

Review 2 of Berthet et al. – How does the phytoplankton-light feedback affect [the](#) marine N2O inventory?

The authors have carefully revised their manuscript in response to the reviewer comments, and I believe the manuscript has improved substantially as a result and is ready to be published with a few minor revisions. Below I focus on the responses to my own comments and on the revised manuscript. I just have one major comment concerning the steady-state of the model and if this comment is adequately addressed in the revised manuscript, the manuscript can be published.

Thank you for your positive review. We have addressed your comments below. Please find our detailed response (in blue) to your comments (black text). In our responses, **lines numbers** are relative to the new manuscript.

Major comments

Looking at the response to my previous review and from the revised manuscript, the authors clearly state that their model is in steady-state. I understand that the spin-up is in steady-state after running for 2000 years but I really question the fact that after 20 years of simulations, the model simulations are in steady state. A good argument to say that their simulations are not in steady-state is Fig. 1. This figure shows an increase of OHC300 over the whole simulations (1999-2018). If the model would be in steady-state during the “simulations period”, the OHC300 would be constant and not fluctuate anymore after a certain period of time. I think the simulations are too short (only 20 years) to reach a steady state. This point should be clearly stated in the manuscript.

The protocol followed in this study is the classical approach used for all climate hindcasts: first a long spin-up is run to allow the system to reach a steady-state (until 1957 here, see Fig. R1), and then a "transient climate" simulation is forced by historical evolving forcings in order to model the state of the system over the recent period (1958-2018 in this study). The spin-up phase brings the system to an equilibrium in order to avoid intrinsic model drifts during the transient phase.

So we agree that the model is not in steady-state over the period 1958-2018, as this is the transient phase of our simulation. Over this period our ocean-only model is driven by external radiative forcings (JRA55-do) that affect the OHC. Because the global warming is included in these JRA55-do forcings, the positive trend seen in the tropical OHC300 in Fig. 1 is expected over this period. We advocate that the fact that we start from a long spin-up run under constant forcings (through a loop over a specific early period) implies that OHC (and more generally, the ocean) has reached an equilibrium in 1957 (see Fig. R1). Then by applying the observed atmospheric forcing from 1958 onwards, we run a transient climate, and the trend seen in the simulation results primarily from the forcing and does not reflect an intrinsic ocean model drift. The fundamental point is to ensure that we start the transient simulation from a steady ocean in order to not include numerical drifts in the modelled trends after 1958.

Thus it is only during a spin-up period that the ocean heat content (OHC) is expected to reach a stage and stay constant, because by definition a spin-up uses constant forcings (or a loop over a specific early period). By definition, the transient phase is expected to not be in steady-state, and it would be weird to mention it.

Moreover, is PLF taken into account in the spin-up ?

Yes (see Fig. R1), this was stated [line 194](#) of the previous manuscript version: "The control simulation (hereafter REF) together with the spin-up both account for a fully interactive PLF".

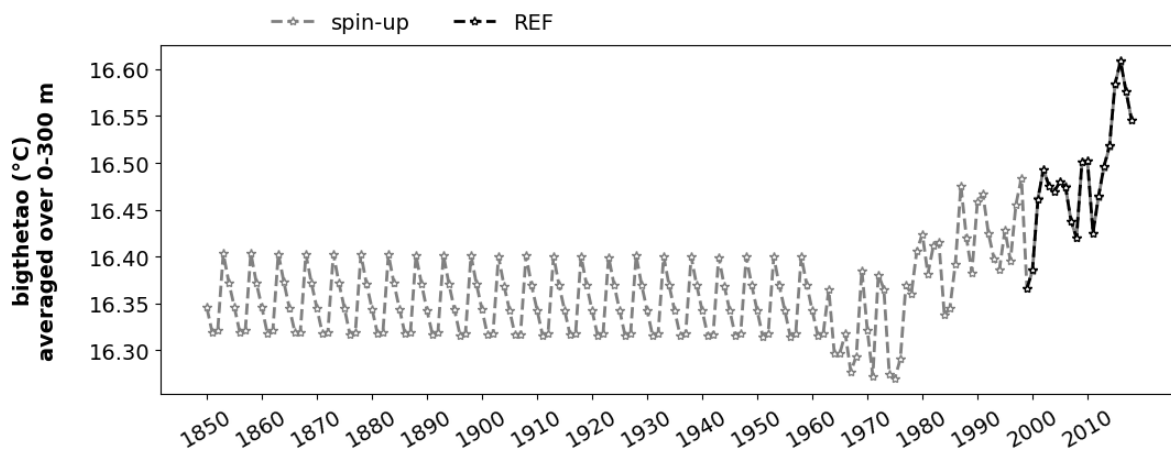


Fig. R1: Mean modelled global temperature averaged over the first 300 meters depth. The last spin-up period is shown in gray and REF is in black. The last period of the spin-up (1850-1957) shows that our modelled ocean is in steady-state. From 1958 the complete JRA55-do atmospheric forcings are applied (this is the transient phase), as shown by the clear positive trend which is the signature of the ocean global warming.

