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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.

Anonymous Referee #2

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The article considers an important problem of parameterization of a simple model based on real data and takes some efforts towards the sensitivity analysis of this parameterization. However, the structure, the presentation and cogency of the arguments are a bit vague and do not allow me to rely upon the article conclusions. To become more convincing the article must be revisited.

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. How-

ever 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). 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. 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.

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.

Below are some of my suggestions.

I would like to see a straight forward comparison of the experimental and model results eg. Fig 5 vs Fig10a on the same plot and with the same vertical scale. Maybe all the figures from the three stations together on one plot.

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. 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))ˆ2),

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/mˆ3. One can also fit both nutrient and phytoplankton profiles converting the experimental concentrations of Chl-a and model concentrations of phytoplankton into mmol nitrogen/mˆ3.

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?

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.

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)?

Finally, English must be essentially improved.

Minor comments

the abbreviation PGM is not common and not necessary.

replace PGM -> our model or our phytoplankton model

p.841 L.26 background stratification -> water stratification

P.843 L.1-2 please mention Fig1a.

P.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.

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?

P854 L25-26 Very close to the results of Beckmann and Hense (Prog. Oceanogr. 2007).

Fig1 a and b should be placed on the left and right panels.

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.

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.

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.

Are the figures 6b, 7b, 8b are really necessary?

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.

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 shell 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.

Finally, I note that I agree with comments of the previous reviewer. Here I just try to avoid repetition.

Interactive comment on Ocean Sci. Discuss., 11, 839, 2014.