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Fishing for facts on the environmental effects of trawling and dredge fisheries: Reply to Løkkeborg

Unfortunately Løkkeborg misunderstood our criticisms of FAO report 472 (Løkkeborg, 2007) as he simply reiterates his arguments without addressing our substantive criticism. In the last 20 years or so, the scientific community has learnt a lot about the impacts of fishing on the marine environment; we have learnt about issues of scaling in ecological and environmental studies, and we have learnt about the value of inferences that are based on multiple lines of cumulative evidence derived from different types of studies (laboratory experiments, field studies, mathematical models, comparative studies and appropriate analogies). There is a strong parallel here with the current argument over the validity of the phenomenon of the current alarming rate of climate change, in which there are people who challenge expert consensus, and these people gain exposure in the popular media while doing a great disservice to science and future generations by delaying effective remedial action (see Ehrlich and Ehrlich, 1998 for a discussion of how environmental rhetoric threatens our future). The Løkkeborg report falls into exactly this format because the varying degrees of damage to the seabed from trawling are documented. The current level of documentation is so widely accepted in the public arena that if we do not move forward from entrenched views it will inevitably lead to polarised alternatives.

In his response to our original critique Løkkeborg opens his rebuttal with quotations from five different sources that are apparently contradictory; unfortunately these are taken out of context and/or out of date. Critique is an important element of science that contributes to the standing of any review amongst our peers. Because of the policy consequences such as extreme moves to completely ban trawling, an honest discussion of the environmental effects of fishing is important to move forward from gainsaying and petty obfuscation and thence to contribute to the challenge posed by the general recognition that habitat disturbance by trawl and dredge fisheries is an important societal and management issue. For example, Løkkeborg (2007) takes exception to the statement quoted from Watling and Norse (1998), yet this is a broad statement that merely states that bottom trawling occurs across the globe, and that where it occurs the substratum in which the fishing gear comes into contact will be altered to some degree, which will vary in severity. There is nothing controversial in this statement and its assertions are upheld by the recent literature (e.g. summarized in Kaiser et al., 2006). Predictions in environmental science commonly test hypotheses across scales of space, time and biological organization and this iterative process is applied equally in research into the effects of fishing on the substratum (e.g. Thrush et al., 1995, 1998; Hiddink et al., 2006). We believe the failure to understand these processes limits the ability of good environmental science to contribute to wise resource management.

Løkkeborg (2007) does not, as he claims 'give a critical evaluation of the methodologies applied in trawl impact studies.' Instead he argues that since soft-sediment benthos is variable in time and space it is not possible to demonstrate clear effects [of trawling]. This statement is simply untrue. In our research careers, similar to many others, we have demonstrated effects on the benthos over various spatial and temporal scales, often associated with broad-scale anthropogenic impacts. Variance is an important functional element in natural ecosystems that is integrally linked to diversity; emphatically it is not noise that obscures process as implied by Løkkeborg. For brevity we included a few references that summarize many of the studies on the environmental effects of fishing. Løkkeborg goes on to state that, 'Without giving examples or citing relevant studies, they [the present authors] state that benthic assemblages are the most widely and successfully used systems globally, for monitoring the impacts of contaminants, eutrophication and other man-made disturbances'. Do we really need to cite references to the thousands of studies on this topic and the countless national and international monitoring programs that use benthic soft sediment systems for monitoring? Surely any undergraduate student of marine biology knows this? Had a soft-sediment ecologist been the author of the FAO paper it would not be necessary to provide such documentation. A simple Google scholar search will demonstrate what we expect most MPB readers know to be true. For example the key terms 'Benthic ecology impact study' return over 30 000 records.

A major issue for Løkkeborg (2007) is that of inference and experimental design, firstly he contends that 'Gray et al. (2006, 2007) seem to ignore the caveats and difficulties associated with impact studies.' Addressing variability and diversity are not difficulties for benthic ecologists and we consider that collectively we have contributed to the recognition of scaling issues, the measurement and prediction of

