Review of "How does the phytoplankton-light feedback affect the marine N_2O inventory?" by Berthet et al.

Rewiever #1: Rémy Asselot, 25 Aug 2022

Summary

The phytoplankton-light feedback (PLF) is the mechanism defining the absorption of shortwave radiation by phytoplankton in the ocean. This mechanism affects the distribution of light in the water column and thus the oceanic temperature. As a consequence, the oceanic heat content and the sea-air gas fluxes are affected, altering the whole climate system.
This paper presents simulations with a complete or incomplete representation of PLF and describes ocean physics and biogeochemistry accordingly. To answer this question, the authors use an Earth system model with oceanic, sea-ice and marine biogeochemical components only. What is novel and interesting is that (1) depending on the representation of PLF, the climate system can be highly modified, and (2) simulated N2O budget are highly uncertain. I believe that this study deserves to be published and provides implications for future climate models development. However, before accepting this manuscript, I think that there are some aspects that could be improved. Especially the description of the model setup and the description of the processes explaining the results. I may have misread some things but I hope that the following comments will help the authors to improve their manuscript.

Our sincerest appreciation to you for your valuable comments and suggestions on our manuscript. We have addressed all of the points your raised in our revised manuscript, and we hope that the manuscript is now ready for publication. Please find below our detailed response (blue text) to your comments (black text). In our responses, changes to the manuscript are indicated by ***bold italicized*** text and lines numbers are relative to the new manuscript.

Please note that, as proposed by the second reviewer, simulation names have been changed in the new manuscript:
- chl_inter is now called REF
- clim_zcst is now called climZCST
- clim_zvar is now called climZVAR

Major comments:

Section 1

The authors seem to say that a consensus exists on the effect of PLF on the thermal structure of the ocean (line 96-102). However, several studies report an increase in SST due to PLF (e.g. Oschlies, 2004; Anderson et al., 2007; Lengaigne et al., 2009) while others report a cooling of the surface of the ocean (e.g. Nakamoto et al., 2001; Manizza et al., 2005; Löptien et al., 2009; Paulsen et al., 2018). So far, no consensus exists but the cooling effect reported in these studies might be due to the non- or weak coupling between the oceanic and atmospheric components of the models (Tian et al., 2021).

Thank you for this remark. In lines 99-104 we do not suggest that there is a consensus on the sign of the perturbation (SST cooling or SST warming), but rather that the first order of the PLF is to perturb the ocean thermal structure. But indeed in the previous submitted version there was no clear mention of cooling cases, which could lead to confusion. This has now been added and we hope that the new version reflects more clearly the state of the art on this issue. We modify the paragraph in the revised manuscript as follows (lines 99-117):

"Despite the diversity of modelled responses, a consensus emerges on the first order effect of PLF on the ocean physics, which is to perturb the ocean thermal structure (Nakamoto et al., 2001; Murtuggude et al., 2002; Oschlies, 2004; Manizza et al., 2005, 2008; Anderson et al., 2007; Lengaigne et al., 2007; Gnanadesikan and Anderson, 2009; Löptien et al., 2009; Patara et al., 2012; Mignot et al., 2013; Hernandez et al., 2017). By trapping more heat at the ocean surface in eutrophic regions, such as coastal or equatorial upwellings areas, the presence of phytoplankton ***initially*** increases the surface warming. Confining heat at the surface leads to less heat penetrating in subsurface. ***In some cases, the advection and upwelling of subsurface cold anomalies can lead to remote cooling effects (Hernandez et al., 2017; Echevin et al., 2022). Dynamic readjustment in response to perturbations in thermal structure has also been shown to have a cooling effect, by increasing upwelling of cold water to the ocean surface. (Manizza et al. 2005; Marzeion et al., 2005; Nakamoto et al., 2001; Löptien et al., 2009; Lengaigne et al., 2007; Park et al., 2014).*** Because these effects depend on upper ocean stratification, an important role is attributed to modelled seasonal deepening of the mixed layer as it determines the intensity of the underlying temperature anomaly and its vertical movement to the surface. In other terms, whatever the temporality of the causal chain, changes in the PLF representation are expected to both perturb the ocean heat uptake, and trigger perturbations of both the water column stratification and associated ocean dynamics."

