feel so awkward responding to the contribution by Nathalie Steins. Obviously, the mobility between academia and the private sector is a good thing -- especially given the dearth of real jobs in the academic world. However, retaining openness to inquiry when transiting from academia to the private sector must be difficult. In this case, for example, I cannot help but notice the rapidity with which the peculiar vocabulary of corporate spokespersons was adopted. Thus, the predicable use of words such as 'romantic' to characterise positions in support of small-scale fisheries. I do not hold romantic notions about small-scale fisheries. Rather, I make the case, based on the cold facts in my Figure 3, that our society is, in many cases, better served by small-scale fisheries. An example that comes to mind is the annihilation, within two decades, of the massive cod stock off Newfoundland and Labrador by trawl fisheries (Walters and Maguire 1996). This is a stock which was exploited by trap and line fisheries for centuries, while supplying national and international markets. It was overfished by industrial trawling and is now gone, quite unromantically.

Also, the language of corporate spokespersons can make one forget that numbers about stocks and catches are estimates of these things, not the things themselves. Thus, for example, the assertion that '[a]fter all, industrial fisheries in the South and North provide the bulk of fish' is true only if reduction fisheries are added to the fish used for human consumption. If only the latter is considered, the landed catches of industrial and small-sale fisheries are about the same (again, see Figure 3).

Indeed, this illustrates the point that was made earlier about field scientists (of different disciplines) not recording the catches of small-scale fishers. The reputedly low catches of small-scale fisheries then make industrial fisheries appear more important than they are. Indeed, one could say, with the same insouciance as displayed here, that 'after all, small-scale fisheries feed and employ millions more people than industrial fisheries, and therefore, they must have priority in any conflicts that oppose these two types of fisheries'.

Then we have Dr. van Ginkel, wielding Latin as if it were an argument ('*si tacuisses...*'), calling me names, and feeling really, really defensive about a stereotyping, self-citing (Pauly 1994; Pauly and Stergiou 2005) biologist who tries to invade his turf. Maybe I exaggerate, but frankly, I did not sense any real appreciation of an attempt at bridge building between our disciplines, if only by identifying what would be carried across these bridges, once built.

Thus Dr. van Ginkel disposes of the suggestion of reporting observed catches in fisher communities: 'I fail to see why social scientists should be better positioned than biologists to collect Third World small-scale fisheries catch statistics. It is not -- nor should it be -- their core business to do so'. When performing field work, anthropologists live for extended periods of time -- often years -- among the people they study. Anthropologists who study fishing cultures thus live and observe people who fish (contrary to most fishery biologists), whose entire culture is centred on fishing, and whose livelihood depends on securing access to an elusive resource. This is what they do, and their success is measured by their catch. What 'core business' can anthropologists have that ignores (as

many anthropological accounts do) the 'core businesses' of the people they study? In fact, it can be done, although my best examples do not involve marine fishing communities (Ruddle 1983, 1986; Ruddle and Chesterfield 1977).

Imagine we wanted a bridge, or better, a common currency between maritime anthropologists, fisheries biologists, economists and policy makers. What better currency than catches? Fishing cultures celebrates them, biologists try to maintain them, economists follow them as they flow through rural and urban markets, and finally, catches animate a whole human coastal system impacted by policy, in which anthropologists should participate. But not if they do not care about the core activity of the people they study.

I knew Bob Johannes, and chose him deliberately for my example. What Dr. van Ginkel saw as 'commendable crossing of the disciplinary boundaries' however, was a one-way trip. Indeed, in his later years, Bob very much rejected the notion of basing fishery management decisions on insights other than those gained directly from fishers. He had come to reject the natural sciences in which he was trained -- and in which he excelled and to this date provide most of the citations his publications get. Bob crossed all right, but afterwards, he was never seen near the bridge.