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diversity in heterogeneous habitats and the design and implementation of impact and monitoring studies, all of which provides some level of expertise with which to comment on the validity of Løkkeborg's opinions. But more generally our question remains, if identifying effects in soft-sediment communities (or any other habitat for that matter) is so difficult, why are ecological responses so commonly used for impact and risk assessment? Løkkeborg (2007) then turns his attention to the issue of an apparent lack of adequate control sites in studies of the impacts of fishing. Again, in some cases, the authors to which Løkkeborg (2007) refers are quoted out of context. For example, the comment by Jennings and Kaiser (1998) related to the problem of how we determine the causal agent/s of large-scales shifts that have been observed in some marine ecosystems. It is often difficult to attribute 'cause' to one 'effect' and many benthic ecosystems may well be exposed to multiple stressors emphasizing the need for a broader perspective in synthesis and analysis. Another important implication of the use of controls overlooked by Løkkeborg is that the effect size determined in an experimental study is not only influenced by the magnitude of the treatments used but also by the adequacy of the controls (i.e., the strength of the gradient of effects encompassed). A number of studies have highlighted that if experiments are performed in areas that are already impacted by fishing effects, the effects that were detected would be even more severe had the experiments been conducted in areas that had not been exposed previously to fishing disturbance (Dayton et al., 1998; Thrush et al., 2005). The critical issue is estimating the magnitude of change and its consequences to ecosystem function and sustainability, and addressing that issue requires good ecological understanding, synthesis and integration across studies. Løkkeborg (2007) also takes issue with the fact that we criticize the study of Lindegarth et al. (2000a,b) that in Løkkeborg's opinion is one of the most comprehensive and well designed impact studies. This is the author's opinion and is open to considerable dispute. Løkkeborg mentions Lindegarth et al's papers no less than 10 times and includes a figure of the sampling design in his FAO paper. We contend, as we claimed, that this demonstrates that Løkkeborg 'strongly relied on' this paper. An important issue here is the scale of biological resolution encompassed by the study. Løkkeborg (2007) indicates that because other cited studies have used techniques that sample only megafauna (e.g. Collie et al., 1997; Kaiser et al., 2000a,b; McConnaughy et al., 2000 and more) that this validates the approach of Lindegarth et al. (2000a,b). However, our careful scrutiny was not addressed. The studies cited certainly sampled megafauna, but they did so with sampling tools and at a scale that was entirely appropriate to sample this fauna effectively. However, Lindegarth et al. (2000a,b) used tools adequate for sampling only the more numerous smaller bodied organisms that they discarded from their analysis. More appropriate techniques such as towed beam trawls, dredges, camera and video surveys that sample at scales of m^2–100s m^2 could have been used in this study. This explains the dichotomy between the findings of Tuck et al. (1998); Lindegarth et al.'s (2000a,b) study. Tuck et al. (1998) found significant effects of trawling on the macroinfauna retained on a 1 mm mesh sieve of muddy sediments in a sea loch, a similar environment to that studied by Lindegarth et al. (2000a,b) that had been protected from fishing through a military closure order for 25 years.

Løkkeborg (2007) contends that the only way to demonstrate effects of trawling is by means of Beyond-BACI designs with many replicate control sites. If most of the continental shelves round the globe are trawled it becomes a challenging, but not intractable proposition to determine cause and effect. This is rather a narrow view of the way that scientists infer cause and effect relationships, and reference to modern texts on the philosophy of science will point to the diversity of techniques we employ to scientifically advance understanding. Many approaches are relevant to studying broad-scale impacts on the seafloor. Løkkeborg implies that detecting cause and effect relationships is a straightforward process if only there are adequate controls and levels of replication, this is overly simplistic. In ecological systems, feedbacks can make defining cause and effect difficult. This is illustrated by recent research emphasis on the functional consequences of biodiversity loss that emphasizes that organisms are not only influenced by their environment but also themselves significantly affect it and that these effects are not restricted to small spatial scales (Thrush and Dayton, 2002; Solan et al., 2004). The loss of functionally important species, ecological relationships, and biogenically structured habitats is a serious threat from trawl and dredge fisheries. Again this emphasizes the need for a broad perspective that encompasses an understanding of ecology and environment in reaching conclusions about environmental effects.