Section 2.a

I understand that model configuration has already been described in Berthet et al. (2019) but it would be appropriate to give more explanation on the model configuration for the readers not familiar with Berthet et al. (2019). For instance, what is the vertical resolution of the oceanic grid? What is the depth of the first oceanic layer?

We have added some additional elements, in particular as suggested on the question of vertical discretization which is indeed important for solar flux penetration issues. But we would prefer not to go into details of the numerical choices at the risk of being too repetitive with Berthet et al. Lines 176-180 we added: "***Our modelled ocean has 75 vertical levels and the first level is at 0.5 meter depth. Vertical levels are unevenly spaced with 35 levels being in the first 300 meters of depth. Atmospheric forcings of momentum, incoming radiation, temperature, humidity, and freshwater are provided to the ocean surface by bulk formulae following Large and Yeager (2009).***"

Is phytoplankton simulated only in the first oceanic layer or can they go further down the water column?

Phytoplankton concentration is simulated along the 75 levels of our numerical ocean. PISCESv2 (Pelagic Interactions Scheme for Carbon and Ecosystem Studies volume 2) is a 3D biogeochemical model which simulates the lower trophic levels of marine ecosystems (nanophytoplankton, diatoms, microzooplankton and mesozooplankton), the biogeochemical cycles of carbon and of the main nutrients (P, N, Fe, and Si). The revised manuscript clarifies this as follows (lines 199-201):

"***PISCESv2 simulates prognostic 3D distributions of nanophytoplankton and diatom concentrations. The evolution of phytoplankton biomasses is the net outcome of growth, mortality, aggregation and grazing by zooplankton (Aumont et al., 2015).***"

A comprehensive presentation of the model is found in Aumont et al. (2015).

Additionally, I couldn't really understand the description of the simulations from the text. However, from Fig.1 it is clear that the authors run a spin-up for 2000 years and then run their simulations for ~~18~~ **20** years only. In total, they run their model for 2018 years. Is the model in steady state?

2000 years correspond to the accumulated spin-up of all the simulations we started from the older ones in our modelling group. We mentioned it in the first draft to certify that our model was in steady state. We agree that the presentation of the simulation protocol can be improved. To clarify we rewrote this part as follows (lines 184-190):

"***JRA55-do atmospheric reanalysis (Tsujino et al., 2018; Tsujino et al., 2020) provided the atmospheric forcings of the ocean. The global domain was first spun-up under preindustrial conditions during several hundred years ensuring that all fields approached a quasi-steady state. The historical evolution of atmospheric CO_2 and N_2O concentrations was prescribed since 1850. To avoid the warming jump between the end of the spin-up and the onset of the reanalyses in 1958, the first 5 years of JRA55-do forcings were cycled, followed by the complete period of JRA55-do atmospheric forcing from 1958 to 2018."***

Maybe the authors should also state that their results are the average of the last 10 years of the simulations.

We agree and we added the following sentence in section 2c (lines 281-282):
"***In the following temporal means cover the last 10 years of simulations, from 2009 to 2018. In other analyses the whole simulated period is shown (1999-2018).***"

In their simulations, do they prescribe the atmospheric CO2 and N2O concentrations or do they prescribe atmospheric CO2 and N2O emissions?

This point has been clarified in the revised manuscript at lines 187-188: "***The historical evolution of atmospheric CO_2 and N_2O concentrations was prescribed since 1850***".

Does the ecosystem modelled consider only bulk phytoplankton or does it consider also e.g., cyanobacteria, diatoms? Is phytoplankton growth limited by light, nutrient and temperature?

