Authors Response to Reviewer's Comments

Reviewer 2

The manuscript investigates how the presence of chlorophyll affects near surface ocean temperature and stratification around the equatorial Atlantic. The study uses idealised experiments with a coarse-resolution ocean model where a monthly chlorophyll climatology is scaled to represent varying levels of chlorophyll abundance. Additionally, it uses a satellite chlorophyll product to demonstrate a declining trend in the equatorial Atlantic and two eastern Atlantic coastal upwelling systems.

In my opinion, this study is interesting and although idealised, it offers much needed insight into the effects of chlorophyll on the physical ocean conditions of the equatorial Atlantic; which is a biophysical feedback mechanism that is currently underexplored and often overlooked in models and theoretical frameworks. Hence, the study has strong merit and is suitable for publication in EGUsphere. However, I have some comments and queries aimed at improving the clarity of the methodology, results, and discussion. Therefore, I recommend minor revisions before the manuscript is published.

We thank the reviewer for the positive evaluation of our manuscript and for the constructive comments and suggestions. We address the different points raised by the reviewer thereafter. The reviewer's comments are shown in black and our responses in blue. When a line number is provided, it refers to the manuscript with track changes accepted. References can be found at the end of the document.

Specific Comments

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- Abstract, lines 9–10. For clarity, I suggest replacing "of chlorophyll-a concentration scaled by 0.01, 0.5..." with something along the lines of: "The response of eastern equatorial Atlantic SST to chlorophyll changes is non-linear, such that doubling or halving the chlorophyll-a concentration does not have a proportional impact."

  We have modified this sentence, also based on comments from Reviewer 1, to: "We also show that when the prescribed monthly climatology of chlorophyll-a concentration is scaled by 0.01, 0.5, 1, 1.5, and 2, the eastern equatorial Atlantic SST variability responds non-linearly, decreasing more strongly under low chlorophyll-a concentrations than it increases under comparable high chlorophyll-a conditions." *L10-12.*

- Line 58. For clarity, consider clarifying that instead of using different climatologies, you are using "experiments forced with different scaling factors applied to the same monthly Chl-a climatology."

  Modified as suggested by the reviewer. The sentence reads now: "Here, we aim to investigate the equatorial Atlantic interannual SST variability response to this bio-physical process through ocean model experiments forced with different scaling factors applied to the same monthly Chl-a climatology." *L58-59*

- Section 2. I recommend splitting Section 2 into three distinct subsections to improve readability: (i) Subsection 2.1- Observational Data (focusing on the current Section 2.1.1). (ii) Subsection 2.2 - Model Configuration and Experiments (combining the current 2.1.2 and 2.2.1, the model setup and experiment details). (iii) Subsection 2.3 - Analysis Methods (covering the current 2.2.2 and 2.2.3 to anomaly definitions and significance testing). Additionally, to aid the reader, I suggest including a summary table of your experiments. For example, explicitly showing that ITCP-MOM5-CTRL corresponds to the SeaWiFS Chl-a monthly climatology, ITCP-MOM5-0.5 corresponds to 50% of the SeaWiFS Chl-a monthly climatology, etc.

  We thank the reviewer for the suggestions to help improve the readability of the manuscript. We have modified Section 2 and added the following summary table as Table 1 in the revised manuscript.

Table 1: Sensitivity experiments performed to assess the bio-physical effect of chlorophyll-a concentration (Chl-a) on the equatorial Atlantic Ocean mean state and interannual variability. ICTP-MOM5-CTRL serves as the reference simulation (see Sect. 2.2). All sensitivity experiments are identical to the control except for the prescribed monthly Chl-a climatology.

Experiment	Description	Chl-a scaling
ICTP-MOM5-CTRL	Reference simulation	1.0× SeaWiFS climatology
ICTP-MOM5-CHL0.01	'Clear-ocean' case with near-zero Chl-a	0.01× SeaWiFS climatology
ICTP-MOM5-CHL0.5	Reduced Chl-a experiment	0.5× SeaWiFS climatology
ICTP-MOM5-CHL1.5	Enhanced Chl-a experiment	1.5× SeaWiFS climatology
ICTP-MOM5-CHL2.0	Doubled Chl-a experiment	2.0× SeaWiFS climatology

- Section 2.2.1. I found the introduction of the observational data somewhat confusing, as it is not immediately clear how they are used at this stage. Are they used as a reference for comparison/validation, or as a baseline to create the climatological forcing for your control run? Please clarify if these data are used strictly for comparison/validation and are distinct from the forcing data (if I have understood correctly).

