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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.**

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## Point by point reply to reviewer #1

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:**

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*p. 840, l. 16-p. 841 l. 2: The first paragraph of the introduction is a summary of studies that is not necessary for the manuscript. Instead, give a motivation by indicating why the vertical structure of chlorophyll could be important (e.g. implications for satellite observations and the biological pump).*

**Reply:**

Suggestion followed. The first paragraph of the introduction will be revised to better focus on the main aspect of this study, i.e. the connection between vertical mixing and surface concentrations of Chl *a*.

**Issue Raised:**

*p. 848, l. 19: Why it is important that the PGM is a non-local model? Omit sentence or explain.*

**Reply:**

The sentence will be omitted in the revised paper.

**Issue Raised:**

*p. 849, l. 6: "Initialized" gives the impression Iin would vary with time, but apparently it is kept constant during the simulation. Maybe use "set".*

**Reply:**

Suggestion will be followed.

**Issue Raised:**

*p. 849, l. 10: What are the values of Nb based on?*

**Reply:**

As was explained in the preceding sentence, Nb is determined from the measurements given by Van de Poll et al. (2013). In the revised paper, we will clarify this better.

**Issue Raised:**

*p. 849, l. 13: A time step of 24 h seems very long in comparison to the shortest time scales of the model. Do the results change with a smaller time step?*

**Reply:**

This was an error in the previous version of the manuscript. The NAG D02EJF routine solves stiff first-order ordinary differential equations. The solution is written out every 24h, but the time step for the temporal integration of the ODE's is determined within the routine to guarantee a specific accuracy of the solutions and is much smaller than

24h. We will correct this in the revision.

**Issue Raised:**

*p. 849, l. 23: Since the forcing is constant, the model is not able to represent any transient processes and should therefore run to full equilibrium. Consider omitting second part of the sentence and run the model to full equilibrium.*

**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:**

*p. 850, l. 3: Not clear. When is the model reaching an oscillatory solution? Always with low vertical mixing? Also in the simulations in this manuscript?*

**Reply:**

We will rewrite this paragraph since the oscillatory behavior does not occur in our simulations. It was only mentioned here as an example of the sensitivity of the model to changes in the representation of vertical transport processes (e.g. sinking and turbulent mixing).

**Issue Raised:**

*p. 850, l. 8: What is the difference between the model configuration in the manuscript and the model configuration of Ryabov et al. (2010)?*

**Reply:**

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

**Issue Raised:**

*p. 850, l. 17: It is obvious that the values of the biological parameters affect growth. Omit sentence.*

**Reply:**

Suggestion will be followed.

**Issue Raised:**

*p. 850, l. 18-26: Why does the model only represent a single species? The parameters could be optimized so that it represents a functional group or phytoplankton community. It is obvious that the simplicity of the model itself requires the optimization of the parameters. Consider omitting from line 18, "Many.." to line 26, "measured*

*data".*

**Reply:**

The concept of having just one representative species is indeed better described as a functional group. We will rewrite and substantially shorten this paragraph to provide the correct interpretation of the parameters in the model.

**Issue Raised:**

*p. 851, l. 20: The reader doesn't know what the irradiance is corrected for until reading the appendix. Refer to the appendix when writing about the corrected irradiance.*

**Reply:**

Suggestion will be followed.

**Issue Raised:**

*p. 852, l. 14: No range is given, just a number. Replace "range" by "of around"*

**Reply:**

Suggestion will be followed.

**Issue Raised:**

*p. 852, l. 16: Not clear. Probably "explicitly" is meant instead of "implicitly"?*

**Reply:**

Suggestion will be followed.

**Issue Raised:**

*p. 853, l. 13: Repetition. Omit sentence.*

**Reply:**

Suggestion will be followed.

**Issue Raised:**

*p. 853, l. 14: Why are not all data points used for the calibration? Using the criteria reduces the information used for the parameter optimization. Explain or test if results change when all concentrations are used for the parameter optimization.*

**Reply:**

We also considered a residual that is based on the direct difference between modeled and observed profiles. However, the measured profiles show variations that an idealized model as used here cannot reproduce. For example, Fig. 5 shows 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 a residual based on all data points would be biased by these values and an accurate outcome could not be guaranteed. Instead we here chose to focus on the major characteristics of the profile to ensure that they would be reproduced in the model. This will be explained in the revised version of the paper.

**Issue Raised:**

*p. 854, l. 1. p. 855, l. 1, ..: The list of the meaning of lines in the main text is not necessary and distracting while reading the manuscript, the explanation in the figure captions is enough.*

**Reply:**

Suggestion will be followed.