PISCESv2 considers nanophytoplankton and diatoms. Phytoplankton biomasses experience growth, mortality, aggregation and grazing by micro- and mesozooplankton. Growth rate mainly depends on the length of the day, the depth of the mixed layer and the depth of the euphotic zone (defined as the depth at which there is 1‰ of surface photosynthetic available radiation). Light absorption by phytoplankton depends on the waveband and on the species. Normalized coefficients have been computed for each phytoplankton group by averaging and normalizing, for each waveband, the absorption coefficients published in Morel and Maritorena (2001). Nanophytoplankton growth depends on the external nutrient concentrations in N and P (Monod-like parameterizations of N and P limitations), and on Fe limitation which is modeled according to a classical quota approach. The production terms for diatoms are defined as for nanophytoplankton, except that the limitation term also include dissolved silica. Nutrient half-saturation constants vary with phytoplankton biomass of each compartment based on observations showing the increase in biomass to be due to the addition of larger size classes of phytoplankton. The aggregation term depends on the shear rate, as the main driver of aggregation is the local turbulence. The diatom aggregation term is increased in case of nutrient limitation because diatom cells are reported to excrete mucus under nutrient stress which increases their stickiness.

We expanded the model description in the revised manuscript (lines 199-202):

"*PISCESv2 simulates prognostic 3D distributions of nanophytoplankton and diatom concentrations. The evolution of phytoplankton biomasses is the net outcome of growth, mortality, aggregation and grazing by zooplankton (Aumont et al., 2015). Light absorption by phytoplankton depends on the waveband and on the species (Bricaud et al., 1995).*"

I think it would also be good to give the absorption coefficients used to parameterize PLF or directly give the equation of PLF.

We agree, thanks. This is now indicated in the manuscript lines 202-215:

"A simplified formulation of light absorption by the ocean is used in our experiments to calculate both the phytoplankton light limitation in PISCESv2 and the oceanic heating rate (Lengaigne et al, 2007). In this formulation, visible light is split into three wavebands: blue (400–500 nm), green (500–600 nm) and red (600–700 nm); for each waveband, the CHL-dependent attenuation coefficients, $\boldsymbol{k_R}, \boldsymbol{k_G}$ and $\boldsymbol{k_B}$, are derived from the formulation proposed in Morel and Maritorena (2001):

$$\boldsymbol{k_{WLB}} = \sum_{\boldsymbol{\lambda_1}}^{\boldsymbol{\lambda_2}} \left(\boldsymbol{k(\lambda)} + \boldsymbol{\chi(\lambda)}[\mathbf{CHL}]^{\boldsymbol{e(\lambda)}} \right)$$

where WLB means the wavelength band associated to red (R), green (G) or blue (B), and bounded by the wavelengths $\boldsymbol{\lambda_1}$ and $\boldsymbol{\lambda_2}$ as detailed above. $\boldsymbol{k(\lambda)}$ is the attenuation coefficient for optically pure sea water. $\boldsymbol{\chi(\lambda)}$ and $\boldsymbol{e(\lambda)}$ are fitted coefficients which allows to determine the attenuation coefficients due to chlorophyll pigments in sea water (Morel and Maritorena, 2001)."

Section 2.b

The paragraph line 240-252 would better fit in the introduction.

To comply with the demand of the other reviewer we moved the whole section 2b to the supplementary material. We choose to keep the paragraph describing N2O production in our "N2O parameterization" section (now in supplementary) because it highlights how the model parameterization represents N2O processes compared to what happens in the real ocean. The "N2O part" of our introduction aims to explain the spatial coincidence between N2O emissions regions and CHL, as well as the current uncertainties in N2O modelling, in order to introduce the need to explore how the PLF may affect marine N2O inventory. We do think that moving the paragraph describing N2O production to the introduction could sidetrack the reader from our main message.

Section 3.a

I would suggest to put the chlorophyll maps in the results section rather than in appendix because it helps to understand to results. Especially, PLF affects the OHC via chlorophyll concentration so it might be easier to follow and easier to understand if the chlorophyll concentration maps are directly in the results section.

Agreed, Figure S1 has been transferred in the main text as Figure 2.

Furthermore, it seems that the authors use their two simulations with incomplete representation of PLF (chl_zcst and chl_zvar) as upper and lower limit of their uncertainties (line 403-405). I wonder why they think that?

In fact, we did not use them arbitrarily as upper/lower bounds. Fig 1 shows that time series of OHC300 from climZCST and climZVAR surround our control experiment: "However Figure 1 highlights that the simulation using a consistent CHL for interacting with both incoming SW and biogeochemical cyclings (REF) does not amplify one of these two trends, as climZCST and climZVAR surround REF". The analysis revealed them as upper/lower bounds for ocean heat content and we used them as such.

