Geoscientific Model Development Discussions

Interactive comment on "The role of ecosystem function and emergent relationships in the assessment of global marine ecosystem models: a case study with ERSEM" *by* L. de Mora et al.

Anonymous Referee #1

This manuscript presents several examples of how "emergent relationships" in model results (e.g., the fraction of chlorophyll contributed by diatoms, and the overall chl:C and POC:PON ratios) can be compared with data from observations to test the performance of plankton ecosystem models at the large scale. It argues that such comparisons provide a way of testing models that is more robust than the widely applied "point-by-point" direct comparisons of specific simulated values (e.g., nutrient and chlorophyll concentrations).

I find the comparisons meaningful and honest, in that they include cases in which the modeled emergent properties both agree and disagree with their counterparts from the observations. I also find that the authors make a compelling case that emergent

properties should be used more as metrics for comparing model results against observation.

Major Concern:

However, I do find that the authors are overly optimistic and tend to overstate the case that mere agreement of such emergent properties from the model with those from the observations constitutes "a strong indication that the model has a appropriate representation of the ecosystem functions that lead to the emergent relationship" (lines 6-8, in the Abstract). For example, the statement quoted above is at odds with the authors' later statement that, "Many interacting parts of an ecosystem can affect the balance of diatoms chlorophyll against the total community chlorophyll:..." (p. 6098, lines 15-16). Given that so many factors may affect any such large-scale "emergent relationship", it must therefore be possible to achieve similar emergent relationships using different model formulations, or different values of parameters for any given model formulation. In other words, some of the interacting parts may be made to counteract others in determining the overall emergent relationship. This is just another case of the well known problem that it is quite easy to get the correct modeled value for the wrong reasons, i.e., with many different incorrect parameterizations. Therefore, I recommend strongly that the wording be changed to not overstate the confidence that one might have that underlying model formulations are correct, merely based on the agreement of "emergent relationships". I do think that testing models with such relationships is quite valuable and should be encouraged, but not over-hyped.

Specific concerns:

1. The wording in the abstract concerning the "strong indication", as discussed above, should be revised not to overstate the case. 2. The following sentence also overstates the case: "The ideal scenario regarding the model version of the ecosystem function is that that is is an emergent property of the model, and is not constrained or imposed in anyway." (p. 6102, lines 2-4), in that the results of any deterministic model must

in fact be constrained and imposed by the choice of equations and the values of parameters employed. I suggest changing to something like, "..., and is not purposefully and obviously imposed a priori by the choice of model parameterization." 3. "in the absence of a causal relationship" (p. 6102, line 27) should be changed to something like "a purposefully prescribed functional relationship". 4. As in the abstract, the statement (p. 6103, lines 1-2) that, "The emergence of a coherent natural relationship in a simulation is a strong indication that the model has a appropriate representation of the ecosystem functions...", specifically the word "strong" should be revised. 5. (p. 6104, final paragraph): Was the model actually fitted (by tuning its parameters) to the data? I get the impression not. If this refers merely to the correlation from a model-data comparison, the wording should be changed to avoid confusion. 6. (p. 6111, line 19), "not a direct consequence" should be revised to something like "not an obvious and purposefully prescribed consequence of..." 7. Section 3.4: One possible reason for the discrepancy in the range of modeled vs. observed POC:PON ratios could be the contribution of dissolved organic matter (DOM) to observed POM, which as been documented by Dave Karl and co-authors in studies of the Hawaii Ocean Time-series. 8. (p. 6120, final paragraph). It does not make sense to state that the minimum dissolved inorganic phosphorus concentration is lower than the minimum of any ratio. This needs revision. 9. (p. 6124, line 2). "strong" is an overstatement. (p. 6126, lines 11-12) "Most importantly, ecosystem functions are the only way to demonstrate the modelscapacity to represent ecosystem function..." The tautology in the beginning of this sentence needs revision. Perhaps "...explicity consideration of ecosystem functions are...".

_____

Interactive comment on Geosci. Model Dev. Discuss., 8, 6095, 2015.

C2525