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Interactive comment on "Climate engineering and the ocean: effects on biogeochemistry and primary production" *by* Siv K. Lauvset et al.

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The manuscript by Lauvset at al. analyses the effects of three proposed solar radiation schemes for geo-engineering on ocean carbon cycling (CC) and net primary productivity (NPP), using a fully coupled earth system model which includes an aerosol and a radiation scheme, a description of atmospheric and oceanic circulation, and land and ocean biogeochemical models. The question investigated is highly relevant, both for understanding possible feedbacks in the system (changes in radiative climate forcing incurred by changes in oceanic carbon uptake) and for possible effects of (engineered or un-engineered) climate change on food security: primary production of the ocean can serve as a (admittedly crude) measure of possible fisheries yields. Three geoengineering schemes, all affecting the radiation balance, two mainly on the

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incoming shortwave radiation, and the third mainly on the outgoing long-wave radiation are applied in this study, in such a way that globally they all lead to a reduction of the radiative flux by 4 W m2, bringing the radiative forcing of the RCP8.5-scenario down to that of RCP4.5. In addition to these coupled model runs, the manuscript uses offline calculations to investigate which factors drive changes in NPP. These help in interpreting the results, but as outlined further below I have some issues with the methodology here. Overall, this is a well thought-through study, the results are relevant, and the manuscript is besides some minor points very well written. I would therefore support publication in Biogeosciences after addressing the points listed below.

Major comments

The description of the offline calculations (lines 139 ff) is missing important information, and also some justification. To me it is not clear at all to which equations the expression 'makes use of the same set of equations as the online calculation' (line 141) refer to: Does the offline model consider three-dimensional transport (advection and diffusion) of the non-prescribed equations? Which equations exactly are those?

We thank the reviewer for pointing out that our description of this method was unclear. Upon rereading we realize that it sounds like we have used an offline model, but this is not the case. We have merely performed a simple offline calculation using the output from the NorESM1-ME model. We took the monthly three-dimensional model output (x,y,depth) and put it into Equations 1-3 (in the revised version) to solve for NPP. We assumed a constant euphotic depth of 100m and therefore averaged the inventory over the top 100m for nitrate, phosphate, and dissolved iron to calculate the limiting nutrient in each month. We also used the average temperature in the top 100m and light was attenuated to 50m (in the middle of our depth layer). There are no other equations in our offline calculation than Equations 1-3. The text has been revised to clarify this method.

Why is the light in the offline calculations attenuated to a constant depth of 50 m, is the offline model two- dimensional or does it resolve depth?
No, we do not resolve depth. We calculate a value for NPP in the top 100m of the ocean and assume that the light at 50m is a good approximation of average light concentration over the 100m layer.

One issue that I found particularly confusing in the description of the offline experiments is that N stands for the most-limiting nutrient (phosphate/nitrate/iron). But which nutrient is most limiting is likely to change in the online runs. Are all nutrients prescribed in the offline runs, is there a climatology of the most limiting nutrient?
In the offline calculation, the most limiting nutrient is computed based on the monthly outputs of nitrate, phosphate, and dissolved iron concentrations. See also our reply above.

I also have a similar problem with the interpretation of the results of the offline calculations as the first reviewer. The authors use phytoplankton biomass as proxy for assessing the impact of changes in nutrient supply to the euphotic zone due to changes in upper ocean stratification (lines 363-364). What one would really like to use as a control variable in these calculations is the vertical flux of nutrients. I see that nutrient concentrations are probably not a good tracer for this nutrient flux, since they are drawn down to limiting values (assuming sufficient light) regardless of the flux. But the phytoplankton biomass is also just an indirect indicator: Firstly it is also affected by other losses such as zooplankton grazing (as the authors also mention, line 366), to which I would add the sinking losses of biomass through aggregation and sinking: Assume that the only loss of phytoplankton was a quadratic loss through aggregation and sinking. Then biomass would be proportional to the square root of nutrient supply.
The reviewer is correct and these are very good points. As explained in our

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reply to reviwer #1 we now calculate a residual term which approximately represents the integrated circulation-induced changes in phytoplankton and limiting nutrient. To a first order this term thus includes the advection of nutrients. The discussion is revised to reflect this. Unfortunately, the vertical fluxes of nutrients are not available as model outputs. And since the ocean model is based on isopycnic vertical coordinates, the computation of surface-deep exchange of nutrients is not straightforward.

Also, phytoplankton growth rate is affected by both nutrients and temperature, which however is considered as a separate driver. To me it is thus nor completely clear how well these two factors can be separated with the offline experiments.
This point was also raised by reviewer #1. We agree that the presentation of NPP variation due to changes in phytoplankton was confusing. We now only compute the total, temperature- and light-induced NPP variability, and discuss the residual. The residual term predominantly represents the NPP change due to circulation-induced changes in nutrient and phytoplankton. See also our reply to reviewer #1.