We disagree fundamentally with Løkkeborg (2007) that he has taken account of Type-II statistical errors. This was discussed in our reply to Director Valdirmarsson of FAO. If Løkkeborg had been aware of any of our papers he would have seen that we are very much aware of the difference between Type-I and Type-II errors. The approach advocated by FAO would give primacy to Type-II errors and interpret data that showed high variability with appropriate caution. In contrast, Løkkeborg interprets such data as showing no effect, thus making a Type-II error himself. This is why Løkkeborg's last sentence (2007) 'if claims of impacts of fishing are subsequently shown to be in error, then there is danger that the role of scientists in offering management advice will be further discredited,' is erroneous. Løkkeborg argues for large-scale experiments. However, our understanding of the spatial heterogeneity and the functioning of benthic systems implies that such studies alone will not provide for strong inference and should be supported by other approaches (small scale studies, modeling, risk assessments). It is a fallacy to assume that because of the broad scales of disturbance potentially generated by trawling and dredging that all experiments must be con-

ducted at this scale. While studies at a broader scale are certainly important, let us consider the determination of contaminant effects with which readers of MPB will be familiar. Laboratory based ecotoxicological studies are an important element in the detection of toxicity and the determination of tolerable levels. Again this does not mean that such laboratory based approaches should be used in isolation, surveys and models of contaminant effects are also important. But these small-scale studies have significantly influenced management and policy, and this has cost industry and society in compliance costs.

Perhaps the most bizarre claim in Løkkeborg's reply (2007) is that the study done in 2006 of the benthos of the Tromsøflaket in Norway did not use multibeam sonar. The study was done as part of the Norwegian MAREANO project run by the Geological Survey of Norway (NGU). NGU routinely maps the seabed using multibeam sonar. On the Institute of Marine Research's own webpage describing the cruise one finds a composite multibeam image of Tromsøflaket based on NGUs multibeam sonar! (http://www.imr.no/aktuelt/nyhetsarkiv/2006/juni/80_km_havbunn_filmet). The main conclusion from this research cruise, which was reported in Norway's daily paper Aftenposten (http://www.aftenposten.no/nyheter/miljo/article1347806.ece), is headlined 'Sea-bottom destroyed' and has a quote from the cruise leader, Løkkeborg's colleague at the Institute of Marine Research, P.B. Mortensen that 'The Marine Research Institute has many hours of video which shows 'ploughed up' seabed with many hundreds of meters of long, deep criss-crossing furrows... in the furrows are many dead sponges'.

Løkkeborg (2007) believes that 'the main concern regarding the impacts of towed fishing gears involves how changes to benthic community structure may affect exploited marine resources'. Such a view reflects a sector-orientated perspective, that of the fishing industry. Fortunately, this is not the view of FAO whose code-of-conduct seeks to ensure that habitats are not damaged by fishing gear. The worst effects of trawling are the homogenization of heterogeneous habitats, the structure of which is maintained by the benthic organisms themselves. (See, Thrush et al., 2006 and Blyth et al., 2004 for recent studies). Our concern is that the natural habitat heterogeneity of soft sediments should be maintained and not be destroyed by towed fishing gear. The key issue is that of sustainability, both in respect of the target species, but also the habitats and biota that maintain the ecosystem functions and processes that support those target species. Most importantly the very scale of the disturbance implied and the potential for benthic communities to recover from this disturbance is such that this must indicate the high risks of ecological damage, change in ecological function and loss of productivity in these ecosystems (Hiddink et al., 2006). This after all is the general pattern we see with the sequential loss of habitat complexity and depletion in the most valued fisheries.

In conclusion, the response by Løkkeborg (2007) sadly has served only to highlight a fundamental misunderstanding of the application and interpretation of the experimental approaches used by ecologists. Such misconceptions will be obvious to any reader with such a background and one wonders what process of external peer review was undertaken with FAO report 472 (Løkkeborg, 2005). Beyond what Løkkeborg considers definitive proof, we must also consider the risk involved in stagnation of environmental science rather than in its evolution. This is not a question of a lack of rigor, simply recognition that management decisions must be made and that they are often based on the best available science. We do not consider this was fairly represented on any level by Løkkeborg's review. Most people now accept that fishing has effects on the seabed (however variable) – therefore the question is how bad is it? What is the potential for recovery and resilience? This requires a shift in the thinking of those involved in the fishing industry and many in fisheries management agencies. What we seek to achieve here is to switch all parties involved in the use and sustainability of the coasts and oceans to the idea that they can be proactive and move to a more productive research agenda focused on ecosystem-aware management because ultimately the debate is about wasting opportunity and natural resources. Considering that the FAO's own precautionary language is utterly ignored in this FAO publication, it is easy to understand the growing groundswell, including even President Bush who is seeking a ban on destructive fishing, (http://www.cbsnews.com/stories/2006/10/03/ap/tech/mainD8KHCGAG0.shtml). This then sets a new baseline for management and in order to re-establish trawling, industry and resource management agencies will need to implement the precautionary approach and assume the burden of proof that the trawling does not do serious environmental harm.

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