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

Anonymous Referee #2

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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 geo-engineering schemes, all affecting the radiation balance, two mainly on the incoming shortwave radiation, and the third mainly on the outgoing long-wave radiation are ap-

plied in this study, in such a way that globally they all lead to a reduction of the radiative flux by 4 W m^2, 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? 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?

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?

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. 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.

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.

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.

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.

Line 66 ff: I found this sentence quite confusing: Is it maybe two sentences in one?

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.

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.

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

Line 297: 'production' missing after 'increasing primary'

Line 299-300: 'after termination it takes less than 5 years': What sets the timescale, the atmosphere (radiation), or the ocean biology?

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.

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

Line 441: Is 18 percent really a 'minor change' compared to 13 percent?

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.

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.

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

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