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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.

Anonymous Referee #1

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General Comments

I found this an interesting paper. I like most of it, and agree with its central thesis that improving the physics is a prerequisite to improving the biological model. It is also a neccessary companion to Part II. However there are a number of erroneous statements that need to be straightened out.

Specific Comments

In order of appearance:

The title is inaccurate: only two simulations are presented, the base simulation and one in which a biological parameter has been changed. Thus Part I does not explore "The role of the large-scale upper mixed layer variability", though Part II does. So the title should be changed to something like "...Part I: The base physical-biological simulation" or "Validation of the biological simulation".

In the last sentence of the abstract, and elsewhere, it is claimed "the work emphasises the need to pay particular attention to the parameterization of mixed layer physics". But this is not demonstrated in the paper. To prove it, the authors need to compare simulations with KPP versus a simpler mixed layer and/or vertical mixing schemes. Comparison with previous studies (e.g. Oschlies et al. 2000) is not sufficient, because too many aspects of the model are different (e.g. the bioloical model, the 6-hr forcing).

The authors use the term "UML" instead of "ML", the difference being that the UML is the layer of active mixing, while the ML may contain a remnant layer (or in the case of entrainment, may be slightly less than the UML). However in Sec. 3.1, in their diagnosis of UML from the model and from obervations, they use a density-based criterion, and thus are actually estimating the ML, not the UML (which is defined by turbulence). So, for accuracy, most uses of UML in the paper should be changed to ML.

p 1073, l 24 to p 1074, l 4, "First of all...values." Identical ML depth definitions should have been used, so that the model output could be directly compared with the data-based estimates. E.g. NMLD uses 10 m depth reference, so the model estimate should have also, for this particular comparison. I assume the authors felt the NMLD definition is not the best possible; in this case, they should have presented two estimates of ML, one for direct comparison with NMLD, and one as their "best estimate". In the latter case it would be even better to monthly average instantaneous model values of UML rather than compute it from monthly averages of T and S.

p 1074, l 11, "exceptionally well...reaching 500 m.": This sentence is a little misleading; the locations are reproduced well, but the depths are not e.g. convection in the Labrador Sea should be much deeper (e.g. Lazier et al., 2002, DSR I, p 1819). Similarly, it should be admitted a depth of 180 m in the North Pacific (line 15) is much too shallow. The maximum ML in the Southern Ocean is also too shallow (Fig. 1a versus 1b). A key issue however is that the depth of convection can vary greatly on short timescales; hence a temporal average (like presented in Fig. 1) can underestimate the maximum depth of convection, which I think is what one really wants to examine in order to assess the impact on the maximum winter surface NO3 concentration, and hence new production.

The model's summer ML depths also seem more shallow than observed (Fig. 1c versus 1d). So I am not impressed that KPP is doing a good job at estimating MLD, though this depends greatly on the correctness of the stratification (determined at depth by circulation and mixing rates) in the model. Nevertheless I completely agree with the main point of the paper, which is that it is necessary to get the physical simulation right before tuning the biological simulation.

p 1078, l 14, "two or so lower": AT BATS and HOT, summer PP looks too low by a factor of 5-10 (Figs. 2d and 3d).

p 1079, l 5-6: It is unclear to me how the modelled equatorial currents being "too broad" could result in elevated PP and Chl (Fig. 1g-l). What is the mechanism?

p 1081, l 6-11: Station ALOHA is not in the lee of the Hawaiian Islands; consequently fig. 1 in Lumpkin and Flament (2001) does not indicate the eddy field is enhanced there. Nor is it clear how an enhanced eddy field would effect the depth of winter mixing.

p 1090 l 13-24: There remains much to be improved in the simulation regarding UML and biology (Fig. 1), particularly at BATS (PP), HOT (UML, Chl, PP) and India (Chl, PP, NO3). I am not asking for perfection, just recognition of imperfection. Problems need to be identified, so that we can work toward solutions. So I wouldn't say "realistic" (line 20) or "good" (p 1090 l 22 and p 1091 l 8) but "improved".

Appendix B: What is depth of the "photic zone"? Does it vary in time and/or space? This is a very important to know.

Technical Corrections

Abstract, l 16: "JFOFS" -> "JGOFS"

Abstract, l 17, "Station India": Shouldn't this be "HOT"? Because Station India is where the lower grazing rate is needed.

Abstract, l 17-21, "One exception...dominant." The meaning of this sentence was very unclear, until after I read the paper. Part of the problem is that the previous sentence lists four sites, and then this sentence says one exception is a rate. It would be better to shorten this sentence to "One exception is the northern North Atlantic, where mesozooplankton with lower grazing rates may be dominant" or even better "One exception is the northern North Atlantic, where lower grazing rates are needed, perhaps related to the dominance of mesozooplankton there."

