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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

M. Vichi

vichi@bo.ingv.it

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As requested by all three reviewers, we have substantially revised the manuscript structure by separating the assessment part and highlighting the scientific findings in a new section entitled "Discussion and applications" (new Sec. 4). Section 3 now presents the results of the objective assessment with only limited discussions on the reasons for model success or failure in order to simplify the description. All the discussions on major biases and the acceptability thresholds of the performance indicators are moved to the new Sec. 4 (and subsection 4.1). We also discuss here the two results that the Referee indicated as major findings: the role of DOC in primary production estimates

(Sec. 4.2) and the implications on the metabolic balance of the ocean derived from model results (Sec. 4.3). Our results suggest that under current climate conditions the ocean is in slightly positive autotrophic balance as also evidenced by geochemical considerations and recent measurements.

Given the need to first objectively quantify the validity of the model, the specific applications are not fully exploited in this paper. For this reason we removed former Sec. 4.3 on the variability of primary production in the equatorial Pacific since, as pointed out by other comments, it required a more thorough analysis of the driving processes and not just a brief paragraph. This topic and further studies on the carbon metabolism will be the subject of future specific investigations both with a coupled Earth System Model and with additional forced simulations.

Conclusions are now separated from the discussion and report the major scientific findings and recommendations as suggested by Anonymous Referee #1.

- *Sect. 2.1, P. 3516 line 9: "... one-at-a-time modification ..." I am not sure what is meant by this.*
  One-at-a-time modification is a term used in sensitivity analysis (e.g. Saltelli et al., 2007) to specify the practice of changing only one parameter value and checking the sensitivity.

- *Sect. 2.2, P. 3517 line 9: Is there any specific reason of not using the most recent, possibly improved, carbon-based NPP data (Behrenfeld et al., 2005)? They are available on this website:*
  http://orca.science.oregonstate.edu/1080.by.2160.monthly.hdf.cbpm.s.php
  We thank the reviewer for this suggestion. The reason is that these data are not a major improvement with respect to the other satellite products, as it is described in Friedrichs et al. (2009) and also in Fig. 7 (the carbon-based satPPM is number 7 and the VGPM is number 8). Similar results were also observed in the PPARR4 model inter-comparison experiments (V. Saba and M. Friedrichs,

personal communication and paper in preparation).

- *Sect. 3.1, P. 3520 line 17: "... the early stratification ... maximize production ..." I don't understand how shallow MLD (and not deep MLD) favours the bloom of diatom. Nutrient is generally maximum when MLD is deep. Please justify or elaborate your statement.*
  Deep mixed layers favour the availability of nutrients at the surface bringing up nutrients from below the pycnocline. However, only when there is sufficient light phytoplankton can grow efficiently, and this is generally found when vertical stratification begins and the MLD shallows.

- *Sect. 3.2: Have the authors look at the regional MEF index between PELAGOS and VGPM as in Fig. 2? It would be interesting to see if the simulated NPP has similar problem in the Southern regions, especially around October.*
  SatPP models as the VGPM are mostly driven by the input chlorophyll data, therefore the performance of PELAGOS against VGPM results is similar to the one shown in Fig. 2 of the manuscript. We include here the results of the requested comparison (Fig. 1, this comment).

- *Sect. 4.2: This study shows the fact that a portion of the modelled NPP is loss to DOC, and needs to be removed when comparing with the observation. Additionally, it also shows that the simulated variability of NPP is determined noticeably by this loss ratio (P. 3524 line 18: "This suggests..."). In my opinion, this is an important findings that can or should be further discussed in the manuscript, even mentioned in the abstract.*
  Also *P. 3536 line 9: "In our specific case..." Does this mean that future measurements of DOC/exudation rate (especially in the oligotrophic areas) are crucial to improve current ecosystem model forecast? If yes, it may be useful to provide some recommendation, or potential strategy to address this problem.*
  We thank the reviewer for this comment that we followed entirely. This finding is

now further explored in the new Discussion section (Sec. 4), which is separated from the conclusions in the revised manuscript. It is also mentioned in the new abstract and recommendations for experimental data collections are included in the conclusions. Concurrent measurement of DOC exudation rates are important to correctly quantify carbon organification and therefore improve model simulations. See also the answer to the related question below.

- *Sect. 5.1: The above parameter is also found to be deterministic in simulating the observed temporal variability (P. 3526 line 23) and that a constant value is insufficient. Is there any other studies who have similar findings (e.g. in situ studies that show this parameter varies with physical conditions)?*
  The quality and quantity of DOC is still not routinely assessed during in situ incubation for primary production studies. Only very recently (and after the submission of our manuscript) a paper comparing the results of 8 different methods of measuring primary production has highlighted the role of dissolved organic matter, which may lead to underestimation of NPP especially in presence of nutrient-stressed cells (Robinson et al., 2009). This feature was related to the composition of phytoplankton (presence of diatoms) but the relationships with physical conditions were not investigated. This reference and comment has been added to the revised manuscript in Sec. 4.2.

