Authors Response to Reviewer's comments

Reviewer 1

A well written relatively short paper discussing the effect of plankton, represented by the concentration of chlorophyll-a, on the surface layers of the equatorial Atlantic. However, the paper does appear to have one major fault in that, in a system where SST and near surface stratification is important, the paper does not consider the interaction of the ocean and atmospheric temperature fields. I discuss the problem more below. This review concentrates primarily on the ocean physics aspects of the study. I do not have the right background to review the comparisons with studies in the Pacific or with observations of Atlantic upwelling systems.
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.

Detailed Comments

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Abstract

- **Line 7**. This does not explain which part of the 'vertical temperature gradient' is weakened. Later are you referring to the surface thermocline or the main oceanic thermocline.

  We thank the reviewer for pointing this out. Throughout the manuscript, the term "thermocline" refers to the main oceanic thermocline, defined as the depth at which the vertical temperature gradient reaches its maximum. To avoid ambiguity, we have modified the sentence in the abstract as follows: "Additionally, the vertical temperature gradient around the main oceanic thermocline of the equatorial Atlantic is weakened, the mixed layer and main oceanic thermocline are deepened, and the equatorial upwelling across the mixed-layer base is reduced." *L6-8*

- **Line 8**. Equatorial upwelling is not reduced. If the winds used in the different runs are the same then the Ekman transports will be the same, as will equatorial upwelling.

  We agree that under the same wind forcing, the Ekman-driven equatorial upwelling remains unchanged. However, the equatorward geostrophic flow induced by the negative zonal SSH gradient is enhanced when the mixed layer is deepened (see our longer response below). As a result, the net poleward transport within the mixed layer is reduced because the enhanced equatorward geostrophic flow provides greater compensation for the Ekman transport. Consequently, the effective upwelling across the mixed-layer base is reduced in the deeper mixed layer case (ICTP-MOM5-CHL0.01). This mechanism is consistent with the results from Sweeney et al. (2005); Löptien et al. (2009); Park et al. (2014b,a).

- **Line 10**. 'non-linearly'. After reading the rest of the paper I find this bland statement misleading. It needs something which implies that a reduction in chlorophyll has a large effect - and so will be the focus of the paper - with increases in concentration have a smaller effect.

  We have modified the sentence to include the idea that the reduction in SST variability under low chlorophyll-a concentration is larger than the increase under comparable high chlorophyll-a concentration. The sentence reads now: "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*

Introduction

- Line 14. 'The underlying dynamics ...'. I am not sure why 'underlying' is needed. Given that the best predictions of El Ninos are produced by computers and nobody understands the 'underlying' 'why', this is a slightly dangerous statement.

  We have replaced "The underlying dynamics" with "The dynamics associated with" *L20.*

- Line 33. 'well observed' is a bit clumsy - how about something like 'accurately and easily measured'.

  We agree with the reviewer, and modified the sentence as suggested. *L34-35*

- Line 59. After a detailed discussion of previous studies, the paper states some aims of the study - but I felt it would help the reader to have some clues along the lines of "As will be shown ... ".

  We thank the reviewer for this suggestion. We prefer to keep the introduction focused on the scientific motivation and research questions, without previewing the main findings.

Data and Methods

- Line 90. It would be cleaner just to say that in the model the incident radiation is split into three components, infra-red, red visible and green visible, in the ratios 0.58, 0.21 and 021.

  We have improved the paragraph following the reviewer's suggestion. It reads now: "The total surface irradiance, I_0, is split into three wavelength bands: the infrared (I_{ir}), red visible (I_{red}), and blue/green visible (I_{bg}) with a light partitioning of $I_{ir} = 0.58 \times I_0$, $I_{red} = 0.21 \times I_0$, and $I_{bg} = 0.21 \times I_0$, respectively." *L106-107*

- Line 95. Given the importance of the depth of heating in the following analysis, it would help to give the e-folding distance (i.e. 1/k) for each of the three 'k'. For the two which depend on concentration and the seasonal cycle, minimum and maximum depths would also be important.

