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Ocean Science Discussions

Interactive comment on "Mechanisms controlling primary and new production in a global ecosystem model – Part I: The role of the large-scale upper mixed layer variability" *by* E. E. Popova et al.

E. E. Popova et al.

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We would like to thank referee 1 for the very considerable attention that he/she paid to our paper including the equations, criticism and suggestions, and especially for the comment that "the work emphasises the need to pay particular attention to the parameterisation of mixed layer physics".

Referee 1.

1. The referee says that the "title is inaccurate" (as did referee 3). We have changed the title to remove the word "role" (that implies an analysis of causal factors) to: "Mechanisms controlling primary and new production in a global ecosystem model - Part I: The relationship between biogeochemical cycling and large-scale upper mixed layer

variability".

2. The referee claims that our statement "the work emphasises the need to pay particular attention to the parameterisation of mixed layer physics" is overstated, and that in order to provide proof simulations with different (e.g. simpler) mixed layer schemes would be required. The emphasis of our work is not that any one mixed layer scheme is necessarily better than others but that, having chosen one scheme, it is necessary to carefully calibrate the parameterisation and subsequently analyse results in detail if realistic simulations are to be achieved. In our paper, we are setting an example of how to go about doing the necessary calibration of parameterisations and analysis, and show the benefit that is gained in terms of getting realistic predictions for biogeochemical tracers. We appreciate that this point may not have been emphasized strong enough in the paper so we added the following text into the end of discussion:

"This study does not advocate the use of KPP parameterisations as necessarily the best or indeed the most appropriate for the use in global coupled models. It does however show that it is necessary to carefully calibrate the parameterisations used and subsequently analyse results in detail in order to understand the extent to which ecosystem variability is driven by physics, prior to investigation of purely biological factors."

3. Throughout the paper we use the term Upper Mixed Layer (UML) which, as the referee rightly comments, reflects the layer of active mixing. This layer is the most relevant for the biological activity and this is the layer we diagnose from the model (Appendix 1 and section 2.1). The data used for the comparison (Naval research laboratory mixed layer depth climatology) does however represent the mixing layer (based on the density profile) rather than mixed layer depth, as pointed out by the referee. In this case, we have therefore replaced "mixed layer" by "mixing layer" (p.1073).

4. p.1073 Comparability of the model and database UML depth. The referee makes a valid point in that these two measures of UML depth are different. In our comparison

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we assumed that mixing layer available from the database is a good approximation of the modelled UML. It was of course possible to extract an identical definition from the model, and work with two different estimates, one for the purpose of comparison with data and one for the analysis of biophysical interactions. However we felt that this would unnecessary complicate and lengthen the paper. We have already stated in the text (page 5, bottom of page) why quantitative comparison of modelled UML and NMLD should not be attempted, citing a number of reasons.

5. p.1074 The referee argues that the depths given for winter convection are too shallow in the Labrador Sea. We do not agree with this statement. The depths of convection shown in Fig. 1 are monthly mean values and do not represent instantaneous maximum values, which are much deeper. With respect to comparison of Figure 1 a and b, the UML and NMLD are of the different quantities and, as it was stated both above and in the paper, we suggest only qualitative comparison. The referee also states that the modelled MLDs are too shallow in the Southern Ocean and the Pacific. The same argument applies in these cases as well.

6. p.1078 The referee claims that modelled primary production at BATS and HOT during summer are too low by a factor of 5-10, whereas we wrote that annual production at these sites is low by "a factor of two or so". There is no inconsistency here - our statement is for annual production.

7. p. 1079 The referee asks what the mechanism is for having elevated PP and chl resulting from equatorial currents being "too broad". We have clarified this matter by adding text to the final sentence in the relevant paragraph, which is now: "This discrepancy is because the modelled equatorial currents are too broad and thus produce overestimated lateral fluxes of nutrients."

8. We have deleted the reference to the Lumpkin and Flament (2001) and thus removed the generation of eddy field as a potential explanation for enhanced mixing.

9. p.1090. We fully agree with the suggestion to change "realistic" and "good" to

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"improved", thus recognising imperfection in the results.

10. Appendix B. We have defined the "photic zone" as requested (the top 16 model levels constituting depth of 105m).

Technical corrections:

Abstract l16: changed as suggested

Abstract l17: changed as suggested

Abstract l17-21: "One exception is that the high zooplankton grazing rates required to maintain low chlorophyll in high-nutrient low-chlorophyll and oligotrophic systems lessened agreement between model and data in the northern North Atlantic, where mesozooplankton with lower grazing rates may be dominant." Changed to: "One exception is the northern North Atlantic where lower grazing rates are needed, perhaps related to the dominance of mesozooplankton there."