- *The PELAGOS seems capable of simulating very well the observed MLD in BATS. Nevertheless, it has trouble getting the right amplitude of chlorophyll variability. I would be interested to know how it simulates the nutrient concentration and compare it to the observation. Table 2 clearly shows that the model significantly underestimate the standard deviation of nutrients, but it is not discussed further in the paper.*
  Sect. 5.2: Discussion on nutrient simulation (similar as above) would be a positive addition to the paper.
  The physical model of PELAGOS simulates the MLD seasonal cycle adequately,

but fails to reproduce the maximum winter values (the slope of the linear test is $\neq 1$ and there is a marked winter bias; this is now written more clearly in the text). Moreover, there is a correlation between the misfits in MLD and NPP during winter ($r = 0.56$). It is likely that the input of preformed nutrients during wintertime is insufficient, although it is not possible to assess this feature because nutrient concentrations in winter are close to zero as shown for instance in Fig. 2 (this comment) for phosphate concentration at BATS. This figure is only commented and not shown in the text to reduce the length of the revised manuscript .Nutrient comparison is still presented as integrated indices in Table 2 but we have now added the following comment in the new Discussion section (Sec. 4.1).

The long-term means of observed nutrient concentrations is an order of magnitude higher than in the model simulations both at BATS and HOT, although all values are already very close to the detectable limits. This is especially found for phosphate, which is reported to be 0 for most of the sampled data in the JGOFS time series. However, the high observed s.d. indicates the presence of nutrient pulses, which at BATS occur during the summer period (not shown).

- *Sect. 6, P 3534 line 7: "It is however clear..." The authors are trying to emphasis (using the modelled bacterial production) that extrapolation of process rate variables could result in misleading general interpretation. This statement, I thought, is one of the main scientific findings from the study. But there is little or no discussion within the paper regarding this issue. The authors should at least introduce past problem or background in the 'Introduction' section. For example: Several studies have pointed out some evidence of biogeographycal provinces and that different ocean regions have different biogeochemical characteristics (i.e. different set of parameters) (Longhurst, 1998: Sarmiento et al., 2004; Hood et al., 2006; Tjiputra et al., 2007)*;

We thank the referee for this comment. We improved the introduction as follows: "Comprehensive biogeochemical models cannot be evaluated for all variables at the global scale, therefore the validity of the underlying biological functional parameterizations can only be tested in certain data-rich regions and with limited ranges of physical conditions. Since several studies have pointed out the existence of biogeographical provinces and that physically distinct oceanic regions have different biogeochemical characteristics (Longhurst, 2007), some specific regional parameterizations might be useful to capture, for instance, the satellite-derived chlorophyll variability (Tjiputra et al., 2007). It is also reported that the extrapolation of observations describing carbon cycle rates which are collected over limited spatial and temporal resolutions may lead to misrepresentation of the microbial processes over the annual scale (e.g. Maixandeau et al., 2005a). This is thus a valid argument for using models of adequate complexity to make this extrapolation, since a properly assessed model is expected to capture the major features of the ocean physical processes and provide a more coeherent (although still approximate) response of the lower trophic levels to these conditions."

- *Sect. 7, P. 3535 line 6: "The bias is further..." This statement is never discussed in the paper earlier. The authors should briefly justify how the usage of adaptive chl:C ratio increase the model bias*
  The sentence has been removed since it required additional explanations that are beyond the scope of the paper. We however added the results of a sensitivity experiment suggested by Ref. #2 that clarify the role of mixing in the creation of the bias.

- *Sect. Appendix A It would be useful for unfamiliar audience to also include the formulation of RMSDcp*
  We have now included the formula of unbiased RMSD and corrected a sign error in the relationship with the bias and total RMSD.

- *Others: Is there any reasons why the authors plot both NPP1 and NPP2 in Fig. 10 but only NPP2 in Fig. 8?*
  There is no particular reason if not to improve the graph readability. The addition of a further set of points hid the improvements of NPP2 in the linear goodness-of-fit plot of Fig. 8a. In HOT this was instrumental to show that the model could not reproduce the observed variability also when considering the total carbon production and that data lie between the two estimates.

All technical corrections and suggestions have been included in the revised text. In particular:

- *Definition of the Subantarctic province.* The Subantarctic province is defined according to Longhurst (1998) as the zone between the sub-tropical convergence around 35°S and the limit of the polar front at about 55°S.
- *P. 3532 line 23: Do the author mean "BCD/NPP" and not "BCD/BP"?*
  Yes, we thank the reviewer for finding this important typos. We mean BCD/NPP as this is the ratio shown in Fig. 13 (now Fig. 12). BCD/BP would give a constant value since bacterial growth efficiency is constant in the model.
- Fig. 6 and 7 have been swapped as requested. The reviewer is right, it was a problem in the final Latex layout.
- The caption in Fig. 7 (former 6) is now completed with information on the specific experiments (NPP1=31; NPP2=32).
- Caption in Fig 12c (former Fig. 13c) has been changed. The map shows the mean value over the period Nov 1991 - Jan 1992.

