The authors would like to thank the reviewer for their thoughtful comments and their effort in helping us to improve our manuscript. This latest version represents a substantial revision of the original document, particularly of the Results and Discussion sections. Reviewer comments are shown in black font. Our response is shown in blue font. Changes to the text are shown in red font.

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 CO2 forcing experiment. The authors describe a series of feedbacks between oxygen and the nitrogen cycle that occur with weaker 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 PO4 and NO3 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.

Reference to the named manuscripts has now been included to the paper. The results section now includes mention of this deep ocean /Atlantic relationship (p5 line 34):

Carbon and nutrient distributions in the UVic ESCM are also sensitive to k_c because parameter choice affects the efficiency of the biological pump (Fig. 1), leading to a redistribution of nutrients (Fig. 4). Low-value k_c simulations experience a greater proportion of global NPP in the high latitudes (regions with higher sequestration efficiency; DeVries et al. 2012), and increasing the k_c value shifts NPP towards the tropics (a region of lower sequestration efficiency; DeVries et al. 2012). As a consequence, more nutrients and carbon end up in the abyssal Pacific Ocean in low-value k_c simulations than in higher value ones. Increased storage of nutrients in this deep ocean basin reduces the inventory available for subduction in the northern Atlantic (e.g., Kwon and Primeau, 2006; Kwon_et al., 2009; Kriest et al., 2012), where water column concentrations of nitrate and phosphate decline (Fig. 4).

Iron limitation is now treated to a closer examination. How it is implemented is now mentioned in the Methods section (p4, line 15):

Iron limitation is accounted for using a seasonally variable mask of dissolved iron concentrations in the upper three ocean layers (Keller et al., 2012).

Growth limitation maps have been introduced as a new figure (Figure 3). Iron is not limiting in this model on an annual-mean basis, though it is represented. This is discussed (p 10, line 3):

Though iron availability is accounted for in the form of a seasonally-variable mask, in our model iron is not a limiting nutrient on an annually-averaged basis. This is in contrast to evidence of iron limitation in the Southern Ocean, North Atlantic, and eastern boundary currents and upwelling systems (see recent review by Tagliabue et al., 2017). More iron limitation of phytoplankton growth in the UVic ESCM might damp the NPP response we show for lower light attenuation simulations in the Southern Ocean and eastern equatorial Pacific. More iron limitation might also mitigate differences in the efficiency of the global biological pump between high and low-value light attenuation parameter simulations. Higher NPP in the high latitudes in low-value light attenuation parameter simulations results in more efficient export and storage of nutrients in the deep ocean, particularly the abyssal north Pacific (also found by DeVries et al. 2012). Model phosphate is conserved in our simulations, thus larger deep ocean inventories result in lower concentrations in downstream surface and intermediate waters (in qualitative agreement with Kwon and Primeau 2006; Kriest et al. 2012). The effect of enhanced deep nutrient sequestration is most apparent in Atlantic phosphate and nitrate profiles, where concentrations are lower for lower kc simulations and NPP is not very much higher at the surface, in spite of being a seasonally well-mixed region. If iron was more limiting in the Southern Ocean deep water formation regions, fewer nutrients would be sequestered in the deep Pacific and more would be available to the north Atlantic, raising regional primary production and export (assuming no iron limitation also existed in the north Atlantic). More iron limitation in the low latitudes might furthermore damp the NPP response of higher kc simulations in the thermally stratified tropics, thus increasing nutrient transport poleward and increasing high latitude NPP.

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.
The last paragraph of the Methods section has been moved to the end of the Introduction. The Discussion section has been rewritten to include more limitations and greater context with earlier work (p9 to p12).

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.
The Discussion section has been substantially rewritten to include more limitations and greater context with earlier work.

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.
"Non-algal" has been replaced with "detrital" throughout the paper.

Page 3, line 1: if possible, could you provide some quantitative estimates of production variability when changing other parameters for comparison?
The sentence is modified as:
These are modest changes with respect to other production and export parameters (e.g., Kwon et al. 2009 found a 5 Gt C y^{-1}, or 50%, increase in global carbon export by raising the export transfer efficiency exponent by 0.4), though regional sensitivities are stronger (Kim et al., 2015).

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.
Reference to Fasham is removed.

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.
This paragraph has been moved to the Introduction.

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.
The passage is modified as:
For our test, we employ eight separate simulations using $k_c = 0.01, 0.02, 0.03, 0.04, 0.05, 0.06, 0.07$, and 0.08 (m mmol N $m^{-3})^{-1}$. Increasing the light attenuation parameter value increases the self-shading effect of the phytoplankton biomass, reducing the amount of light available for photosynthesis. In the following analysis they will be referred to as ‘K1-8’, as we assess the impact of parameter choice on model net primary production, carbon and nutrient distributions in a model equilibrated to pre-industrial climate conditions and then forced with historical and projected greenhouse gas concentrations.

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?
The sentence is modified to read:
In the more stratified (and nutrient-limited) tropics, the effect is opposite in that K8 yields zonally averaged NPP of up to double K1 because stronger self-shading inhibits deeper photosynthesis (see the globally averaged NPP depth profile plot in Fig. 1, which is dominated by the low latitude response), making more nutrients available at the surface (Figs. 2 and 4, and similar to the effect of light attenuation by CDOM described previously by Kim et al. 2015).

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

The paragraph edits are given under "Specific Comments"

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).

RMSE are now included in the figure.

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 trendKw 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).

"Trend" is modified to "increase".

Norris et al (2013) is now cited (p11, line 13):

... the Southern Ocean and the low latitude Pacific in the real world, and 2) light attenuation characteristics of dominant phytoplankton (Katz et al., 2004) and ocean oxygen content (Lenton et al., 2014) and rates of change (e.g., Paleocene Eocene Thermal Maximum Norris et al., 2013) have changed over geologic timescales.

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 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 3 has been revised to actual profiles, global RMSE with observations, and a larger legend.

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

Figure 7 is removed.

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).