  We thank the reviewer for pointing that out. We have modified this section as follows:

  "The monthly mean SST data from the Optimum Interpolation SST version 2.1 (Reynolds et al., 2007, OI-SST), produced by the Physical Sciences Laboratory of the National Oceanic and Atmospheric Administration (NOAA), are used as a reference dataset to validate the seasonal cycle of SST and of SST variability of the control run (introduced in the next section) and to compare with the ones from the different sensitivity experiments. The dataset is available at 0.25° horizontal resolution from September 1981 to present day. Monthly means of Chl-a, derived from the daily gap-free product Copernicus-GlobColour (Copernicus, 2024), are used to compute linear trends in Chl-a in the tropical Atlantic for the period 1998-2024. This product is available at 4 km horizontal resolution from 4th of September 1997 to present day. More information on the performance of this product and on the methods used to merge the different satellite measurements and to fill the gaps can be found in the "User manual" and "Quality information" at `https://data.marine.copernicus.eu/product/OCEANCOLOUR_GLO_BGC_L4_MY_009_104/description`." *L69-78*

- Line 85. If I understand correctly, this is the baseline climatology used across all experiments (albeit scaled). I suggest explicitly highlighting this point.

  The reviewer is correct. We have highlighted that in the revised manuscript as follows: "Additionally, ICTP-MOM5-CTRL includes a monthly climatology of Chl-a, which is based on 8-day composites of Sea-viewing Wide Field-of-view Sensor (SeaWiFS) images taken from 1999 to 2001. This is the baseline climatology used across all the sensitivity experiments described in the following." *L89-90*

- Section 2.2.1. Please clearly state the integration periods for both your control and sensitivity simulations. (Maybe is mentioned elsewhere and I missed it but this is probably the place to clarify it).

  We thank the reviewer for spotting this oversight. The control run simulation and all sensitivity experiments were integrated from January 1958 to December 2021 using the JRA55-based surface forcing dataset for driving ocean-sea-ice models. We have clarified this in the corresponding section: "The control run was integrated from January 1958 to December 2021 using the JRA-55-based surface dataset for driving ocean-sea-ice models (Tsujino et al., 2018, JRA55-do)." *L86-87*

- Line 99. I am unsure what is meant by "if" a monthly climatology is provided here. My understanding is that a monthly climatology is forced across all experiments; please clarify.

  We agree with the reviewer the "if" is misleading. We have modified the sentence to: "We note that, in all model simulations of this study, the prescribed monthly climatology of surface Chl-a is considered vertically independent." *L114-115*

- Line 113. Could you please clarify what is meant by "raw data"? Do you mean the model outputs? Additionally, please specify the temporal frequency of the model outputs that you are using.

  We have clarified this sentence as follows: "To obtain the detrended monthly mean anomalies, the monthly mean model outputs (and observational data, where applicable) are first linearly detrended point-wise over the study period 1982/01-2020/12." *L134-135*

- Lines 125–126. I believe you mean the difference between the "two model simulations/experiments" rather than "the two models" (e.g., CTL vs. CHL0.01).

  The reviewer is correct. Following the comments from Reviewer 1 this paragraph has been modified as follows: "To evaluate whether the variability (i.e. standard deviation of anomalies) or the mean state (time mean) differ between two simulations, we apply a nonparametric bootstrap method (Efron and Tibshirani, 1993) independently at each grid point. For the mean-state analysis, the statistics of interest is the difference between the time means of the two simulations, while for the variability analysis it is the difference between their standard deviations. Uncertainty in these differences is estimated using bootstrap resampling with replacement. For each simulation, the time indices are resampled independently 5000 times, and the statistics of interest is computed for each resampled dataset, resulting in a bootstrap distribution of the difference. From this distribution, we estimate a two-sided confidence interval using the 0.5th and 99.5th percentiles. Differences are considered statistically different from zero at the 99% confidence level when zero falls outside this interval." *L144-151*

- Lines 158–161. It is not entirely clear how Figure 2 (showing seasonal SST climatology across experiments and observations) demonstrates the importance of accounting for phytoplankton effects on the vertical distribution of shortwave heating. The SST bias in this region is not necessarily driven by a misrepresentation of Chl-a. While increasing Chl-a makes the surface cooler, it does not mean it reduces the bias for the correct physical reasons, as evidenced by the persistent year round bias between observations and all model configurations regardless of Chl-a scaling. I recommend rephrasing this part to focus strictly on the physical effects/changes induced by the scaling, rather than framing it as a correction of model bias. Furthermore, because you later argue that Chl-a controlling the vertical distribution of shortwave heating is not the dominant mechanism in the equatorial Atlantic (if I understood correctly), introducing it this way here creates some confusion.