Abstract, l 21-22, "parameterizations were needed" -> "parameterizations are needed", since such a parameterization has not been developed yet.

p 1068, l 7, "primary production" -> "new production"

p 1073, l 19: ML depth cannot be a "proxy" for "convection"; one of these words needs to be changed.

p 1073, l 21: "hence, exert" -> "hence exert"

p 1075, l 8-9: These 1985 and 1987 references are ancient; is there anything more recent?

p 1075, l 20-22, "Other approaches...areas.": Field experiments are restricted to localized areas, but estimates of oxygen utilization/production or nitrate supply/consumption can be applied to global-scale climatologies (e.g. Najjar and Keeling, 2000, GBC, p 573; Schlitzer, 2002, DSR II, p 1623.) Though here the line between data analysis and

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modelling can be vague.

p 1077, l 3, "cyclonic mesoscale eddies": I would just say "mesoscale eddies", since anticyclonic eddies may also be involved in eddy-eddy interactions that involve up-welling.

p 1077, l 5-6, "short-term UML deepening...(PCO6).": Actually PC06 shows that high-frequency forcing is not of major importance to the ecosystem in the center of the subtropical gyres (Sec 3.2.2 in PC06). This is because the nitracline is deeper than the maximum UML there.

p 1077, l 6, "The exact position": "The position"

p 1077, l 9-13, "These discrepancies...resolution.": No, in Fig. 1 the edge of oligotrophy in OB1 goes from Cape Hatteras, dips south, and touches northern Spain, which I doubt is the path of the Gulf Stream in the simulation. Rather, this "edge" of the oligotrophic part of the subtropical gyre is set by whether winter convection reaches the nitracline or not. Winter convection reaches the nitracline well south of the Gulf Stream/NAC (e.g. at BATS). So I think the discrepancies are likely due to a problem with southern limit of winter convection in the northern (convective) part of the subtropical gyres.

p 1078, l 7-17: Another possible reason most 3-D models underestimate PP in oligotrophic areas may be due to C:N decoupling. Models that include C:N decoupling seem to do a good job at reproducing PP (Anderson and Pondaven, 2003, DSR I, p 573; Moore et al., 2002, DSR II, p 403).

p 1079, l 1: "oligotrophic"

p 1079, l 16: "grazing"

p 1080, l 3: eliminate "compared to that in the North Atlanic"; it looks the same in Fig. 1.

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p 1080 l 12: missing ")"

p 1081 l 22: "the only" -> "a"

p 1086, l 18, "for each site": except at Station India.

p 1087, l 28, "in oligotrophic areas (PC06)": "on the edges of the subtropical gyres" would be more accurate, as this is not true in the center of the gyres.

p 1088, l 8, "growth rate": "ingestion rate" might be better, since in the model excretion rate is not proportional to zooplankton growth rate.

p 1089, l 16, "our work here has shown": Actually this is shown in Part II, not Part I. Maybe say "our work (PC06) has shown".

p 1094 l 7: "averages" -> "vertical averages"

p 1094 l 8: "turbulent velocity" -> "turbulent velocity v_t"

p 1095, l 3: since "Rickard et al" is not in the references, full names are needed. Who is Rickard? Who is "et al"?

p 1095, l 7: "mixed layer -> "boundary layer" ?

p 1906, Eq. B2: Should "R" be to the left of the bracket? Shouldn't "thetaˆ-1 xi" be "theta xiˆ-1"?

p 1906, Eq. B4: I think the final "De_z" needs to be eliminated.

p 1906, Eq. B7: Is there a reference that previously used this equation? Either "J" has units "dayˆ-1" or "R" has units "day"; whichever is correct, it should be mentioned in Table 1.

p 1097, Eq B8: "theta = Chl/N" cannot be right, no? In Table 1 it is described as the Chl:C ratio. And "N" is the nitrate state variable.

p 1097, Eq B16: Either "theta" should be removed or "Chl" should be "xiˆ-1 P".

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p 1097, B18: I think the "lambda_D D" should be eliminated, no?

p 1097, B20: "B_P" should be "B_N". "lambda_D" should be "mu_D", no?

Table 1: "gamma" is missing from the Table. "mu_e" is in Table 1, but not in the equations. Some variables are missing their units. Values are missing for constants "alpha" and "delta". There may be more problems; check carefully!

Fig. 1: The colorbar ranges on panel (a) and (b) should be the same. The colorbar ranges on panel (c) and (d) should be the same. The figure is too small; it needs to be bigger, appropriate for a hardcopy.

Fig. 4: Should show data-based estimates of UML.

Fig 2-6: On some of the y-axis labels, "M" should be replaced with greek "mu". Also, is panel (b) depth-averaged Chl (units mg Chl mˆ-3) or depth-integrated Chl (units mg Chl mˆ-2)? If depth-integrated, then the caption and y-axis labels need to be fixed; if depth-averaged, then the caption needs to be fixed. Also, what is the depth of integration/averaging? As it is an acronym, "uml" in the y-axis labels should be capitalized. In Fig. 4, I think a linear scale on the y-axis of UML would be better.

Fig 2-6 Captions and text: For clarity, the "numerical experiment" should be referred to as e.g. experiment OB2.

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