  We agree with the reviewer and we have computed the seasonal cycle of the *e*-folding distances (1/k) for each simulation using the Chl-a seasonal cycles given in Fig. 1b and plotted them in Fig. R1c. The *e*-folding distances and Fig. R1c are now discussed in the revised manuscript: "As defined in Eqs. 2 and 3, the use of these different climatologies of Chl-a affects the *e*-folding depth scale of the light penetration in the visible red ($1/k_{red}$) and in the visible blue/green wavelength bands ($1/k_{bg}$; Fig. R1c) while the infrared ($1/k_{ir}$) is independent. The effect of Chl-a is limited for $1/k_{red}$, as the *e*-folding scale is approximately 4 m in all simulations throughout the year (Fig. R1c). In contrast, the effect of Chl-a is important for $1/k_{bg}$. In ICTP-MOM5-CHL2.0, the ATL3 seasonal cycle of $1/k_{bg}$ varies around 14 m, whereas in ICTP-MOM5-CHL0.01 it varies around 40 m." *L125-130*

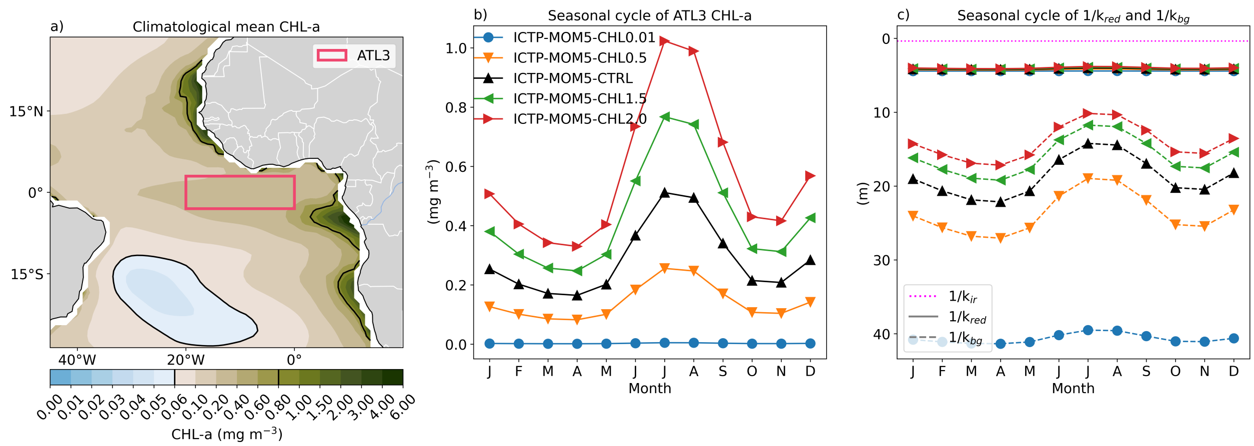

Figure R1: (a) Climatological mean chlorophyll-a concentration prescribed to ICTP-MOM5-CTRL. Black contours indicate the 0.06 mg m^{-3} and 0.80 mg m^{-3} chlorophyll-a concentration levels. (b) Seasonal cycle of the prescribed chlorophyll-a concentration averaged over the ATL3 (20°W-0°E, 3°S-3°N) region and for the different model simulations: ICTP-MOM5-CHL0.01 (blue), ICTP-MOM5-CHL0.5 (orange), ICTP-MOM5-CTRL (black), ICTP-MOM5-CHL1.5 (green), and ICTP-MOM5-CHL2.0 (red). (c) Seasonal cycles of the e-folding depth scales of the light penetration in the ATL3 region for the infrared ($1/k_{ir}$, magenta dotted line), the red ($1/k_{red}$, full lines), and the blue/green ($1/k_{bg}$, dashed lines) wavelength bands. As $1/k_{red}$ and $1/k_{bg}$ depend on the chlorophyll-a concentration (cf. Eqs. 2 and 3), the seasonal cycles are plotted for each model experiment.

- Line 99. The paper states "this climatology is considered vertically uniform". It then states "This is certainly a limitation as the maximum on Chl-a is usually found close to the nutricline". The phrase "considered vertically uniform" is too vague. I presume that this means the chlorophyll field is independent of depth in the model. But I think a more serious problem is that the paragraph hints that the model is not suitable for many studies involving chlorophyll. In reality the chlorophyll and nutrient distribution will depend on surface mixed-layer depth and stratification. It therefor needs to explain why the model is suitable for the different runs. I can understand it may have been tuned for the standard case and still be valid with no chlorophyll, but with double chlorophyll one might expect additional feedbacks.