We thank the reviewer for pointing this out. We have modified this text and it reads now:" Comparing the ATL3 SST seasonal cycle from both control and sensitivity simulations to the one derived from OI-SST shows that ICTP-MOM5 simulations are able to capture the main features of the seasonal cycle of SST in the ATL3 region (Fig. 2d). However, a year-round warm SST bias remains, which is common in ocean and climate models in this region (Richter et al., 2012; Farneti et al., 2022). We note that the idealized increase in Chl-a leads to a reduction of the SST bias, particularly in JAS (Fig. **??**d). However, this does not necessarily imply that the SST bias in this region results from an incorrect representation of Chl-a, but rather highlights the sensitivity of SST to phytoplankton-induced changes in the vertical distribution of shortwave heating. *L180-186*

- Lines 162–169. I agree with your argument. However, for completeness, maybe highlight here that this specific response occurs within the equatorial Atlantic (and other upwelling regions, along the easter coastal Atlantic). Outside this (e.g., in Fig. 2a), we do see a typical surface cooling associated with a reduction in chlorophyll, which aligns with the traditional view that lower chlorophyll allows shortwave radiation to penetrate deeper, cooling the surface. Highlighting that this counterintuitive warming response is unique to the upwelling zones (the equator and potentially the Senegal/Namibia coasts in Fig. 2) would add valuable context.

  We have modified the paragraph as follows: "The surface warming in the equatorial Atlantic in the absence of Chl-a is somewhat counterintuitive, as a reduced absorption of the light near the surface would be expected to lead to local surface cooling, as observed in the open ocean and away from the equator (Fig. 2a). Such a counterintuitive response appears to be related to upwelling regions, as it has already been observed in the equatorial Pacific upwelling (Manizza et al., 2005; Sweeney et al., 2005; Löptien et al., 2009; Park et al., 2014b,a) and in the Senegalo-Mauritanian and Benguela upwelling systems (Frouin et al., 2007; Hernandez et al., 2017)." *L187-191*

- Line 167. Please clarify what you mean by "opposite what is done here"? In your setup, you have a control run and experiments where chlorophyll is fractionally increased or decreased. How does this contrast with traditional experiments featuring active or not active bioheating?

  We apologize for the confusion. What is meant here is that in this study we show the bio heating effect by plotting the difference EXP minus CTRL while previous studies generally showed CTRL minus EXP, hence in our figures we expect to see the opposite effect. We clarify this sentence as follows: "We note that in most previous studies, the bio-physical effect was highlighted by showing the difference between the control run and the experiment (control minus experiment), whereas in this study the difference shown is experiment minus control." *L192-193*

- Lines 168–169. Please clarify the phrase "strengthens the upwelling". Do you mean it leads to a faster upwelling velocity that is more regionally confined and draws water from different depths?

We have clarified these sentences. The text reads now: "They showed that the dominant mechanism explaining the surface cooling is the vertical redistribution of shortwave heating by Chl-a, which enhances upper-ocean stratification, shoals the mixed layer, and increases the divergence of mixed-layer transport by weakening the compensating equatorward geostrophic transport relative to the poleward Ekman transport. This leads to enhanced equatorial upwelling across the mixed-layer base. Our findings are therefore consistent with this mechanism but exhibit anomalies of the opposite sign." *L194-198*

- Lines 170–180 (and Discussion lines 229–231). I would welcome some clarification on the physical mechanism proposed here. While I agree that a deeper mixed layer in an upwelling region can coincide with higher SST under steady winds, the explanation could be clearer. Mechanistically, volume transport and upwelling are determined by wind stress, so they should remain relatively unaltered by changes in mixed-layer depth . Instead, when the mixed layer deepens (as seen in CHL-0.01), the meridional velocity decreases to matches changes in the vertical velocity (consistent with Fig. 3b). The mixed layer deepening pushes the thermocline and the underlying cold water downward. Consequently, under identical wind forcing, the water being upwelled is warmer, and at the same time because the horizontal transport of this water away from the equator is slower, it has more time to warm from the atmosphere. The combination of these two would lead to the surface temperature increase in the CHL-0.01 experiment. Maybe I have misunderstood but please consider revising this part for clarify of the dynamics.