Do they consider that chl_zcst is a simple simulation giving the minimum OHC300 while they consider chl_zvar as a complex simulation (close to reality) giving the "real" OHC300? And that REF is somehow fluctuating between these two simulations?

No, due to their use of an incomplete PLF, we consider that both climZCST and climZVAR simulations are more simple configurations than our control run REF. REF is the complex simulation.

Section 3.b

As in Sweeney et al. (2005), the authors state that small changes in chlorophyll concentration drives important changes of the mixed layer depth in the subtropical regions (line 447-453).

However, they do not explicitly explain what are the mechanisms behind these changes in MLD. Is it due to PLF? What is the link between chlorophyll and MLD? I think what the authors want to say is that changes in chlorophyll drive changes in temperature via PLF, which in turn drives changes in thermocline/pycnocline and thus in MLD.

Yes in section 3a we first show that changes in CHL drive changes in temperature and ocean heat content. Next, Figure 4 illustrates that these thermal perturbations are associated to dynamical ones. Finally in section 3b we insist on the fact that the heat and subsequent dynamical modifications are associated to pycnocline perturbations (lines 384-385): "Perturbations of the annual pycnocline depth (Figure 5, a-c) highlight a vertical adjustment to the heat (Figure S2) and subsequent large-scale dynamical anomalies (Figure 4)." It is our assumption that perturbations of the isopycnal layers detected in the pycnocline/thermocline (Figure 5) give evidences of MLD perturbations. This result is consistent with the MLD perturbations due to small CHL changes in subtropical gyres highlighted by Sweeney et al. (2005).

The authors state that the decrease/increase in N2O concentration is driven by different mechanisms depending on the region studied. For instance, the decrease in N2O concentration in the South Pacific is due to increased temperature, enhanced circulation and deepening of the pycnocline. But in the North Atlantic the increase in N2O concentration is only due to the shoaling of the pycnocline. Additionally, in the North Pacific, the decrease in N2O concentration is due to the higher O2 concentration. Why do we have different mechanisms involved at different places of the world? For instance, why does O2 concentration is important for the North Pacific region but not for the North Atlantic or South Pacific regions?

Equation 3 in section A of the supplementary material allows to understand the different mechanisms involving N2O production (remineralization, grazing and nitrification) and depletion. These mechanisms vary in space and time, and do not occur at all places of the 3D ocean with the same intensity. In addition, the local suboxic production of [N2O] is adapted to the local oxygen concentration (through the f(o2) function, please refer to equation 1). This explains the specific regime seen in the oxygen minimum zone of the North Pacific.

N2O concentration reflects both local and advected production/depletion. Depending on the region the role of the circulation differs. The goal of this paragraph is to show that N2O concentration is driven by regional features which are a combination of transport, stratification and biogeochemical processes.

Section 4

In conclusion, the authors detail some results about oxygen without showing them previously. I think they should talk about these results in the result section rather than showing them suddenly in the conclusion section.

We consider that the paragraph about OMZ volume is a discussion which extends the significance of our study. In that perspective we renamed the last section "Discussion and conclusion". (Note that we replaced all OMZ shorthand notations by the whole expression, as asked by the second reviewer).

Furthermore, the authors should also discuss the fact that their model setup does not consider an atmospheric component. Do they expect similar results if they use a coupled ocean-atmosphere model rather than using an atmospheric N2O and CO2 forcing? For instance, Asselot et al. (2022) show that PLF affects the climate system mainly via sea-air greenhouse gas fluxes. Adding an interactive atmospheric component could therefore lead to higher greenhouse gases in the atmosphere, increasing the atmospheric temperature. The higher atmospheric temperature might increase the oceanic temperature and thus enhance the effect of PLF on N2O fluxes.