Abstract l21-22: changed as suggested

p.1068 l7: changed as suggested

p.1073 l.19: "winter convection" was changed to the "depth of the winter convective mixing"

p.1073 l.21: changed as suggested

p.1077 l3: changed as suggested

p.1077 l.5-6 We agree with this comment, however we would like to leave the text unchanged here since our statement is correct for the gyres in general and this will avoid unnecessary long discussion. Discussion of the centre of the gyres is given in the next two paragraphs.

p.1077 l6: changed as suggested

p.1077 l9-13 The northern edge of oligotrophy is not exactly constrained by the Gulf

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Stream, but is rather determined by the intensity of the lateral advection and diffusion of nutrients from the boundary currents towards the centre of the gyres. These discrepancies are the result of overestimated horizontal, rather than vertical, fluxes.

p. 1078 “Some of this difference may be explained by the fact that the model does not contain nitrogen fixation nor eddy-induced horizontal and vertical supply of nutrients.” Changed to “Some of this difference may be explained by the fact that the model does not contain nitrogen fixation nor eddy-induced horizontal and vertical supply of nutrients, nor variable C:N ratio (e.g. Anderson and Pondaven, 2003).”

all comments for pp.1079-1094: changed as suggested

p.1095: the names are added: Rickard, G.J., C.Gordon and A. Pardaens

p. 1096 B2: Brackets are correct, theta and xi changed (typo).

p. 1096 B4: eliminated (tupo)

p.1096 B6: Fasham and Evans (1995); reference is in the text. [J]=1/day, inserted into the table.

p. 1097 B8 N replaced by C (typo)

p.1097 B16: theta removed (typo)

p.1097 B18 lambdaDD eliminated

p. 1097 B20: Eqn changed to: B_N=lambda_A A + mu_D D

Table: value for gamma is inserted mu_e deleted alpha is a function (eq.B8)

Figures in general: the final publication has a portrait layout, which will automatically enlarge all figures. It is rather unfortunate that the journal has a landscape format for the OSD and a portrait for OS. It complicates preparation of the figures since they cannot be optimised for a particular page layout!

Figure 1 Using the same colour bar ranges will mask variability in either data or in

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the model. We would like to keep it as is, since we stressed a need for qualitative comparison in the text.

Figure 4. Lack of UML data is a serious shortcoming of INDIA, however monthly mean values would not make any sense on the figure 4 which presents 6h model output. They cannot be compared.

Figure 2-6; y-labels changed; “depth-averaged Chla-a” is replaced by “surface Chl-a” in captions.

Figure 4; A linear scale (0-1500m) will not show summer variability (0-30m). We would like to keep the existing scale.

Figs.2-6 We think that “OB2” instead of a “numerical experiment” will be confusing.

Referee 3

1. As with referee 1, this referee suggests altering the title of the first paper. We have done so (see reply to referee 1).

2. The referee questions whether “the choice of variables is optimal for a 6-component system”, but without making recommendations for potential improvement. Over and above the “standard” NPZD we use ammonium and chlorophyll. The use of ammonium is widespread in marine ecosystem models, following from the seminal work by Fasham et al (1990). Justification for the inclusion of Chl-a is given in section 2.2.

3. The referee asked us to check our use of word “significant”. We have deleted “significant” on pp. 1074 (l3), 1084 (l20), 1085 (l2).

4. p.1071 l16 We are asked to give details of “weak restoring”. Due to large uncertainties in the freshwater fluxes, a restoring term is included which is derived from the difference between model sea surface salinities and the monthly climatological values from Levitus and Boyer (1994) and the Levitus et al. (1994) climatology. The relaxation coefficent is chosen to provide the equivalent of a 30 day timescale for relaxing the top

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20m. We have altered the last paragraph in section 2.1 to include the text above.

p. 1972 l17 We are asked to provide the time constant for relaxation. The first paragraph of section 2.3 was altered in the following way: “The physical model was spun up for 8 years. This consisted of a 4 year ’robust diagnostic’ integration (relaxation of tracer values towards climatological values at all depths with a 1 year timescale of relaxation) followed by a repeated 4 year period with only surface forcing.”

p.1072 l.25 We are asked to discriminate between trend and interannual variability. We consider a trend as a consistent decline or rise of the annual averaged concentrations while interannual variability is sign-varying. BATS makes an impression of a trend, however the next year after three years presented on the figure 2 had the highest mean annual concentration of nitrate. Thus we stated that “no significant consistent trend in the nitrate field was detected”.

p. 1077,1078 The referee asks how much is speculation and how much is substantiated? On p. 1077 (l9) we have changed “These discrepancies” to “We speculate that these discrepancies”. The remaining statements on pp.1077-1078 refer either to numerical experiments, or part II of the paper, or clearly state that “we speculate”.

Figure captions: We changed “Numerical experiment” to “grazing experiment” as suggested by the referee.

p.1084 l25-28. Data are correct.

Appendix A. We feel that a brief description of KPP is needed to understand a nature of the alteration we made to it.

Appendix B. Equation B7 is correct, it is equivalent of eq. 7 in Taylor et al, 1997 who assumed that R is regulated by the ratio of achieved - to -maximum photosynthesis.

B 18 and B20 are corrected (typo) (see reply to referee 2).

Figure captions for Figs2-6:

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“ Thick” changed to “black”

Interactive comment on Ocean Sci. Discuss., 3, 1065, 2006.