



Reply

Reply to Andersen *et al.* (2016) “Assumptions behind size-based ecosystem models are realistic”

Rainer Froese^{1*}, Carl Walters², Daniel Pauly², Henning Winker^{3,4}, Olaf L. F. Weyl⁵, Nazli Demirel⁶, Athanassios C. Tsikliras⁷, and Sidney J. Holt⁸

¹GEOMAR, Helmholtz Centre for Ocean Research, Düsterbrookweg 20, 24105 Kiel, Germany

²Fisheries Centre, University of British Columbia, Vancouver, BC, Canada

³Kirstenbosch Research Centre, South African National Biodiversity Institute, Claremont 7735, South Africa

⁴Centre for Statistics in Ecology, Environment and Conservation (SEEC), Department of Statistical Sciences, University of Cape Town, Rondebosch, Cape Town 7701, South Africa

⁵South African Institute for Aquatic Biodiversity, Grahamstown 6140, South Africa

⁶Institute of Marine Sciences and Management, Istanbul University, Istanbul 34134, Turkey

⁷Department of Ichthyology, School of Biology, Aristotle University of Thessaloniki, Thessaloniki, Greece

⁸Independent Scientist, Paciano, Italy

*Corresponding author: tel: + 49 431 600 4579; fax: + 49 431 600 1699; e-mail: rfroese@geomar.de

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In a recent publication (Froese *et al.*, *ICES Journal of Marine Science*; doi:10.1093/icesjms/fsv122), we presented a critique of the balanced harvesting (BH) approach to fishing. A short section dealt with the size-spectrum models used to justify BH, wherein we pointed out the lack of realism of these models, which mostly represented ecosystems as consisting of a single cannibalistic species. Andersen *et al.* (*ICES Journal of Marine Science*; doi:10.1093/icesjms/fsv211) commented on our paper and suggested that we criticized size-spectrum models in general and that we supposedly made several erroneous statements. We stress that we only referred to the size-spectrum models that we cited, and we respond to each supposedly erroneous statement. We still believe that the size-spectrum models used to justify BH were highly unrealistic and not suitable for evaluating real-world fishing strategies. We agree with Andersen *et al.* that BH is unlikely to be a useful guiding principle for ecosystem-based fisheries management, for many reasons. The use of unrealistic models is one of them.

Keywords: balanced harvesting, ecosystem-based fisheries management, size-spectrum models, unrealistic model assumptions.

Introduction

In a previous publication (Froese *et al.*, 2015), we criticized an approach to fishing called “balanced harvesting” (BH), which aims to distribute “... a moderate mortality from fishing across the widest possible range of species, stocks, and sizes in an ecosystem, in proportion to their natural productivity, so that the relative size and species composition is maintained” (Garcia *et al.*, 2012, p. 1045). Our critique concluded that evolutionary theory, population dynamics theory, ecosystem models with realistic assumptions and settings, and a large body of empirical evidence do not support BH. Andersen *et al.* (2016) published a comment to our paper in

which they concur with several of our points, notably “that BH in its current pure form is technically difficult to implement in industrial fisheries; is unlikely to offset fisheries induced evolution; [and] is economically unviable for many countries and cultures [...]”. They also agree with our conclusion that exploitation should be guided by moderate harvesting of resilient species, while impacts on stocks and ecosystems are minimized. Andersen *et al.* are, however, concerned that we have dismissed size-spectrum models “*en bloc*” as highly unrealistic. That was not our intention, as is evident from the heading of the pertinent short section, which reads “Size-spectrum models used to justify BH are highly unrealistic”, and from the conclusion

of that section, which reads “In summary, the size-spectrum models presented in support of BH make a number of unrealistic and even contradictory assumptions, which call into question the validity of their support for BH”. Clearly, our statements did not refer to size-spectrum models in general, but only to those used to support BH.

Andersen *et al.* then went on to defend mostly the size-spectrum model of Jacobsen *et al.* (2014), who are among their co-authors. They present six points detailing their claim that we have made erroneous statements about size-spectrum models:

The first point states that we wrongly perceived Jacobsen *et al.* (2014) as supporting BH. That study explored four different exploitation patterns and concludes in the abstract: “We find that unselective balanced fishing, where individuals are exploited in proportion to their productivity, produces a slightly larger total maximum sustainable yield than the other exploitation patterns and, for a given yield, the least change in the relative biomass composition of the fish community”. We interpreted that statement as “support of BH”, but we appreciate that Andersen *et al.* may see this differently.

The second point takes issue with our inclusion of Jacobsen *et al.* (2014) among three references cited in support of the statement “Size-spectrum models that have predicted higher yields from individual species by using BH [...]”, because Jacobsen *et al.* (2014) did not use one, but 20 simulated species to predict higher overall yield from using BH. Fair enough, but given that Jacobsen *et al.* (2014) did predict higher yields, same as the other cited models, is this really an erroneous statement requiring the publication of a correction?