We agree that the use of a forced ocean model may limit the response. This issue is now discussed, with the following paragraph that has been added to our conclusion section (lines 515-523):
"*In forced ocean simulations, atmospheric forcings constrain surface temperature, salinity and thus solubility. However, the N2O concentration integrated over the upper 300 meters depth of the water column (Figure 5, e-f) showed differences with the control run that follow those of the in-depth temperature (Figure S2, c-d): in climZCST (climZVAR), a warmer (colder) tropical ocean leads to a decreased (an increased) N2O concentration. Because higher marine greenhouse gas emissions will increase the temperature of the coupled atmosphere-ocean system, adding an interactive atmospheric component is expected to amplify the PLF-induced mean changes in marine N2O concentration highlighted in this ocean-only numerical set (Park et al., 2014; Asselot et al., 2022)*."

Specific comments

Line 17: Replace “thanks to” by “by”.
Done.

Line 22: Replace “experiments” by “simulations”.
This comment is valid for the entire manuscript.
Done.

Line 23: Replace “have been performed” by “are performed”.
The sentence has been rephrased as follows (line 23): "*We exploit global sensitivity simulations at 1-degree of horizontal resolution over the last two decades (1999-2018) coupling ocean, sea ice and marine biogeochemistry*".

Line 34-35: Replace “shine a light on a current uncertainty of the modelled marine nitrous oxide budget in that climate models.” by “shine light on current uncertainties of modelled marine nitrous oxide budget in that climate model.”
We rephrased as follows (line 34): "*Our results based on a global ocean-biogeochemical model at CMIP6 state-of-the-art shed light on current uncertainties in modelled marine nitrous oxide budgets in climate models".*

Line 41: Replace “suffers” by “undergoes”
We rephrased as follows (line 41): "*This natural effect is either not represented in the ocean component of climate models, or included in a simplified manner*".

Line 43: Replace "uncertains the forecast" by "leads to uncertain forecast"
We rephrased as follows (line 44): "...***which in turn leads to uncertainties in projections of oceanic emissions...***".

Line 92-94: The authors state that "Two main causal chains have been proposed to interpret the sign of the final heat perturbation" but they do not give these two causal chains.
That was the idea of the rest of the sentence. We rewrote the paragraph as follows (lines 94-98): "Two main causes were put forward to explain the sign of the final heat perturbation: ***either an indirect dynamical response (Murtugudde et al., 2002; Löptien et al., 2009) or a direct thermal effect (Mignot et al., 2013; Hernandez et al., 2017). Hernandez et al. (2017) further distinguished a local from a remote thermal effect by highlighting the important role played by the advection of offshore CHL-induced cold anomalies in the Benguela upwelling waters."***

Furthermore "causal chains" could be directly replaced by "causes".
Done.

Line 132: I guess the authors are speaking about atmospheric emissions here.
No we are still discussing oceanic emissions. As written just before, 20% of the annual N2O flux occurs in the coastal upwelling systems that are undersampled by observations. Figure 1A of Yang et al. (2020) presents an up-to-date map of spatial distributions.

Line 132: Which decade?
In the revised manuscript we specified " The recent global budget of Tian et al. (2020) estimates natural sources from soils and oceans to contribute with up to 57% to the total N_2O emissions ***between 2007 and 2016***, with the ocean flux reaching 3.4 (2.5–4.3) Tg N yr^{-1}."

Line 136: Replace "model marine" by "simulate marine"
Done.

Line 157: Parameterization.
The sentence has been deleted because the section has been moved to the supplementary material.

Line 202-203: Rephrase please
Done. "These two simulations differ from each other by the "realism" of the vertical profile derived ***in each grid point*** from the ***surface value of the*** ESACCI CHL climatology ***to the level of light extinction*** (Table 1). ***climZCST uses constant profiles of CHL spreading uniformly in the vertical direction (Figure 2, b and d-f). climZVAR uses variable vertical profiles computed following Morel and Berthon (1989) (Figure 2, c and d-f)***."

Line 213: Replace "biogeochemical element cycling" by "biogeochemical cycles".
Done .

Line 325: Replace "by the Ifremer" by "by Ifremer".
Done.

Line 332-337: The authors compare their modelled temperature and oxygen with several database. Is it shown somewhere? In figures or appendix?
Yes, in figure 3 (temperature), and supplementary figures S2 (temperature) and S5 (oxygen).

Line 388-392: I cannot say what the authors mean with these two sentences.

