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Discussions

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

Siv K. Lauvset et al.

siv.lauvset@uib.no

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General comments
"Climate engineering and the ocean: effects on biogeochemistry and primary production" by Lauvset et al. provides a single-model assessment how three different climate engineering methods (stratospheric aerosol injections, marine sky brightening and cirrus cloud thinning) affect ocean biogeochemistry. This is one of the first studies on the topic and comparing different methods within the same model is a valuable addition to previous works. They concentrate on four key variables in ocean biogeochemistry: sea surface temperature, oxygen, pH and net primary production. For NPP, they complement the interactive Earth System Model simulations with offline calculations that make possible to disentangle different drivers of NPP change. This method adds

to the value of the manuscript, although I have some concerns and questions about the method (see specific comments). The manuscript is mostly clearly structured and written, and thus easy to read. However, some more commas when dependent clauses start sentences would enhance readability. For example, I would insert a comma in "When only phytoplankton concentration is allowed to vary temporally in the offline calculation there is a decrease of $\tilde{8}$% by 2100 in RCP8.5." (Lines 369-371) and similar sentences. Also, the use of present tense throughout the manuscript differs from the general practice of using past tense to describe the results and methods. Overall, I would recommend this manuscript for publication if my comments below are adequately addressed.

Thank you for this nice summary and comments about the manuscript. Since the results and discussion are combined into one section we feel that present tense is the most appropriate. The tense has been changed in the methods section.

Major comments

The offline model for NPP calculations needs more precise explanation and evaluation. In Lines 139-149, you imply that monthly-mean values are used for nutrients. On the other hand, on Lines 362-364 you write that phytoplankton concentration is used as a proxy for nutrient availability. Moreover, on Line 417, phytoplankton concentration is said to be a proxy for circulation changes. The last two statements are in my understanding consistent with each other (but it would be good to explain explicitly why they are related), but please clarify how the first statement of monthly-mean nutrient fields should be understood.

Upon rereading these sections we see that our description of both the method and the interpretation of results could have been better. We believe some of the confusion comes from the difference between phytoplankton growth rate and primary production, and the text has been revised to clarify this. The growth rate of phytoplankton is a function of temperature, light, and the concentration

of the limiting nutrient (in our case either nitrate, phosphate, or dissolved iron). The growth rate is expressed as the first two terms in Equation 1 in the original paper [r(T,L)*(N/(N+N0)]. In this equation, monthly mean nutrient data from the model are used. As is seen from this formulation, any change in the limiting nutrient has a very small impact on the growth rate. NPP is the growth rate multiplied by the phytoplankton biomass (expressed as a concentration), i.e. Equation 1 in entirety. To help clarify this we have, in the revised paper, split Equation 1 into one equation for growth rate and one for NPP.

Also, doesn't NPP significantly affect phytoplankton concentration? Using phytoplankton concentration to calculate NPP sounds circular reasoning to me and I see a risk that the method overestimates the contribution of circulation changes to NPP changes. For example, if temperature increased phytoplankton in the online simulations and this in turn increases NPP in offline calculations, don't you attribute this increase to circulation in the offline calculations instead of to temperature?

The reviewer is correct, and we appreciate this being pointed out. As described above, NPP is driven by temperature, light, nutrient and phytoplankton concentrations. Since the last two drivers depend on each other, in the revised manuscript, we have quantified the changes in NPP (i.e., through the offline calculation) due to changes in temperature, light, and residual parameters. The residual term is approximately represents an integrated circulation-induced changes in phytoplankton and limiting nutrient as described in the revised manuscript. We believe this will avoid confusions on the 'circular effects' as the reviewer pointed out.

I think it would also be good to provide some short evaluation of the offline NPP calculation method to show whether it provides similar results as the online calculation. The value of offline calculations is to disentangle different drivers of NPP change, but how well does the offline version compare to online version when all drivers are accounted for (both regionally and at global mean level)? Specifically, comparing Fig. 5 to Fig. 7a would be helpful.

We agree with the reviewer that comparing the offline NPP with the online is useful. Given the method used to calculate NPP offline (see my reply above) we expect there to be some differences between the offline and online estimates. Fig 1 below shows a comparison between the 2006-2020 NPP in the model and the 2006-2020 NPP calculated offline. In 2020, the offline global average NPP is 75In the five regions we discuss in more depth the percent change in 2071-2100 relative to 1971-2000 differs by 1-9% between online and offline NPP. A new figure, Figure 8, identical to Figure 6 but plotted using the offline NPP, has been added to the manuscript. Some text has also been added to clarify these differences and make clear throughout the discussion where NPP is being discussed.

Minor comments

Lines 20-22: If the drivers of NPP are "partly" affecting the inhomogeneity of the NPP changes, what is responsible for the rest of the inhomogeneity?

