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Interactive comment on "Sensitivity of phytoplankton distributions to vertical mixing along a North Atlantic transect" *by* L. Hahn-Woernle et al.

L. Hahn-Woernle et al.

l.hahn-woernle@uu.nl

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Point by point reply to reviewer #2

24 July 2014

We thank the reviewer for the useful comments on the manuscript. A reply to the issues raised by the reviewer follows below.

Issue Raised:
The article contains three parts. In the first part the authors determine the best parameter sets to reproduce data from three stations along an Atlantic transect. This part is (for me) relatively more important, as it might give a ready to use set of parameters to model different parts of the ocean at different time. The second part contains a sensitivity analysis, which shows the model behavior under different conditions. However this second part raises too many questions. In particular, because it assumes that a species from point A (eg north station) can live at point B (eg southern station) or opposite. However, the stations are located in different ocean provinces, which are characterized by different species (Dutkiewicz et al 2009 Global Biogeochem. Cycles).
Reply:
The reviewer likely has misunderstood the second part of the paper. This tells us that we will have to give a clearer outline of our method in the introduction and abstract. In the second part of the paper, only the vertical mixing is changed in the sensitivity study. The environmental boundary conditions, which are characteristic for the single locations, are not changed. In other words, the nutrient concentration at the bottom and the incoming light intensity remain the same at each location during the analysis. So we do not 'assume that a species from point A can live at point B'. Our focus lies on the effect that a change of vertical mixing can have on the phytoplankton distribution at a given location. The mixing profiles measured along the transect serve as a set of typical mixing scenarios which are likely to occur in the Northern Atlantic and are only displaced from their real measurement location.

Issue Raised:
Furthermore, a quite comprehensive analysis of such models have be done already by Beckmann and Hense (Prog. Oceanogr. 2007), Klausmeier & Litchman (2001 Limnology and Oceanography), and Ryabov et al (J. Theor. Biol. 2010) and it is not clear how much can we gain from this additional analysis.

Reply:
Thank you for the suggestion of the first two papers, which we will mention and discuss in the reviewed version. All these studies (including ours) analyze the sensitivity of the bio-chemical components to changes in single model parameters. The new approach in our work is the direct use of in-situ measurements as system parameters as well as the calibration of model parameters to in-situ measurements, e.g. the chl-*a* concentration it the mixed layer. Furthermore, realistic vertical mixing profiles with a strong vertical variability are applied to the calibrated model. Hence, we study the capabilities of the model to simulate real world situations under natural conditions as well as its sensitivity to changes of these conditions.

Issue Raised:
Finally, the third part represents correlations between some quantities obtained from the model, but there is no comparison with real data. So this part also does not make an effort to justify the chosen set of parameters.
Reply:
The third part is aimed to determine the impact of the shape of the mixing profile on the phytoplankton distributions. As it cannot be decoupled from that of the nutrient profile, we combine this impact by using the nutrient flux, as defined by eq. (8). The impact is shown to be an integral effect where, according to the correlation in Fig. 15, a larger nutrient flux is related to larger phytoplankton concentrations. It makes no sense to compare these results to observations here, as the mixing profiles are displaced from their real location.

Issue Raised:
I would suggest to concentrate more on the fitting of the parameters and leave the sensitivity analysis and the data analysis (eg. Figs. 14,15) for another paper.
Reply:

This suggestion is likely to be connected to the fact that the reviewer apparently did not understand the results of parts 2 and 3 of the paper. These two sections are an essential part of the article, as is explained above. With the appropriate moderations, following the suggestions by reviewer #1 and reviewer #2, we will leave the set up intact.

Issue Raised:
I would like to see a straightforward comparison of the experimental and model results e.g. Fig 5 vs Fig. 10a on the same plot and with the same vertical scale. Maybe all the figures from the three stations together on one plot.
Reply:
The model result is the best fit to the measured profiles based on the given criteria. We will provide a modified Fig. 10 including the observed profile from Fig. 5 to allow for a better comparison. A plot containing the results for all three stations is very unclear so this suggestion will not be followed.

Issue Raised:
I'm a bit suspicious about the use of the normalized difference between model and observations (Eq 6) to fit the model parameters. This quantity will equally assesses a 50% error when the chlorophyll level is low or when it is high, or (if the biomass maximum depth is used) the error at greater depth will have a much less impact than an error at a shallower depth.
Reply:
The sum of squares S is based on different properties in this case the maximum phytoplankton concentration and the depth of this maximum. The order of magnitude of these two quantities differs by 10^7, the dimensions are different and hence the residuals need to be normalized somehow to weigh them equally. A normalization with the observed values is very natural, as when the model values are exactly the

observed values, S = 0.