**Issue Raised:**

*p. 854, l. 14: If the plot is based on the crosses, it seems like S is extrapolated rather than only interpolated and values away from the crosses are highly uncertain. This plot could be left out.*

**Reply:**

Fig.6 (a) actually showed only a part of the region in parameter space and the interpolated surface is based on more model runs than actually shown. However, because the importance of the plot for the paper is questioned by both referees, we have decided to omit it in the revised version.

**Issue Raised:**

*p. 855, l. 22: Explain the consequences and reasons for the different values of S or omit sentence ("Note that, ..."). The reader is left wondering why S is so different.*

**Reply:**

This will be explained in the revised version.

**Issue Raised:**

*p. 856, l. 13: Why is the suite of parameter values tested different for the different stations? A more consistent and informative analysis would have been to use the same. Explain or perform a more consistent sensitivity analysis.*

**Reply:**

At all three stations about 100 parameter sets have been tested. Since boundary and

physical conditions are different at the three reference stations, the resulting best fits are also different. We will add a table in the revised paper showing the range of tested parameter sets.

**Issue Raised:**

*p. 856, l. 26: If a simulation is not in steady state with the best parameter set, consider to run the model longer for all experiments or define a steady state definition that is reached in each simulation.*

**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:**

*p. 858, l. 13: Clarify how the assumption that the total phytoplankton growth is idealized as one species is influencing the choice of k. I suggest taking the mean of the determined values is a consistent approach and doesn't need extra motivation.*

**Reply:**

Suggestion will be followed; we will omit the sentence about the idealization.

**Issue Raised:**

*p. 858, l. 25 - p. 860, l. 12: Why should vertical mixing not play a role in the equilibrium? Maybe it does not in the mixed layer since nutrients are homogeneously distributed but at the depth of the DCM vertical mixing should be crucial due to the gradients of nutrients. The whole section about equation 7 seems redundant because no additional conclusions can be drawn and validity of the equation is questionable. I suggest omitting the section (p. 858, l. 25 - p. 860, l. 12). More interesting are the sensitivity analyses to boundary condition Nb and forcing Iin that follow.*

**Reply:**

The analysis involving eq. (7) is indeed only applicable to the mixed layer where the nutrient distribution is vertically homogeneous. The analysis is capable to explain the relatively high values for HN and hence is valuable to keep in the paper. The application to the DCM is not as straightforward though and hence we will revise this part (p.859, l.24 - p.860 l.18).

**Issue Raised:**

*p. 862, l. 4-5, p. 862 l. 19-20: Move plot description to figure caption.*

**Reply:**

Suggestion will be followed.

**Issue Raised:**

*p. 865-866, How are the results of Ryabov et al. (2010) connected to the results in this manuscript? What is the integrated conclusion from both results?*

**Reply:**

The impact of the shape of the mixing profile cannot be decoupled from that of the nutrient profile and hence we combined this into 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:**

*p. 866, l. 14-20: It is very unfortunate to leave this as the last sentences for the reader. The possibility to infer vertical mixing from chlorophyll distributions could theoretically be possible but the results are not sufficient to show this. The indications of the results on this possibility should not be mentioned at this prominent place at the end of the manuscript and not in this negative way but could rather be mentioned as a side-note.*

**Reply:**

Suggestion will be followed: the last paragraph will be completely rewritten.

**Issue Raised:**

*All Figures: Use actual superscript instead of m3 or m2. For better readability move multiplier to the units instead of the multiplier at the axis, e.g. in figure 5, write [108 cells / m$^3$] instead of 10$^8$ at the axis. Whenever applicable use unit prefixes, kilo, mega, giga, etc. Move the plot descriptions (e.g. top: spring, bottom:summer) to the figure caption.*

**Reply:**

Very good suggestion and we will adapt it.

**Issue Raised:**

*Figure 5, 7a, 8a: In the right panel: Move x axis label to the ticks at the bottom. Consider arranging these plots together in one figure with three plots for better comparability.*

**Reply:**

The first suggestion will be followed but we keep the arrangement of the plots as is because of clarity of presentation.

**Issue Raised:**

*Figure 6a: If the purpose of the plot is only to show the complexity of the calibration it could be left out because the complexity itself is not an interesting result and does not*

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*help to understand the other results.*

**Reply:**

Suggestion will be followed.

**Issue Raised:**

*Figure 6b, 7b, 8b: Extend legend by HI: and HN: for better readability. Consider arranging these plots together in one figure with three plots for better comparability.*

**Reply:**

The first suggestion will be followed but we keep the arrangement of the plots as is, because of clarity of presentation.

**Issue Raised:**

*Figure 9b, 10b: Plot growth limiting factors together in one plot for better comparability.*

**Reply:**

Plot 9a, 10a are already combining three different quantities and adding another would reduce readability. We therefore prefer to keep the growth function separate, but we will plot the growth functions so that they can be better compared.