As for Clifford Geertz, he certainly was no 'straw man', and citing him was not caricaturing anthropology. In fact, '[d]uring the past thirty years, Geertz and a number of like-minded colleagues and followers have been at the height of academic authority not only in anthropology but also in history, the social sciences, the study of literature, and the emergence of the field known as cultural studies' (Windschuttle 2002). The reason I cited him is because in his work, Geertz not only argued for the primacy of the local, but explicitly rejected science (as also in 'social sciences') and what might be called 'human universals' (Brown 1991). Such universals, however, are required for generalisations of the type I advocated to have any validity.

On the one hand, I may have presupposed too much: human universals are still much contested in anthropology. On the other hand -- and I admit being prescriptive here -- anthropology (the discipline, not the caricature) desperately needs to air its nether parts with a fresh dose of human universals. The alternative is that it continues to emit whiffs of that old colonial stench, the one that prevailed when the discipline was born. (Such airing is presently occurring in all biological disciplines involving humans, and these disciplines will be much the better for it.)

The fourth comment, by Collett, contains many statements, which confirm and extend statements I made in my paper. Some other statements contradict, or even refute mine. But the fact is, I cannot disentangle the thread of Collett's argument, let alone write a coherent rejoinder. Perhaps it is because Collett does not deal primarily with methodological issues -- about which we could exchange opinions -- but rather with the results of studies on Pacific Island, Mediterranean and other fisheries, most evaluated in normative mode. Here, I shall let the reader do the work.

As for *Google Scholar*, which Collett alludes to in his title, its performance is comparable to that of Thomson ISI (Pauly and Stergiou 2005), commonly used for comparative evaluations of research impact. It is tempting to dismiss such quantitative tools for evaluating oneself, or one's discipline, but more difficult to demonstrate that their output is meaningless. In fact, even when bias is

present, useful information can be gained, such as the citations patterns of colleagues working in the tropics (Pauly 1984).

This brings me back into familiar territory: the contribution by Chandrika Sharma. I agree with her observation that in India, 'middlemen' are a very significant factor in the life of small-scale fishers, and that the 'Malthusian overfishing' model, which does not include them explicitly, is thus incomplete. I have a sense, though, that the dominant, even overwhelming, role of middlemen is very much a phenomenon limited to South Asia and parts of Southeast Asia. In other regions of the world, marketing tends to be performed by female relatives of the fishermen, or through relationships, which, although involving an asymmetry of information and benefits (for example in the 'suki' in the Philippines; Cuyos and Spoehr 1976), may not necessarily embody the crass power imbalance, and hence extremely exploitative relationships, that Chandrika Sharma has in mind. Moreover, at least some of the money lending 'middlemen' operate where the frequent occurrence of typhoons represents a risk which no bank would ever assume, for example along the coast of Vietnam, and where their social obligations include, in the case of death at sea, taking care of bereaved households (Ruddle 1998).

She also notes that I did not include pollution as a factor influencing the yields of small-scale fisheries. I do agree that in many areas coastal pollution -- and also coastal development -- can have a strong impact on coastal fisheries. I do fear, however, that at regional and national scales, pollution cannot be a driver of the same magnitude as overfishing, which can and does destroy fishery resources. Remember the 4.4 percent mean annual increase of effective fishing effort alluded to above. In the absence of data to the contrary, this rate of increase can also be thought to prevail in India, with all the consequences that it entails.

As for her remark that it should not be only social scientists who should speak on behalf of small-scale fishers, I can only say *touché!* Fisheries biologists have indeed tended to study small-scale fisheries anecdotally, and not systematically. The best evidence of this is the absence, to date, of a comprehensive database of key information on the small-scale fisheries of the world.

Thus, I conclude this rejoinder by announcing that, a number of colleagues in and around the *Sea Around Us* Project (see www.searoundus.org) will publish online a global database of small-scale fisheries catches, constructed as described by Chuenpagdee and Pauly (*in press*). This database will feature definitions (small-scale fisheries do differ), number of fishers, and their annual estimated catch (as minimum input) for all countries and territories of the world. Perhaps more importantly for maritime anthropologists and other social scientists interested in fisheries, it will also include, to the extent that we secure the required collaborators, portraits of the fisheries in question, and hopefully, documentation for these (in PDF format). This, then, is the bridge that I propose to contribute to.

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