Light absorption by marine cyanobacteria affects tropical climate mean state and variability

Reply to Reviewer 1

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We thank the reviewer for the positive feedback and for the helpful and constructive comments. A point-to-point reply is given below. The referee's comments are in red color, and our reply in black color.

Summary: This manuscript presents a sensitivity study of the effects of including a biophysical feedback of general phytoplankton and cyanobacteria light attenuation in an earth system model. With respect to cyanobacteria, the authors find a significant effect on tropical general circulation that opposes the prevailing understanding based on observations and regional modelling. Biophysical feedbacks are typically ignored in ocean modelling, thus this manuscript advances our understanding of potential deficits in the state of the science. The science is of a high quality and is suitable for publication in ESD. The language needs editing but I have no major concerns about this study.

General Comments: The positive buoyancy of cyanobacteria in the model is novel, as far as I am aware, and deserves more discussion. If a high concentration of cyanobacteria warms the local water (or produces a cooling via advection), this should also change the surface density. I assume the cyanobacteria buoyancy then adjusts, but are there upper or lower limits to this? If limits are imposed, is there an implication for the model results?

In the model, we assume a constant rising velocity of 1 m d^{-1}. Thus, density changes of the surrounding water do not feed back on the buoyancy of cyanobacteria. The vertical movement of cyanobacteria is not well constrained, and observations range from a negative buoyancy (sinking) to a rising velocity of about 260 m d^{-1} (Walsby, 1978; Villareal and Carpenter, 2003; Guidi et al., 2012). The aim of prescribing a positive rising velocity (instead of a neutral buoyancy as prescribed for bulk phytoplankton) is to simulate accumulations of cyanobacteria biomass at the surface (representing surface blooms). In Paulsen et al. (2017), we did sensitivity experiments with varying rising velocities (ranging from 0 to 50 m d^{-1}). We found the largest sensitivity between 0 m d^{-1} and 1 m d^{-1}. Already a value of 1 m d^{-1} counteracts the downward mixing and leads to a major fraction of biomass in the first model layer. As higher rising velocities give only slightly different results, we decided to take a value of 1 m d^{-1}, analogous to Sonntag (2013).

Specific Comments:

Abstract: Please add a sentence explaining the reason for the apparent disagreement between your model results (cooling effect) and observations (heating effect).

Observations rather represent snapshots of local heating events. In the model, we also find a local increase in SST on short timescales in times of high cyanobacteria concentrations. On larger timescales, however, the cooling due to the upwelling of cooler subsurface water outweighs the local warming effect in largest areas. The focus of this paper is on these global effects on climatological timescales which leads to the apparent disagreement between the observed local heating and the simulated cooling. We slightly modified the sentences in the abstract, so that the difference between observations (short time scales) and presented model results (climatological timescales) is made more clear.

Page 2, line 14-16. Not quite true, see Dutkiewicz et al. (2015) Biogeosciences and references contained within. I'm not sure how many of these include bio-physical feedback however.

The study of Dutkiewicz et al. (2015) explicitly includes the effect of several optically important water constituents (amongst others different phytoplankton functional types) to resolve the penetration of spectral irradiance. The study, however, does not include the feedback from phytoplankton light absorption on temperature, but only investigates the effects of different light attenuation formulations on ocean biogeochemistry. In the respective text passage in our manuscript we emphasize that to our knowledge there is no study that differentiates between different phytoplankton types and investigates their contribution to the feedback on physics. We modified the text in the manuscript to make this more clear to the reader (page 2 lines 14-16).

Page 18 line 16. The model has short time steps. Does it produce temporary local heating? And please mention whether there has been observed subsurface cooling associated with the localized warming.

The increased light absorption due to the presence of cyanobacteria always leads to a relative local heating of the surface water. If the net effect is, however, a warming or a cooling depends on which of the two processes – the warming or the cooling due to upwelling and transport of cooler subsurface water – dominates. On short timescales, we indeed see the local heating effect which takes place in times of high cyanobacteria concentrations. On climatological timescale, the timescale we focus on in this study, the surface cooling prevails in largest areas. There are, however, some limited regions, such as the eastern tropical Atlantic and Pacific, with a positive SST anomaly on the climatological mean (as described in the manuscript, page 10, lines 16-20).

The observations focused on the surface ocean (Kahru et al., 1993; Capone et al., 1998; Wurl et al., 2018). The respective studies only reported a surface heating effect and do not mention the effect on the subsurface. It is probably difficult to attribute measured subsurface anomalies to the impact of cyanobacteria light absorption due to other processes acting at the same time.

Technical Corrections:

We thank the reviewer for the detailed corrections of the language. We changed everything as recommended. In the following, we only list the points that require additional commenting by the authors.

Page 2 line 19: edit to "to particularly affect", and insert the relevant regions associated with each study for clarity.

We edited the formulation (page 2 lines 18-19). All mentioned studies, however, focus on similar regions. So, it is not necessary to separate between the studies.

Page 3 line 18: So there is a single shortwave radiation value at each latitude per day? This should be made clearer.

As we are using a fully coupled Earth system model, incoming shortwave radiation that enters the ocean depends on regionally varying factors, such as cloud conditions. It is, however, correct that the atmospheric model is coupled to the ocean model once per day, that means there is no diurnal cycle in the incoming radiation. We added this in the manuscript (page 4 line 10).

Page 4 line 1: What other models include positive buoyancy in some phytoplankton types? This should be addressed by one sentence in the Introduction.

The studies which are given as references in the manuscript (Hense, 2007; Sonntag and Hense, 2011; Sonntag, 2013), use an idealized biogeochemical model which includes positive buoyancy of cyanobacteria. It is applied in an one-dimensional as well as three dimensional setup of the North Atlantic, and the Baltic Sea, respectively. We already mention in the Introduction that these studies include the positive buoyancy of cyanobacteria (page 2 line 9). To our knowledge there is no global model that includes positive buoyancy of phytoplankton types. We modified the respective text passage in the Introduction to make clear, that we refer to global model studies in this context (page 2 line 16).

Page 4 Equation 1: There is no light attenuation by sea ice? Are the parameter values also adopted from Zielinski et al (2002) or were they tuned for MPI-ESM according to some criteria?

Light is not able to penetrate through sea ice in the model. Equation 1 refers to ice-free regions of the ocean.

Yes, the parameter values for light attenuation are adopted from Zielinski et al. (2002). We added one sentence in the manuscript (page 4 lines 15-16).

Page 6 line 29: remove "probably somewhat"

In the eastern tropical Pacific, there are no observations of cyanobacteria biomass and N_2 fixation available. The statement that the model overestimates biomass and N_2 fixation rates is only based on the fact, that the dust deposition is overestimated in this region (Paulsen et al., 2017). We reformulated the sentence to make this more clear (page 6 lines 27-29): "In the eastern tropical Pacific, on the other hand, concentrations and fixation rates are high, but the lack of observational data does not allow for a proper assessment."

Page 8 line 7: really? My impression from Page 6 line 25 and Page 7 is the top layer is 22 m thick.

The first model layer is 12 m thick, modified by variations of the sea surface height. The figures in the manuscript show the mean of the first two layers (∼22 m). The caption of Figure 1 of the manuscript, which said "surface concentrations", was indeed somewhat misleading. We modified it accordingly to "in the upper 22 m" to make it clear (see caption of Figure 1 in the manuscript, page 7).

Page 21 line 4: remove "seems to", change to "overestimates"

As mentioned above, in the eastern tropical Pacific there are not sufficient observations to evaluate the model performance with respect to cyanobacteria concentrations. We therefore reformulate it to "might overestimate" (page 20 line 34).

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