We agree with the reviewer that this sentence was unclear and have revised to: "The spatially inhomogeneous changes in ocean NPP are related to the simulated spatial change in the NPP drivers (incoming radiation, temperature, availability of nutrients, and phytoplankton) depending in the RM methods." In addition, we have added some text with concrete examples of how the different RM methods affect NPP differently.

Line 93: Spell out SST as it's used here for the first time. **Done**

Line 118 and throughout the manuscript: You apparently use NPP and primary production interchangeably. I would recommend using NPP (shorter and more precise) everywhere consistently or explain if there is some subtle difference between NPP and primary production in the manuscript. **Done**

Line 165: I think it would more precise to say that you scaled AOD to match the level of a 20 TgS/year injection as you don't explicitly model the aerosol injection here. **The text has now been clarified to: "As the NorESM1-M model does not include an interactive aerosol scheme in the stratosphere, the dataset of Tilmes et al. (2015) was used. The stratospheric zonal aerosol extinction, single scattering albedo and asymmetry factors resulting from SO2 injections in the tropics were prescribed such that the prescribed aerosol layer in year 2100 corresponds to an SO2 injection strength of 40 Tg yr-1 (Muri et al. 2017)."**

Line 172: Maybe good to say here explicitly that the other two methods had -4.0 W m-2 forcing. **Done**

Line 193: SST should be defined on Line 93 already. Maybe not necessary to repeat it here. **Done**

Lines 207-209: You use a high emission scenario. I would add that RM does not prevent long-term impacts in a scenario where CO2 emissions don't go to net zero. If they did, the situation would probably look a lot different. **Done.**

Lines 230-232: Are there many areas where changes are greater with RM than without? If the results in RCP8.5 with RM are spatially highly variable, the changes can't be attributed to RM.
We are unsure what the reviewer asks here since Figures 2, 3, and 6 all show the spatial variability in changes incurred by adding RM to RCP8.5. As shown

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in Figures 2 and 3, RM induced changes are always smaller, or in a few cases in the opposite direction, than the results in the RCP8.5 reference simulation. We have rephrased "(...) possibly lead to new and detrimental (...)" to now read "(...) still lead to similar albeit weaker detrimental (...)"

Lines 291-292: I'm not sure what this sentence means. What is smaller than in RCP8.5? The exhibited decrease of NPP or the changes in NPP in RM simulations? Please, clarify.
The temporal decrease in global ocean NPP is smaller in experiments with RM than in RCP8.5. The sentence has been rewritten for clarity and now reads: "All RM methods also exhibit decreases in ocean NPP, but the decrease is never as strong as that in RCP8.5."

Line 332-334: Isn't the increase in NPP with CCT only present in offline calculations? In Fig. 5, NPP decreases in all simulations, and I think the online calculations are more reliable.
Yes, this is present only in the offline calculations and it is right that the online calculations are more "correct". However, on lines X-Y (previously 332-334) it is the results from the offline calculation that are being discussed. This is now clarified in the text which now reads: "In fact, CCT results in an increased productivity by 2100 (Figure 7a) in the offline calculation". While we agree that this statement was misplaced, we maintain that the effect of CCT on NPP is an interesting result and have moved this discussion to section 3.3.

Line 363: As discussed earlier, please explain here or elsewhere what you mean by using phytoplankton as a proxy for nutrient availability. **See earlier reply.**

Line 378: Is this section based on online of offline NPP calculations? If you use only offline calculations, could you provide some evaluation how well the offline results match the online results at regional level?

Since the online NPP cannot be decomposed into its individual drives this section is based entirely on the offline calculations. This is clarified in the text, which now reads "For a more detailed analysis, five regions have been identified and analyzed based on the offline calculations of NPP and its drivers." We have evaluated how the offline calculated NPP compares to the online model output. Depending on region, the total percent change in 2071-2100 relative to 1971-2000 differs by 1-9% between online and offline. The online change is higher in 3 of the 5 regions, while offline changes are higher in the remaining 2 regions. The new Figure 8 allows for comparison between the spatial variations of the online and offline NPP.

Line 388-390: What do you exactly mean by being consistent with CMIP5? Consistent with the sign of model ensemble mean or do all CMIP5 models give the same sign for these regions?

Our results are consistent with the CMIP5 model ensemble mean. This has been clarified in the text.

Lines 403-409. Why higher NPP would not lead to higher fish catches but lower NPP would decrease fish catches? Is this based on some dynamics of the ecosystem or are you just more careful to predict any increases than to predict decreases?

NPP is the building block of the food web. It is therefore straight forward to predict that if this decreases there is less food for all higher trophic levels. It is not, however, as straight forward to predict what happens to higher tropic levels if NPP increases. In addition, higher tropic levels in the ocean is more than just fish. We have reworded this section for clarity, and added the following

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statement: “The IPCC-AR5 states that due to lack of consistent observations it remains uncertain how the future changes in marine ecosystem drivers (like productivity, acidification, and oxygen concentrations) will alter the higher trophic levels (Pörtner et al., 2014).”

