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Biogeosciences Discussions

Interactive comment on "Skill assessment of the PELAGOS global ocean biogeochemistry model over the period 1980–2000" *by* M. Vichi and S. Masina

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General Comments:

The authors, in this study, assess the performance of their biogeochemical model in simulating observed spatial and temporal patterns of chlorophyll, NPP and other biogeochemical and physical variables. This comparison is performed both on the global scale using satellite derived observations and at selected sites or regions using in situ data. They propose numerous tools (indices) to thoroughly evaluate the model performance. Their general conclusion, regarding this point, is that their model is as good as other similar models. In other words, it does not perform to badly when considering large scale and mean patterns. The main problem is the occurance of a strong

spring bloom in the Southern Ocean which is typical of global biogeochemical models and is, at least, partly related to the simulated spring and summer mixed layer depth. However, the model does a poor job when simulating the interannual variability at both (oligotrophic) JGOFS stations despite the physical considered variables are well reproduced.

This study represents a very nice validation exercise. It would be nice if all modelers could do the same before using their models for other purposes rather than visually validate their model using general typical statements such as "the model performs reasonnably well". However using numerous statistical tests to evaluate a model will only give scores that should be interpreted depending on what the model will be used for. A model may perform badly but may still remain usable to study specific aspects. Anyhow, model validation is a wide topic where subjectivity will remain largely present.

Unfortunately, I find that this paper is almost only a validation excercise and contains insufficient results. In particular, the authors only assess the pluses and the minuses of their model results without analysing carefully why the model fails or succeeds. My recommendation is not to accept the paper in its current format. I strongly encourage the authors to resubmit this study after including more scientific results and discussions. Potential aspects that could be developed are:

1) The failure in the Southern Ocean. The authors can articially increase the depth of the mixed layer in the Southern Ocean and analyse what happens there. this can be done, for instance, by allowing part of the input of TKE to be deposited below the depth of the mixed layer.

2) The problem of DOC is, I think, quite interesting. The discussion on this point can be extended as it is critical when interpreting bottle data and when using it to validate models.

3) The discussion on the metabolic balance is also interesting. However, some aspects are not clear. For instance, the authors mention that heterotrophy is obtained over the

global Atlantic ocean when using boreal winter data. From that, they conclude that extrapolation of any local data to larger spatial scale leads to bias toward heterotrophy. Hum, I don't see why ?? Thus, a potential improvement of the manuscript would be to extend the discussion on this important topic which could merit a full dedicated study rather than a short analysis in a general paper.

Interactive comment on Biogeosciences Discuss., 6, 3511, 2009.