  Please see our response below.

- Lines 179-180. This links to my comment above. Maybe I have misunderstood, but it is not clear how a shallower mixed layer would lead to weaker upwelling under identical wind forcing. Please consider rephrasing to clarify.

  We agree that the original wording was imprecise and we improved it in the revised manuscript. Since the surface winds are identical in the different experiments, the net Ekman transport and the associated Ekman-driven equatorial upwelling remain unchanged. What changes between the simulations is not the total Ekman transport itself, but rather the compensating geostrophic flow within the mixed layer. Following the framework of Sweeney et al. (2005), the steady zonal momentum budget integrated over the mixed layer, with Reynolds stresses neglected and assuming that the baroclinic pressure gradients associated with horizontal density variations are small relative to the barotropic pressure gradient associated with sea surface height variations, gives:

$$M_y \approx \underbrace{-\frac{\tau_x}{\rho_0 f}}_{\text{(1) Wind-driven Ekman transport}} + \underbrace{\frac{gD}{f}\frac{\partial \eta}{\partial x}}_{\text{(2) SSH-gradient (geostrophic) contribution}} \qquad (1)$$

where M_y is the mixed-layer-integrated meridional transport, D is the mixed-layer depth, η is the sea surface height , τ_x is the zonal wind stress, f is the Coriolis parameter, and ρ_0 is a reference density. The mixed-layer-integrated meridional transport can be decomposed into (1) the wind-driven Ekman transport and (2) a compensating geostrophic transport associated with the zonal SSH gradient integrated over the mixed layer. Because the Ekman component is unchanged between ICTP-MOM5-CTRL and ICTP-MOM5-CHL0.01, differences in the net poleward transport arise from changes in the mixed-layer depth and/or in the zonal SSH gradient. The zonal SSH gradient, which represents the barotropic pressure gradient in this approximation, is similar in both simulations (Fig. R1). Therefore, the differences in meridional transport are primarily driven by changes in mixed-layer depth. In ICTP-MOM5-CHL0.01, the deeper mixed layer enhances the depth-integrated geostrophic compensation, thereby reducing the residual poleward transport within the mixed layer. Consistent with previous studies (Sweeney et al., 2005; Löptien et al., 2009; Park et al., 2014b,a), this leads to reduced equatorial upwelling into the mixed layer, although the total Ekman-driven upwelling remains unchanged. We have revised the paragraph in *L199-221*.