  Yes, the reviewer is correct, we meant that the Chl-a is independent of depth, i.e. the Chl-a concentration is the same at every vertical level. We have modified the paragraph as follows: "We note that, in all model simulations of this study, the prescribed monthly climatology of surface Chl-a is considered vertically independent. This is certainly a limitation as the maximum Chl-a is usually found close to the nutricline in the tropical Atlantic Ocean (Herbland, 1983). However, Hernandez et al. (2017) compared a forced ocean simulation with a depth-independent profile of Chl-a to a forced ocean simulation with a depth-dependent Chl-a profile and reported only small differences in SST between the two simulations (see Fig. A1 in Hernandez et al. (2017)). Hence, for simplicity and consistency with previous studies, we have used depth-independent profiles of Chl-a in the following." *L114-120*. Furthermore, the Modular Ocean Model has been used in several studies focusing on the impact of chlorophyll on ENSO (Sweeney et al. (2005); Park et al. (2014b,a); Löptien et al. (2009)) suggesting that it is a suitable model to study the impact of chlorophyll-a on interannual variability in the tropics.

- Line 108. It would help to state here that the paper will focus on the changes in the low chlorophyll run.

  We have modified the sentence to: "In the following, we examine the biological effect by focusing on the difference between the low Chl-a simulation (ICTP-MOM5-CHL0.01) and the control run (ICTP-MOM5-CTRL)." *L130-131*

- Line 118. This needs to be clear about the difference between the near surface thermocline and main oceanic thermocline.

  Throughout the manuscript when we refer to the "thermocline", we refer to the depth at which the vertical temperature gradient is maximum, as commonly done in the tropics.

- Line 122. The null and alternative hypotheses statements are misleading. The null hypothesis should probably be that on the basis of the evidence the signal cannot be distinguished from zero. But I don't than that a yes/no argument is valid. What the paper discusses later is the 'probability' that the signal is different from zero. The bootstrap method of resampling with replacement is one way of doing this. I think the paragraph needs a good rewrite, with some good references describing the method, the quality of the results and how it compares with other methods for estimating the probability that results are significant.

  We thank the reviewer for making this point. We have removed the null and alternative hypotheses statements, revised the paragraph, and we have included a reference. It reads now: "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 statistic 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 are 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*

Results

- Line 132. The paper appears to make a serious mistake in not considering the strong interactions between air temperature and sea surface temperature. It states that the JRA-55 atmospheric fields have been used. As well as the wind stress fields and the downward radiation fluxes JRA-55 also includes the 10m air temperature and humidity. Within the ocean model these variables will normally be used to calculate the sensible and latent heat fluxes across the air sea interface. If this does not happen in the version of Mom used, they my following comments should be ignored and the paper corrected with a better explanation of the way the ocean model handles the JRA-55 data. The sensible heat flux depends just on the air-sea temperature difference. The latent heat flux is a function of both the temperature difference and the humidity of the atmosphere. Ocean models usually include code to represent these processes and I would be surprised in Mom is different. The fluxes are not small and in reality this means that SST and the surface atmospheric temperature are usually close. In the present case, where the atmospheric temperature is fixed, it means that the SST will always be close to the imposed air temperature. This helps explains why the two functions in Fig. 4c stay so close over such a long period of time. The SST differences seen in Fig 4a, are thus really a measure of how hard to system is trying to get away from the imposed air temperature. In a fully coupled system the differences will be larger. I am not sure how the surface boundary condition affects the rest of the results. I suspect that they also show the direction in which changes will occur in a fully coupled system - but this is really up to the authors to think through.