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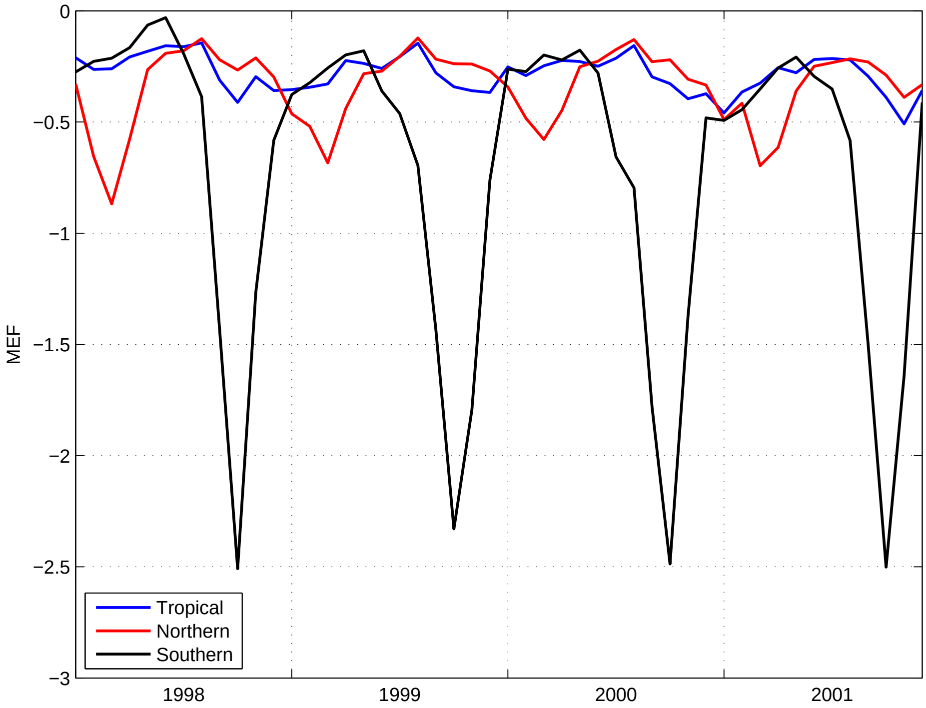

Fig. 1. MEF index for PELAGOS and VGPM primary production data over the period 1998-2001.

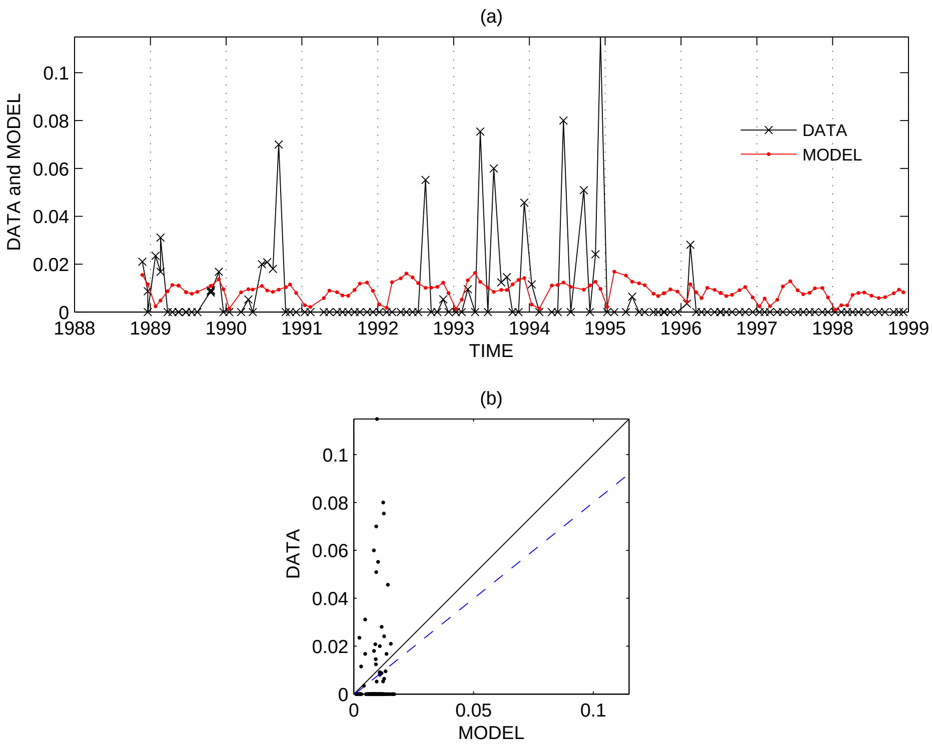

Fig. 2. Comparison of observed and simulated average phosphate concentration in the mixed layer at BATS (in mmol m$ˆ{-3}$) . (a) JGOFS BATS time series; (b) scatter plot