A smaller question that I didn't find the answer to in the model description (lines 129-138), and that may affect the interpretation of the manuscript slightly, is whether the model considers direct effects of ocean acidification (line 536) on carbon cycling through the marine ecosystem, e.g. by reductions in calcification.
No. In the HAMOCC model, calcification is indirectly determined by the silicate availability. In regions of high silicate, biogenic opal production dominates, and when silicate is low, calcium carbonate production dominates. In the interior ocean, ocean acidification induced changes in carbonate ion saturation governs the dissolution rate of calcium carbonate.

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Also, the description of how the different RM methods have been implemented in the model (Lines 163-173) is quite short: to me it was for example a bit unclear how the SAI scenario was modelled. It is said that a layer of sulfate aerosols was prescribed, but then the next sentence states an injection strength, which to me implies that the layer was not prescribed, but calculated as resulting from a balance between injection and some unclear losses.
The description of the implementation of the RM methods has been clarified. We prescribed a layer in the stratosphere with optical properties representing an injection strength of 20 Tg(S) per year in year 2100, to offset -4.0 W m-2. The aerosol layer was represented by stratospheric zonal aerosol extinction, single scattering albedo and asymmetry factors, as derived from the Tilmes et al. (2015) data set.

Minor comments

Line 42: At least the CCT method does not act to 'increase the amount of solar radiation reflected' but rather to increase the loss of long-wave radiation passing through the atmosphere.
This is true, and is the reason for our definition and use of the term Radiation Management (RM) on line 65.

Line 66 ff: I found this sentence quite confusing: Is it maybe two sentences in one?
The sentence is revised for clarity and now reads: "As pointed out by Irvine et al. (2016) there are several gaps in the research on the impact of RM on both global climate and the global environment, especially considering that only a few modelling studies to date systematically compare multiple RM methods."

Line 100: contrary to the statement on line 100 I have not found any presentation of impacts on inorganic carbon in the manuscript, only impacts on air-sea carbon flux. They are of course closely related, but be precise.

The reviewer are correct that we do not discuss changes in inorganic carbon content by itself, but we do discuss changes in pH as well as air-sea fluxes, which is a part of the inorganic carbon cycle. We agree this was unclear and now state that we look at changes in the inorganic carbon cycle (which is also the title of section 3.2).

Line 138: It is stated that seawater carbonate chemistry formulation follows the OCMIP protocol. But which one, OCMIP 2 or 3? OCMIP 3 corrected a few smaller errors in the OCMIP 2 protocols.
The model uses the OCMIP2 protocols and this is now reflected in the text.

Line 223-225: This result could be emphasised a bit more, it shows why we need full coupled atmosphere-ocean-biogeochemistry models to study this type of effects
It is indeed important to use full Earth system models to address the climate responses and implications of RM scenarios. The so-called “monsoon-like” response to the tropical and extra-tropical circulation as a result of the MSB forcing has been discussed in several papers before, and we will hence not spend too much time on it in this paper.

Line 297: ’production’ missing after ’increasing primary’
This is now changed, and primary production is replaced with NPP throughout.

Line 299-300: ’after termination it takes less than 5 years’: What sets the timescale, the atmosphere (radiation), or the ocean biology?
This timescale is set by the atmosphere. The ocean biology reacts to the (very) fast atmospheric response to termination of RM. We have added a sentence reflecting this.

Line 327: ’Only CCT significantly changes..’: Does that not contradict what has been

said before? Maybe I did not understand what should be said here.
This section discusses the offline calculations only, the results of which differ somewhat from the model experiment. The sentence is revised to clarify this and now reads: "For the top 100 m of the ocean, the offline calculation shows that only CCT significantly changes NPP compared to RCP8.5."

Line 336-337: insert 'the' in 'once terminated, CCT method..' **Done**

Line 441: Is 18 percent really a 'minor change' compared to 13 percent?
Considering the uncertainties in NPP change I'd say these numbers are very similar. However, I agree with the reviewer that the statement may be misleading so "marginally" has been removed.

Line 447 ff: This and the next paragraph talk about reduction on NPP; it would be clearer if the percent changes would therefore have a negative sign also. **That is true and the paragraphs have been changed accordingly.**

Line 477: 'are quite different': It would be good to have a short summary of the differences, so the reader does not have to read Partanen et al. (2016) herself.
This is a good suggestion from the reviewer and we have now added a brief description of the major differences between our results and those of Partanen et al (2016), as follows "Overall, the effects of MSB in this study and that of Partanen et al. (2016) are quite different. Spatially, Partanen et al. (2016) sees a very strong correlation between the regions where the MSB forcing was applied and the regions of strongest NPP change which is not apparent in this study. Temporally, the change in NPP in Partanen et al. (2016) comes in form of a relatively rapid decrease over the first ten years MSB is applied while in this study the change is more even throughout the period of MSB forcing."

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Line 563 ff, references: It the Ahlm paper still in the discussion forum or is there a citable full reference by now?

A revision has been in review and is now accepted with minor revisions.

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