We thank the reviewer for raising this important point. We have added the following paragraph on the absence of ocean-atmosphere feedbacks in ocean models and inherent limitations: "A caveat of forced ocean-sea ice configurations is the lack of two-way ocean–atmosphere coupling. In ICTP-MOM5, the ocean model is forced with the time-evolving JRA55-do atmospheric state, including winds at 10 m, shortwave and longwave heat fluxes, and near-surface (2 m) atmospheric temperature and humidity, which are used to calculate latent and sensible heat fluxes through bulk formulae. This forcing enables the model to realistically reproduce historical oceanic conditions over the simulation period. Additionally, the absence of SST restoring, together with the weak sea surface salinity restoring, prevent the artificial damping of intrinsic ocean variability. Nevertheless, prescribing the near-surface atmospheric state imposes some constraints on the simulated SST through the turbulent heat fluxes. Despite this limitation, Park et al. (2014b) showed that the impact of Chl-a on eastern tropical Pacific SST is qualitatively consistent between ocean-only and fully-coupled model experiments. Their results indicate that atmosphere-ocean coupling mostly amplifies the chlorophyll-induced mean change through a positive atmosphere feedback, but does not change the sign of the response. Moreover, the simulated response of ENSO in both fully-coupled and ocean-only model experiments was consistent Park et al. (2014b). These findings suggest that forced ocean simulations provide a robust framework for investigating the influence of Chl-a on the equatorial Atlantic variability. Yet, we acknowledge that the magnitude of the response may be lower than if a fully coupled system was considered." *L92-104*

- Lines 165-180. The statement of line 170 correctly implies that because the winds are the same in the different runs, the Ekman net transport will be the same. This further means that Ekman driven upwelling at the Equator will also be the same. So nothing can "strengthen equatorial upwelling through poleward volume transports" What can change is the vertical distribution of the Ekman transport and this will change the velocities at each level. The changed transport will also affect the vertical structure of equatorial upwelling. As an example of the Ekman transport profile changing, over much of the ocean a large fraction of the Ekman transports is usually found in the surface mixed layer. In this case if the mixed layer shallows but the transport in the layer stays the same, then velocities in the layer will increase to give the same total transport. Anyway both paragraphs need to be rewritten as do any similar statements elsewhere in the paper.

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:

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

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. R2). 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*.