The third point states that we wrongly criticized size-spectrum models as unrealistic because they do not produce “lumpy” or “dome-shaped” biomass distributions. But that is not what we wrote. Rather, we criticized that some models “have assumed mortality rates high enough to cause decreasing biomass with size over a wide range of sizes within each species, as if decrease in biomass with size for communities as a whole also applies to each species within the community size spectrum”. Andersen *et al.* concede that this is the case in the size-spectrum models of, for example, Law *et al.* (2012, 2013), but not in the multi-species model of Jacobsen *et al.* (2014), for which a graph produced by Andersen *et al.* (their Fig. 1B) shows the peak in cohort biomass at larger sizes, as expected. We apologize for having wrongly cited Jacobsen *et al.* (2014) in this context. Our interpretation of biomass density graphs followed Law *et al.* (2012, p. 605), who derive such graph from their one-species model, with high biomass density at small body size, and state that the resulting graph is “[...] analogous to a conventional biomass pyramid for trophic levels, laid on its side”. Andersen *et al.* explain the strange accumulation of cohort biomass at early life stages in the models of Law *et al.* (2012, 2013) as follows: “[...] juveniles do not compete for food. Density-dependence instead emerges late in life through a reliance of large individuals on cannibalism and the ensuing competition for feeding on juveniles”. We are not aware of a real-world species or ecosystem that fits this description. Thus, our statement was correct for the cited size-spectrum models published by Law *et al.* (2012, 2013), but not for the model of Jacobsen *et al.* (2014).

The fourth point refers to our description of the size-spectrum model used in Law *et al.* (2012), where we wrote: “Once fish reach maturity, a proportion of assimilated food (rather than body weight, as normally assumed) is allocated to the production of new eggs”. Andersen *et al.* argue that our comment in parenthesis is unjustified and that linking reproduction to available food is more realistic than, for example, a fecundity–weight relationship. We are not convinced by these arguments, as most fish transform

fatty tissue previously accumulated into gonads (Pauly, 2010), and the amount of fatty tissue available for production of eggs and sperm is part of their body weight. In other words, past food availability determines body weight, which then determines reproductive output. We do not believe that we made an erroneous statement here.

The fifth point refers to our statement: “Other models assume a strong replacement of natural mortality rates of small creatures by fishing mortality”. In Jacobsen *et al.* (2014, p. 6), this observation is expressed as follows: “As the abundance of predators is reduced, the release from predation makes it possible to fish the remaining individuals harder than expected from single-species fish stock assessments where the fishing mortality producing MSY typically would be around 0.3 yr^{-1} for larger species. Due mainly to reduction in predation mortality, the MSY [associated with BH] is generated at much higher levels of fishing mortality, where a high yield is achieved at the expense of a collapse of the largest species”. Andersen *et al.* do not dispute our statement, but insist that the replacement of natural mortality by fishing mortality is not an assumption but a result from size-spectrum models. We do not fully agree, because the strong interaction between predator abundance and prey mortality is a result of several other unrealistic model assumptions such as: (i) strong and continuous food limitation (no periods of high food availability where losses of body weight due to, for example, reproduction can easily be compensated for) and (ii) extremely limited trophic flows, when instead real-world predators use a wide range of food items opportunistically (see diet compositions compiled in FishBase; www.fishbase.org). As we pointed out in the original paper, if the proposed link between large and small species and the assumed degree of cannibalism were true, then the widely observed decrease in the abundance of large species (Pauly *et al.*, 1998; Myers and Worm, 2003) and of large individuals within species (Shin *et al.*, 2005; Froese *et al.*, 2008) should have led to an outburst of small pelagic fish stocks able to support much higher fishing mortality than predicted from single-species models. Also, the strong decrease in natural mortality of recruits should have led to massively improved recruitment in large and small fish in the past decades. Instead, we have observed the collapse of small pelagic fish stocks (Essington *et al.*, 2015) and severely reduced recruitment in both small and large species (Myers and Barrowman, 1996; Gascuel *et al.*, 2015). We maintain that the proposed strongly reduced natural mortality of small species and early life stages is not observed in the real world and is most likely an artefact of unrealistic model assumptions.

The sixth point refers to our criticism of highly unrealistic and contradictory assumptions about size-dependent natural mortality, for example, in Law *et al.* (2012, 2013). Andersen *et al.* argue that these assumptions were used to prevent undesired model behaviour, that such assumptions are also made in models other than size-spectrum models, and that alternative assumptions could have been made. That may be so, but pointing out these unrealistic assumptions clearly was not an erroneous statement on our part.

Andersen *et al.* then discuss peculiarities of different types of size-spectrum models, that is, topics which, in their own words, “reach beyond the issues of BH”, and which we therefore need not respond to here. They continue with a call for more modelling with different types of models and stress that: “Ultimately, conclusions can only be reached by judgement of model results in light of the limitations of each model, careful consideration of different model hypotheses and better integration with observations of ecosystems”. We agree.

In their conclusions, Andersen *et al.* stress that all ecosystem models are caricatures of the natural system. Yes, but a good caricature reproduces key traits of its target, so that these can be immediately recognized, even if incomplete or exaggerated. That is the case for data-driven ecosystem models, where results can be immediately compared against existing knowledge of the modelled system. Instead, most of the size-spectrum models used to justify BH consisted of a single, exclusively cannibalistic species. There was no resemblance to any ecosystem that we know. Yet, the results of such modelling were supposed to inform optimum exploitation of real-world ecosystems. We admit that we do have a problem with that.

The conclusions of Andersen *et al.* contain the sentence: “BH is unlikely to be a useful guiding principle for ecosystem-based fisheries management for many reasons, one of them being the unclear definition of what BH actually is”. We agree, and we have identified several of the other reasons here, and in our original contribution.

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