The sentence has been rephrased as follows (line 334): "***It can be expected that experiments having spin-ups run with different representations of the PLF, would give even stronger sensitivities than those highlighted in this study. The sensitivities of OHC300 to the PLF formulation evaluated here should be considered at the lower end of estimate of OHC discrepancies that may emerge from changing the PLF representation."***

In other words, we may expect that a simulation in which even the spin-up period would have been run with an incomplete PLF will show much greater difference in OHC300 with our control simulation (REF), as the deviation increased along time (and spin-up period encompasses a long period).

Line 393: green on Figure 1?
Yes that was the initial idea, but colors indications have been deleted from the text to improve readability.

Line 394: directly use OHC300 as it is previously used.
Done.

Line 400: blue on Figure 1?
Yes that was the initial idea, but color indications have been deleted from the text to improve readability.

Line 427: Replace "modifying" by "different".
Done.

Line 442: Replace "raising" by "shallowing".
It is true that it corresponds to a "shallowing of the isopycnals", but what we want to highlight here is their rise to the surface. We reformulated as follows (line 389): "as it triggers an upward displacement of the isopycnals ".

Line 445-447: "Over these ... the pycnocline" please rephrase.
We rephrased by splitting the sentence as follows (line 392): "Over these subtropical gyres heat is redistributed along the vertical as the subsurface warm anomaly dives. ***The subduction of these heat anomalies causes*** in turn a deepening of the pycnocline (**Error! Reference source not found.**, b and c)."

Line 460: Replace "upper line" by "upper panel".
Done.

Line 466-469: Please rephrase.
The sentence has been deleted.

Line 474: Replace “By contrast” by “In contrast”.
Done.

Line 484: Remove “next”.
Done.

Line 484-486: “Approaching … heat uptake” please rephrase.
For clarity we splitted the sentence as follows (line 428):
"The relationship between N_2O concentration and OHC300 in the Tropical Ocean is derived from a linear regression for each of the three 20-years simulations (**Error! Reference source not found.**Figure 6). ***The resulting slopes allow to identify three distinct tropical N_2O production pathways along time as a function of the oceanic heat uptake.***"

Line 500: Should be section 3.c rather than section 3.d
Done.

Line 502: Replace “the degree of realism of the PLF” by “the way PLF is simulated/modelled”.
Done.

Line 504-507: Please rephrase because it seems that you consider that Dpn2o anomalies reflect only differences in surface N2O concentration while Dpn2o anomalies can also reflect differences in N2O solubility.
Solubility is mainly driven by temperature and salinity. Figures 3 and S3 show that the majority of temperature and salinity anomalies in climZCST and climZVAR compared to REF are located below 25 meters depth. We have almost no solubility perturbations close to the surface. This explains why spatial patterns of Dpn2o perturbations are similar to that of surface N2O concentration. To clarify we inserted a new sentence at lines 449:

"Because the atmospheric partial pressure of N2O is identical among simulations, differences in Dpn2o are driven by changes in surface N2O concentration normalized by those in N2O solubility. ***Since solubility is mainly driven by temperature and because surface temperature anomalies are very weak (Figure S3, c and d), we do not expect solubility perturbations close to the surface.*** It results that spatial patterns of Dpn2o anomalies (**Error! Reference source not found.**) reflect differences in surface N2O concentration."

Line 512-513: Are the units atm or natm?
natm, thank you for detecting the typo.

Line 548: Remove “and”.
Done.

Line 551: Replace “add” by “added”.
Done.

Line 552: Remove “, and”.

Done.

Line 552: Remove “with time”.
We replace "with" by "along".

Line 556: Replace “their” by “a”.
Done.

Line 591: Replace “so why it used” by “that is why it uses”
Done.

Figure S4: I don’t understand how densities (y-axis) can be negative. Is it a density anomaly represented?
Yes, densities are plotted as anomalies compared to 1999. The following sentence has been added to the legends of Figures 6 and S4 in the revised manuscript: "***All points reflect anomalies compared to year 1999***".

Figure 3a: The colorbar can be improved.
Done.

Thank you for considering my input to your research.
Rémy Asselot

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