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Geoscientific Model Development Discussions

Interactive comment on "The ecological module of BOATS-1.0: a bioenergetically-constrained model of marine upper trophic levels suitable for studies of fisheries and ocean biogeochemistry" *by* D. A. Carozza et al.

Anonymous Referee #2

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Carozza et al. present an interesting and streamlined model of the ecology of commercial fishes (pelagic, demersal and benthic) in which they have clearly invested considerable time and effort. I think that the approach that they take has much to recommend it; particularly its relative simplicity and thus tractability for both computation and analysis. I also appreciate the authors making the code available and well commented, and I referred to it a couple of times for further clarification. All in all, it is a promising approach.

Of course, it is tricky to say how well the model captures the underlying ecology without

comparisons to data at multiple sites – and as the authors indicate, it is difficult to locate such data for open ocean ecosystems. The model also cannot reproduce processes that others can resolve, such as trophic cascades and predator-dependent mortality rates, which the authors acknowledge. In fact, I think the authors are a little too defensive about their necessary assumptions – every model is a simplified representation of a system, and of course will not capture everything perfectly.

I want to point out just a few places where I think the simplifying assumptions may break down somewhat; not necessarily for the authors to change their approach, but to indicate that these are important assumptions that should be further explored at some point.

Firstly, the input of all NPP as a potentially exploitable resource for commercial fish species seems excessive. A substantial fraction is taken up by the many other non-commercial organisms inhabiting the oceans, for example, by export flux that goes to mesopelagics, by marine mammals, by the millions of non-commercially fished marine species. Although this is moderated somewhat when growth is constrained by allometry instead of productivity, it would make sense to have some fraction of NPP go to other groups and not be available to modelled organisms. This fraction could be a parameter, and may improve fit to data.

As it stands, the model is likely biased in that for low productivity systems, most of the NPP will be taken up by commercial species (since NPP will be a strong constraint), whereas in high productivity systems, a much larger fraction will go to other groups (since NPP constraints will be relaxed and allometry play a stronger role). The authors might want to consider any effects this will have on their results.

Additionally, does the fact that this model can be (is) applied to the open ocean, where depths may be great, mean that there is also an issue in deeper regions, because the export productivity going to demersal and benthic organisms will be much lower (i.e. decrease exponentially), and so the 1/3 sharing for each group also be biased?

Secondly, having natural mortality be independent of predator biomass seems restrictive in terms of situations where it will be an appropriate assumption. For example, if all top predators are fished out, then (if I understand correctly), the mortality rate will not change, even though there are no uber-predators. Although the authors acknowledge this, their justifications ('without necessarily losing realism', and - to paraphrase - suggesting that 'lack of data is sufficient to assume that trophic cascades in the ocean do not happen and thus this simplification is OK') seems like a stretch. I would recommend not suggesting that this is 'realistic' (which it is not), but a necessary simplification which may lead to problems with certain biomass spectra. It might also be something worth exploring in the future.

Thirdly, no dispersal. On P10150 l15-17 'we effectively ignore nonlocal movements over spatial scales > 100x100km'. Whether this is an issue presumably depends upon the time-step of the model relative to the spatial scale. Here it is 15 days (1/2 month). For a fast region of the ocean (e.g. gulf stream, assume 6km/h, 144km/day, potentially 2160km/15 days), or even a moderate one, it does appear as if advection could move species through many grid cells and thus play a role. This should be included as a caveat, rather than saying 'we expect it to have a negligible impact on our results'. Also note that grid cells are much smaller at high latitudes on a 1 degree grid.

I also think the Watson et al. paper is mis-cited here; rather than saying 'These are complex processes whose role in determining fish biomass are difficult to quantitatively evaluate at the global scale given present knowledge', the last line of the Watson abstract is 'These results highlight the importance of considering movement in global-scale ecological models!'