Lines 411-414: Splitting this to several sentences would make it easier to understand. Also, “do” on Line 413 seems redundant. **Done**

Line 422: I don’t understand what you mean by “Radiation changes become more important in driving changes with RM”.
The reviewer is correct that this was a poorly worded sentence. The sentence is now revised for clarity and reads: “When RM is applied, shortwave radiation changes at the surface become more important in driving NPP changes than they are in RCP8.5 and RCP4.5”.

Line 463: Why is this unusual? Compared to what? Doesn’t increased temperature lead to increased NPP in other regions as well?
The unusual part is how large the temperature component is. The sentence has been revised for clarity and now reads: “The temperature changes lead to an unusually large, compared to other regions, increase in ocean NPP of 4% in 2121-2150 in all experiments.”

Line 467: Considering the low number of previous studies on the topic, could you write something about the results of Hardman-Mountford et al (2013) that you mention in the introduction? I know that comparing an ESM to single-column model is challenging, but it would be interesting to know how the results compare.
A brief description of the Hardman-Mountford et al (2013) results and how they compare with our study has been added at the beginning of section 3.6 (before the comparison with Partanen et al (2016)).

Lines 494-497: I would add here that the potential interaction of SST and the clouds is missing in Partanen et al. (2016). Their forcing is calculated with an AGCM that has a fully interactive aerosol scheme and takes thus into account interactions with clouds and sea salt aerosol, but with prescribed SST, the model might miss some relevant feedbacks.
Thank you for pointing this out. A comment on this has been added: "Partanen et al. (2016) take their SRM forcing from Partanen et al. (2012), which use an atmosphere only version of their model and hence neglect important feedbacks, including SST/ocean feedbacks. Partanen et al. (2016) furthermore prescribe their SRM forcing in terms of changes to the radiation, and hence miss out on further feedbacks, that we include in our fully coupled Earth system simulations. E.g., as seen in Ahlm et al., (2017) and Muri et al. (2017), MSB may lead to an increased sinking of air over the oceans and hence a reduction in cloud cover."

Lines 497-500: Could you speculate, what are the implications of using a high emission scenario (RCP8.5) instead of a low emission scenario (RCP4.5)?
Generally, the global mean and rate of change of ecosystem drivers in RCP4.5 are smaller than RCP8.5 (Henson et al., 2017). Applying the same RM forcing on RCP4.5 projection would yield a global mean state that is closer to the pre-industrial state with model-dependent regional variations. A short sentence has been added reflecting this.

Table 2: I would write that AOD is modified to reflect a sulphur injection not to give an impression that the sulphur injection is calculate online in the current study.
The table has been updated with a more precise definition of the experiments.

Figure 2 and other maps: Could you move labels a,b,c,... outside the plots? They are a bit hard to see and I first thought they were missing altogether. **Done**

All line plots: The lines are a bit hard to tell apart. I know that with so many overlapping lines it's hard to make them easy to distinguish, but I think there could be some room for improvement using dashed lines or slightly thicker lines or something.
We have altered the figures slightly so that they now, hopefully, are easier to read.

Figure 5. The legend is missing. Also, why is there a gap in the line of CCT around 2100?
The gap is a glitch in the making of a png figure, it does not exist in the higher quality pdf figure. The pdf version is attached to this reply (Fig 2 below) and will be included in the revised submission. The legend is added.

Figure 6: Standard deviation of what? Inter-annual variability of annual means of the reference period?
One standard deviation is defined as the standard deviation of the mean of the 1971-2000 period in the historical run. This is now clarified in the text and in all relevant figure captions.

Figure 7. Could the legend be included in sub figure a already? **Done.**

Technical corrections – **All have been changed accordingly.**
Line 34: temperatures -> temperature Line 39: I think "induced" is redundant here. Line 235: continue -> continues (if you keep the present tense) Line 408: decreases -> decrease Lines 472-473: A verb is missing. (in -> are ?)

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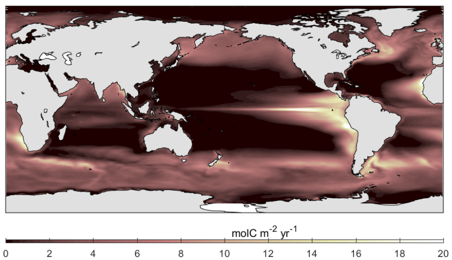

Fig. 1.

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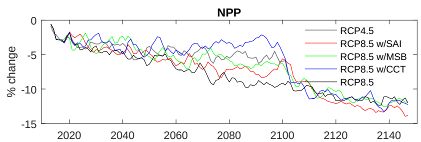

Fig. 2.

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