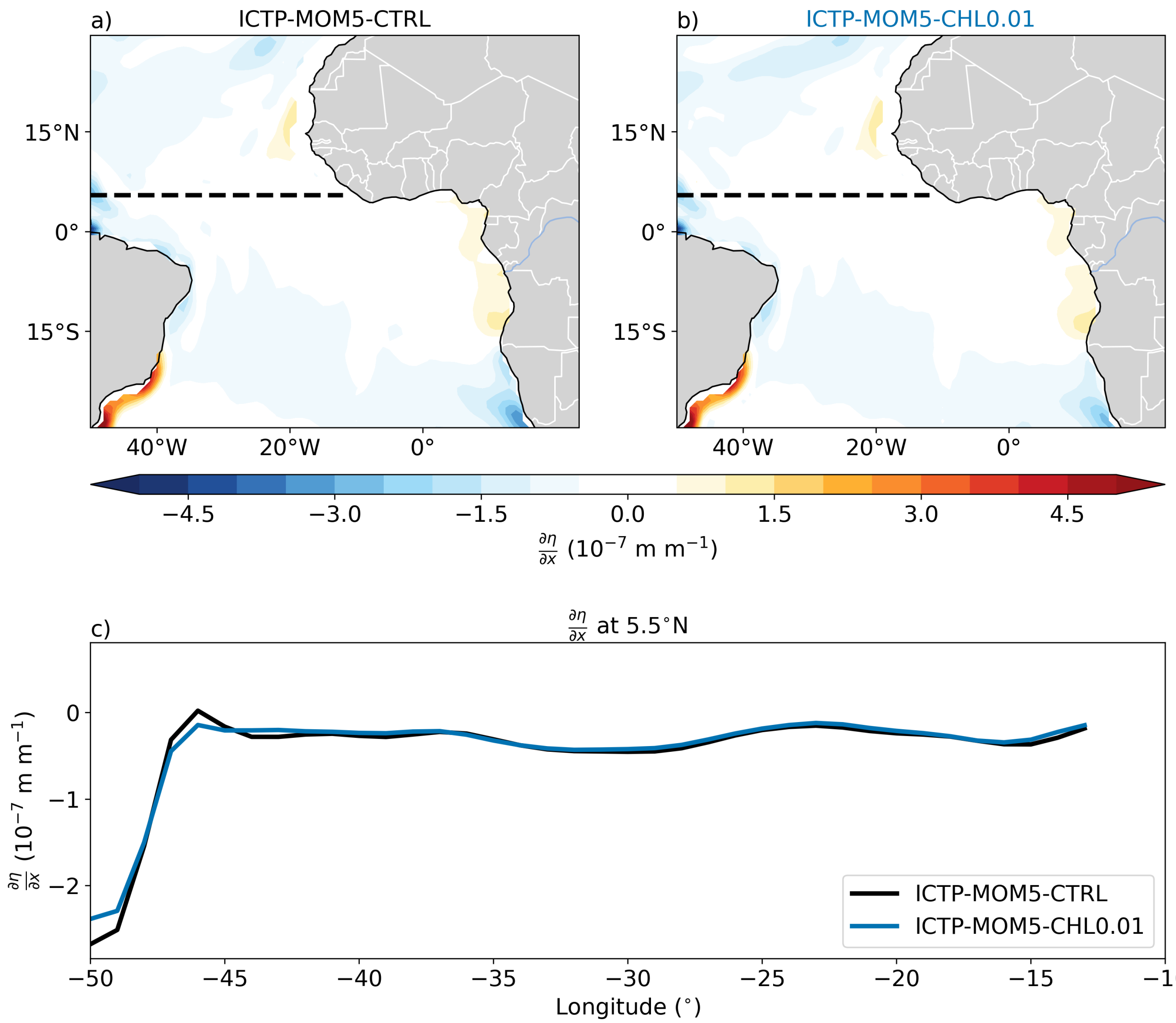

Figure R1: Zonal sea surface height gradient averaged between January 1982 and December for (a) ICTP-MOM5-CTRL and (b) ICTP-MOM5-CHL0.01. (c) Zonal SSH gradient at 5.5°N between 50°W and 10°W.

- Lines 190, 193, Figures 4 and 5. Please explicitly define the abbreviations SSTA and TEMPA in the text and captions (I am not sure what they mean).

  We thank the reviewer for spotting this oversight. "SSTA" means "SST anomalies" and TEMPA means "TEMP anomalies". As both of them are not used that often and because there are already many acronyms in the manuscript, we have decided to use "SST anomalies" and "TEMP anomalies" throughout the manuscript.

- Figure 5. Could you please clarify your trend estimates? Specifically, why are the values multiplied by 120 and 100?

  We have clarified and modified the caption of Figure 5 in the revised manuscript as follows: "Trends are computed from a linear least-squares fit applied to monthly mean chlorophyll-a time series at each grid point. The resulting slope (mg m^{-3} $month^{-1}$) is converted to mg m^{-3} $decade^{-1}$ by multiplying by 120 (12 months $\times$ 10 years). Relative trends are then expressed in % $decade^{-1}$ by dividing by the climatological mean chlorophyll concentration at each grid point and multiplying by 100."

- Figure 6. Please clarify both in the text (lines 215–220) and in the figure caption whether these estimates are derived from the GlobColour product (which I think is the case) or the model's control run.

  The reviewer is correct, Figure 6 and the text are based on GlobColour. We have made this clear in the revised manuscript. The revised paragraph is now: "The fact that changes in the mean state of Chl-a affect the interannual SST variability in the tropical Atlantic Ocean is of particular interest, as a merged satellite product of Chl-a (GlobColour) shows marked trends over the period 1998-2024 (Fig. 5c). Similar trends have also been reported in recent studies (Zhao et al., 2025; Hong et al., 2025; Silsbe et al., 2025). The timeseries of the GlobColour Chl-a averaged over the ATL3 region reveals a clear reduction in the yearly maxima, while the yearly minima remain stable (Fig. 6a). Comparing the seasonal cycle of the merged satellite product of Chl-a averaged over the ATL3 region during 1998-2011 with the one obtained over 2012-2024 reveals that the largest decrease occur during July-August-September, the season of maximum Chl-a (Fig. 6b). These ongoing changes in Chl-a in the tropical Atlantic Ocean underscore the need to improve our understanding of the relationship between Chl-a and interannual SST variability in the tropical Atlantic." *L255-263*

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