Biogeosciences Discuss.,
doi:10.5194/bg-2017-118-RC2, 2017

Biogeosciences Discussions
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EGU

Interactive comment on "Primary production sensitivity to phytoplankton light attenuation parameter increases with transient forcing" *by* Karin F. Kvale and Katrin J. Meissner

Anonymous Referee #2

Received and published: 18 May 2017

General Comments

The manuscript by Kvale and Meissner presents a study exploring the sensitivity of primary production and biogeochemical tracers to the parameter that controls the magnitude of light attenuation by phytoplankton in the Earth System model UVIC. In a steady-state preindustrial simulation the authors demonstrate that primary productivity is relatively insensitive to the choice of parameter value and suggest that low and high latitude productivity respond in different ways to this choice. However the authors then demonstrate that the choice of parameter value leads to significant differences in primary productivity over a transient CO_2 forcing experiment. The authors describe a series of feedbacks between oxygen and the nitrogen cycle that occur with weaker

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self-shading that be important to consider for past changes in ecosystems and oxygenation.

The findings of the manuscript contributes to a recent body of literature on the issues of calibrating biogeochemical models for the preindustrial ocean and the potential for biogeochemical feedbacks in both past and future climate changes. As such, the findings are significant for our understanding of biogeochemistry and are appropriate for the journal. However, I have one key question about the interpretation of the modelling results that needs resolving before recommending publication.

Specific Comments

The authors describe mechanisms for increases in chlorophyll in the Southern Ocean (a weak self-shading effect facilitating greater production) and the increase in the tropics (a strong self-shading effect leading to a decrease in deep photosynthesis and release of nutrients). I think there is an additional factor that has not been discussed which is the change in the distribution of nutrients. The authors describe a general increase in deep ocean concentrations of PO_4 and NO_3 with weaker light attenuation (Section 3.1 and Figure 3) but do not mention the concurrent decrease in deep Atlantic concentrations. This pattern has been observed previously in biological pump sensitivity studies as a result of increased biological pump efficiency sequestering more nutrients in the deep ocean (Kwon & Primeau 2006; section 5.3 of Kriest et al., 2012; DeVries et al., 2012). This leads to a drop in surface nutrients concentrations in the Atlantic which are transported to the deep Atlantic via deep water formation. High production, particularly in the Southern Ocean, during experiment K1 could therefore shift the balance of nutrients towards the deep ocean from the surface ocean driving differences in production elsewhere purely from these changes in nutrient distribution. Additionally, because of the significance of production in the Southern Ocean in the simulations, there needs to be some discussion of the representation of iron limitation in the model. Because of the relevance of these mechanisms throughout the manuscript, this additional factor needs to be included and preferably quantified in the

manuscript.

The manuscript would benefit from a minor restructuring. The last section of the Methods would be better suited at the end of the Introduction to give a fuller background and to complement the description of the more complex parameterisations. The Discussion also needs to include some caveats/limitations of the study such as whether these results model dependent, whether the nutrient feedback mechanism is a result of using the more simplified parameterisation and what differences one might expect if using the more complex parameterisation.

Technical Comments

Page 2, lines 5-25: this discussion of inherent optical properties is interesting but given the focus of the manuscript on the sensitivity of the simpler parameterisation this needs to be integrated better. I suggest at least revisiting these points in the discussion and commenting how the use of inherent properties might alter the results of the manuscript.

Page 2, line 32: I'm not sure what non-algal particles are or where they are derived from, a small description would be useful.

Page 3, line 1: if possible, could you provide some quantitative estimates of production variability when changing other parameters for comparison?

Page 4, line 2: "probably derive" is odd terminology to use here, either state that it is derived from Fasham or remove the mention to Fasham.

Page 4, lines 1-15: some of the text describing the range of parameter values and their assumptions would be better placed towards the end of the introduction after the description of inherent versus apparent optical properties. This would then serve as a good justification for exploring the sensitivity of model to the parameter value following the discussion of inherent optical properties but which are computationally more expensive.

Page 4, line 15: it would help for clarity to explicitly reiterate here that increasing values of Kc represent increasing attenuation of light with phytoplankton biomass and provide a brief description of the experiments including what aspects of sensitivity you are considering, e.g., sensitivity of productivity and biogeochemical tracers.

Page 4, line 31: Kim et al., (2015) find this effect when testing the light attenuation by CDOM rather than phytoplankton biomass. Are these two parameterisations directly comparable? For example, concentrations of CDOM and biomass might respond differently to stratification and therefore affect attenuation differently?

Page 5, lines 3-6: see specific comments, this needs a reference to tracers in the deep Atlantic.

Page 5, lines 10-13: I appreciate the aim is not to find the best parameter value but it would be useful to state the RMSE for the global tracers, and maybe at the basin-scale too, as it would put later results in context (e.g., page 6 line 13, page 7 line 10 18) and allow comparison against other sensitivity studies such as Kriest et al., (2012).

Page 7, lines 25 – 27: It is not clear what “evolutionary trend in light attenuation characteristics by dominant phytoplankton” refers to. I suggest being explicit about which trends in phytoplankton the authors are referring to (i.e., changes in size, appearances of dominant groups such as calcifiers and diatoms). I am not sure that ”evolutionary trend“ is appropriate here as this is not a specific trait of the individual organisms themselves. The mention of rapid climate change can also be given more context by citing the Paleo-Eocene Thermal Maximum for example (e.g., Norris et al., 2013).

Figure 3: I find interpretation of this figure difficult because the difference from observations is plotted and therefore includes some structural model error as well as differences from the parameter choice. Plotting the actual profiles, as per Figure 6, might be easier to interpret and allow for direct comparison with Figure 6. The legend is also very small and hard to relate to panels in the far bottom right corner. A graded continual colourscale, rather than different discrete colours, would also help for all plots

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with K1 to K8 variability (I also find it hard to distinguish some of these colours when they are next to each other on the plot).

Figure 7: please clarify explicitly that the difference plots are K1 – K8 in the figure caption.

References

DeVries, T. and Primeau, F. and Deutsch, C., (2012) The sequestration efficiency of the biological pump. Geophysical Research Letters. 39 (13), L13601

Kriest, I. and Oschlies, A. and Khatiwala, S. (2012) Sensitivity analysis of simple global marine biogeochemical models. Global Biogeochemical Cycles. 26 (2), GB2029

Kwon, Eun Young and Primeau, Francois. (2006) Optimization and sensitivity study of a biogeochemistry ocean model using an implicit solver and in situ phosphate data. Global Biogeochemical Cycles. 20 (4), GB4009

Norris, R.D. and Kirtland Turner, S. and Hull, P.M. and Ridgwell, A (2013) Marine ecosystem responses to Cenozoic global change. Science. 3541 (492).

Interactive comment on Biogeosciences Discuss., doi:10.5194/bg-2017-118, 2017.

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