Issue Raised:
I think to get a robust results, one should use absolute deviations between the model and data. Isn't it better to minimize the absolute difference, e.g.

$$\Delta P = \sum_i ((P_{obs}(z_i) - P_{mod}(z_i))^2), \quad (1)$$

where z_i are equidistant points in the interval of depths from 0 to 100 m and P_{mod} is converted into mg Chl-a/m^3. One can also fit both nutrient and phytoplankton profiles converting the experimental concentrations of Chl-a and model concentrations of phytoplankton into mmol nitrogen/m^3.
Reply:
We also considered a residual like ΔP, based on the direct difference between modeled and observed profiles. But the measured profiles show variations that the idealized model used here cannot reproduce. Fig. 5 shows for example that the phytoplankton concentration is larger than zero close to the surface. The simple growth function in the model restricts growth to very limited regions and such a distribution could not be modeled. Therefore the proposed definition of ΔP would be biased by these values and no useful parameter fits are obtained. Instead we here chose to focus on the major characteristics of the profile to ensure that they are reproduced in the model. This will be explained in more detail in the revision.

Issue Raised:
Fig. 6a. The choice of points (crosses) is not clear. Apparently the lowest level will be for HI = 65 and HN = 0.6 (the deep blue area). However, this point was not considered. Why?
Reply:

The model run with the parameter set (HI=65 and HN=0.6) leads to S = 1.4e-3 while the chosen parameter set (HI=58 and HN = 0.5) leads to S = 5e-5. This will be mentioned explicitly in the revision.

Issue Raised:
Fig 6b, 7b etc. It is not clear how the residuals are calculated. Furthermore, as I understand (I might be wrong) these figures show the relaxation process from some initial conditions to a steady state. Then only the last point makes sense (when this state have been reached), because the rest depends on the initial conditions.
Reply:
In the revised version, we will define a clear criterion for an equilibrium state and will run the model longer such that each solution satisfies this criterion.

Issue Raised:
The effect of the turbulent mixing on the phytoplankton distribution is not discussed sufficiently. What is the new message in comparison with, for instance, (Huisman et al 2006) or (Ryabov et al 2010)?
Reply:
The main result is on the impact of the shape of the mixing profile on the phytoplankton concentrations. Since this effect cannot be decoupled from that of the nutrient profile, we have combined this in the nutrient flux as defined by eq. (8). The impact is shown to be an integral effect where, according to the correlation in Fig. 15, a larger nutrient flux is related to larger phytoplankton concentrations. In the revised version, much more discussion will be provided on the results of Fig. 15.

Issue Raised:
The abbreviation PGM is not common and not necessary.
Reply:

The PGM is basically our own implementation of Ryabov et al. (2010) model. In the revised version, we will refer to it as NP model or simply model.

Issue Raised:
p.841 L.26 background stratification → water stratification
Reply:
We will change the end of the sentence to: " [...] indicates that the vertical mixing processes are not solely controlled by stratification.".

Issue Raised:
P.843 L.1-2 please mention Fig1a.
Reply:
Suggestion will be followed.

Issue Raised:
pP.853. Section 4.2 It's better to consider stationary results. The transient results depend on the initial conditions. When comparing transient results, one must be sure that the observed system and model system start from the same initial conditions. To compare transient results, one could run the model with periodical seasonal variations in light and diffusivity and then compare the results.
Reply:
In the revised version, we will define a clear criterion for an equilibrium state and will run the model longer such that each solution satisfies this criterion.

Issue Raised:
P854 L21... The definition of the residual is not clear. Is this a normalized difference between a real field value and the same value calculated from the model?

Reply:
Yes; this will be further clarified in the text.

Issue Raised:
P854 L25-26 Very close to the results of Beckmann and Hense (Prog. Oceanogr. 2007).
Reply:
The connection to the results in Beckmann and Hense (2007) will be mentioned in the revision.

Issue Raised:
Fig1 a and b should be placed on the left and right panels.
Reply:
Suggestion will be followed.

Issue Raised:
Fig2,3.. remove tick labels from the upper figure. Instead of the caption "Top:Spring, Bottom:Summer" write "Spring" and "Summer" in the title of the corresponding figures.
Reply:
Suggestion will be followed.

Issue Raised:
Consider to exchange Fig 2 and 3. It is more logical fist to speak about phytoplankton distributions, and only then go to some details such as the vertical mixing coefficient.
Reply:
Suggestion will be followed.

Issue Raised:
Fig.5 (right). Was the same dependence of $K_T(z)$ used in the model? If so it would be interesting to see the comparison on the real $K_T(z)$ profile with the nutrient profile.
Reply:
The mixing profile in Fig. 5 (right panel) was used in the model to give the phytoplankton profile in Fig. 10. They will be compared explicitly in the revised paper when Fig. 10 is discussed.

Issue Raised:
Are the figures 6b, 7b, 8b are really necessary?
Reply:
Yes, as they show the equilibration time scale of the model results as well as their sensitivity to the model parameters for the different cases.

Issue Raised:
The comparison of Figs 11, 12, 13 shows that probably there is no unique set of parameters, which would fit to the whole year and to all three stations. The results might also depend on the initial conditions.
Reply:
The reviewer has very likely misunderstood these results as only the vertical mixing profile is changed and not the external conditions. In the revision, the text will be rewritten to more clearly explain the procedure and results.

Issue Raised:
The scatter plots 14 and 15 do not deliver so much information. They are rather about the model behavior, assuming that we use everywhere the same parameters, but why shall we use parameters from the southern station to predict something at the north station? This importance of finding a unique set of parameters which characterizes the

certain region at certain time might be one of the additional messages in the paper.
Reply:
This remark is again connected to the misunderstanding mentioned above.