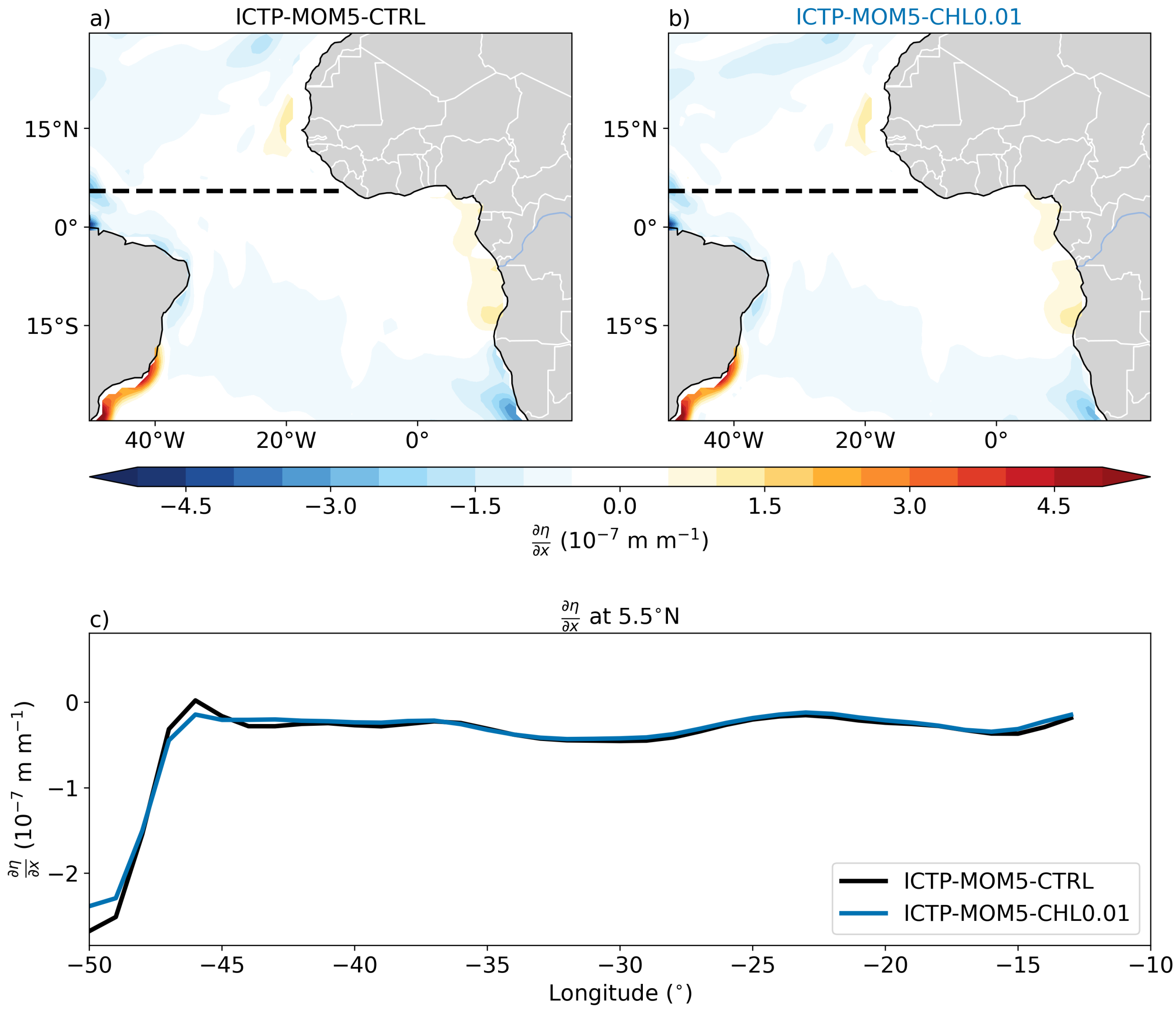

Figure R2: Zonal sea surface height gradient averaged between January 1982 and December for (a) ICTP-MOM5-CTRL and (b) ICTP-MOM5-CHL0.01. (c) Zonal SSH gradient at 5.5°N between 50°W and 10°W.

- Line 200. "weakened equatorial upwelling". See above.

  See our answer above. We have modified the sentence to: "Additionally, weakened equatorial upwelling across the basis of the mixed layer (Fig. 3d) also contributes to the reduced interannual SST variability in ICTP-MOM5-CHL0.01." *L241-242*

- Line 206. "MJJ", "ND". Expand.

  We have expanded MJJ and ND throughout the manuscript.

References

Efron, B. and Tibshirani, R.: An introduction to the bootstrap / Bradley Efron and Robert J. Tibshirani., Monographs on statistics and applied probability ; 57, Chapman & Hall, New York, [corrected printing].

edn., ISBN 0412042312, 1993.

Herbland, A.: Le maximum de chlorophylle dans l'Atlantique tropical oriental : description, écologie, interprétation, Océanographie Tropicale, 18, 295–318, 1983.

Hernandez, O., Jouanno, J., Echevin, V., and Aumont, O.: Modification of sea surface temperature by chlorophyll concentration in the Atlantic upwelling systems, Journal of Geophysical Research: Oceans, 122, 5367–5389, https://doi.org/https://doi.org/10.1002/2016JC012330, 2017.

Löptien, U., Eden, C., Timmermann, A., and Dietze, H.: Effects of biologically induced differential heating in an eddy-permitting coupled ocean-ecosystem model, Journal of Geophysical Research: Oceans, 114, https://doi.org/https://doi.org/10.1029/2008JC004936, 2009.

Park, J.-Y., Kug, J.-S., and Park, Y.-G.: An exploratory modeling study on bio-physical processes associated with ENSO, Progress in Oceanography, 124, 28–41, https://doi.org/https://doi.org/10.1016/j.pocean.2014.03.013, 2014a.

Park, J.-Y., Kug, J.-S., Seo, H., and Bader, J.: Impact of bio-physical feedbacks on the tropical climate in coupled and uncoupled GCMs, Climate Dynamics, 43, 1811–1827, https://doi.org/10.1007/s00382-013-2009-0, 2014b.

Sweeney, C., Gnanadesikan, A., Griffies, S. M., Harrison, M. J., Rosati, A. J., and Samuels, B. L.: Impacts of Shortwave Penetration Depth on Large-Scale Ocean Circulation and Heat Transport, Journal of Physical Oceanography, 35, 1103–1119, https://doi.org/https://doi.org/10.1175/JPO2740.1, 2005.