Other comments:

Conversion between abundance and biomass: I don't buy that the conversion between abundance and biomass (e.g. 10153 l14-16) would not influence model dynamics. In an ideal setting (i.e. a continuous spectrum), then I think this would be the case. But

here, where there are 50 mass bins, this discretization will prevent conservation of mass and abundance. If the model was run with a fixed total amount of NPP input, and all pools of biomass resulting from this measured (including respired, detritus etc), I do not think this total would remain constant, as it would in a continuous setting. This is because of fishes 'jumping' between the mid-points of size bins, i.e. the growth rate may only be enough to just about take them into the next size bin, but they are automatically inserted at its geometric mean, representing an instantaneous accumulation of biomass not resulting from NPP. This is simply a computational artefact, but it will clearly affect both biomass and, therefore, abundance. So while true in the limit, I don't think that this model will actually give the same results when run as abundance or biomass.

I find the notation a little cumbersome; why not drop the mass and time dependencies (where possible) as per Appendix A? It would make it easier to read.

I was wondering where the details of parameter estimation were to be found; there is only a brief reference on p10171. This makes it hard to judge how effective the model fitting process is. Could a line or two be added to give more details (e.g. is this a Bayesian approach? If so, are flat priors being used? How many MC runs are used?

I would really like to see how precisely biomass spectra fit to data (slopes of -1.0 to -1.2 on p10173). There is nothing in the figure, and in the text it just says that 'they are consistent with published values', though maybe not at lower temperatures.. It would be good to get the mean values (and the confidence around that, from the MC simulations) into both the text and the figure, for comparisons sake.

The numerical methods (Appendix C, particularly C1) are really important and should be moved to the main text (Section 3), or at least the key points, so that all details of the model (timestep, grid cell size, numerical approach used, mass bin structure) are in one place. Details of the model mass bin approach (number of bins, bin boundaries) are not numerical methods, they are model structure details like the timestep or cell size.

Minor editorial comments

P10148 l5-6: '. . . not always coupled directly with predictive models of fishing activity'. It would be good to see a reference or two for this.

P10148 l10-11 'aims to represent the global community of marine organisms'. This is incorrect; all non-commercial species (millions!), marine mammals etc etc are left out. Please rephrase to more carefully delineate the boundaries.

P10148 l11 'a suite of super-organism populations' – not quite sure what is being referred to here. Is it the three size classes?

P10148 l18-19: 'which requires arbitrarily defining under-constrained feeding relationships' seems a little strong, given that the present model is arbitrarily defining many things (e.g. size bins). As mentioned previously, I don't think the authors need be so defensive.

P10151 l12-14: But there is a strong difference in how they will experience the total primary productivity input, particularly in deeper cells.

P10156 Eqn 5: The notation here is confusing – why not use the same symbol for formation of biomass (whether reproductive or somatic), instead of the same symbol for energetic input and somatic biomass?

P10157 l8: Is there any evidence for equal partitioning of NPP among size classes? If not, this should be stated as a (fairly strong) assumption.

P10170 l15-20: It would be useful to know bottom-depth at these sites.

P10174 l21: 'often unconstrainable ecological processes' – again this seems overly harsh, and not necessarily accurate.

P10175 l8: 'Reasonably realistic' would be more appropriate.

Table 2: By variables, do you mean fitted parameters? I'm just trying to get a sense of how many parameters are actually estimated in the model – it would be good to have this value in the text somewhere as well, because right now it is just stated that there are fewer parameters than comparable models, without saying how many there actually are.

I found Fig 1 a little unintuitive – is there a clearer way of presenting this?

Fig 5. 'Note that the spectral slope does not depend on NPP' please clarify for those just looking at the figures; also needs a clearer title for panel C

All in all, this is a good approach, and I think the assumptions are reasonable; they just need to be more carefully acknowledged and spelled out, rather than dismissed. Doing so would not reflect poorly on the model. I look forward to reading more about applications of this model in the future.

Interactive comment on Geosci. Model Dev. Discuss., 8, 10145, 2015.