Minor comments

The line numbers correspond to the tracked changes version of the manuscript.

Line 34-36: Please rephrase.

Line 41: Replace "effect" by "process".

Line 43: Replace "biophysical" by "biogeophysical" and same for the rest of the manuscript.

Line 44: Replace "which leads" by "leading".

Line 67-69: Replace "is consistent" by "is the same as".

Line 75: Replace "included to" by "included in".

Line 78: Replace "affected" by "affects".

Line 91: Replace "than to the" by "than the".

Line 92-93: Replace "amplifies the mean of PLF-induced changes, but without altering the sign" by "amplifies the magnitude of the PLF-induced changes, without altering the sign".

Line 107-108: is the remote cooling effect at the surface or at the subsurface ?

At the surface.

Line 122: 265-298 times higher ?

Line 257: Replace “consistent” by “identical”.

Line 261-262: Please rephrase.

Line 285: Replace “the year” by “when”.

Line 287: Replace “OHC of deeper layers” by “OHC in the deeper layers”.

Line 297-298: I am not sure by these two lines. Are your results detailed following two different time periods ? One time period covers 10 years (2009-2018) and a second one covers a longer period (1999-2018) ?

Yes time series (Fig. 1, Fig. S3, Fig.4, Fig. 6) present analyses over the whole simulated period (1999-2018).

Line 326: Replace “comparable to that of our” by “comparable to our”.

Line 329: Replace “accordance” by “agreement”.

Line 331: Replace “order” by “about”.

Legend Figure 3: I would say that the 60°S-60°N is not the tropical band anymore. I would encompass that you calculate the average over the tropical and mid-latitudes band.

Line 335: Replace “branching of” by “switching on”.

Line 338: Replace “weaker profiles” by “smaller/weaker concentrations”.

Line 341: Replace “by comparison” by “compared”.

Line 351: Replace “spin-ups run” by “spin-up runs”.

In this sentence “run” is a verb.

Legend Figure S4: I am confused by the units. The potential density (y-axis) should be in kg/m³ as in the main text (line 360). The OHC300 (x-axis) should be in ZJ and not ZJ/yr.

This is effectively a mistake, thank you for detecting it ! Fig. S4 presents annual density integrated over the vertical, so why it is in kg/m², but not in kg/m²/yr. Line 360 has been corrected accordingly. In addition units of the annual mean of OHC300 in the legend of Fig. S4 have also been corrected [ZJ].

Line 360-362: The sentence can be reduced and directly state “The opposite trend (a reduced OHC300 compared to REF) is simulated when considering a variable vertical profile of CHL (climZVAR).”

Line 362-365: This sentence can be reduced as well, giving “However Figure 1 highlights that the simulation REF does not amplify one of these two trends, as climZCST and climZVAR surround REF.”

Line 370: Replace “Ranges of uncertainty” by “uncertainty ranges”.

Line 373-375: Please rephrase.

Legend Figure 6: The units of N₂O should be μmolN/m² and the units of OHC300 should be ZJ.

Thank you, the /yr have been removed.

Line 446: Replace “along time” by “through time”.

Line 449: Replace “appear a” by “appears as a”.

Line 464: Please define Dpn_{2o} as it is the first time it appears in the main text.

Dpn_{2o} is defined line 294.

Line 466: I guess you mean surface oceanic N₂O concentrations.

Yes, of course. As we run ocean-only experiments, we have no atmospheric variables (except in the forcing files). But for clarity we added "oceanic".

Line 479-481: I don't get the point of this sentence, there is no conclusion. Does the fact that the global N₂O budget of Tian et al. (2020) is only based on five global ocean-biogeochemical, weakens the estimates of Tian et al. (2020) ? Or does this fact mean that the N₂O budget of Tian et al. (2020) means that this budget has high uncertainties ?

Yes, the fact that the budget has been estimated from a small number of ocean-biogeochemical models having different PLF representations add an important uncertainty to it. This point is explained **lines 479-483**: "*As a matter of fact, the oceanic contribution to the recent global N₂O budget by Tian et al. (2020) is based on only five global ocean-biogeochemical models (as still only few models simulate marine N₂O emissions). These models have different configurations of the PLF which adds considerable uncertainty to simulated marine N₂O emissions.*"

Line 487: Replace "in that regions" by "in these regions".

Line 489: The authors state that "regional N₂O fluxes by 24%" compared to REF. However, is this number true for climZVAR only, for climZCST only or is this number an average of the overestimation for both climZVAR and climZCST ?

No, it is not an average, the overestimation in each experiment (climZVAR and climZCST) reach 24% in the subtropical gyres of South Pacific and South Atlantic.

Line 506: Replace "experiments" by "simulations".

Line 515-518: Please rephrase by "The heat perturbations plus the uncertainty... N₂O production result in three N₂O production trajectories through time..."

Line 521: I think it's Northern Hemisphere with capital letters.

Line 528: Replace "experiments" by "simulations".

Line 531: Replace "In forced ocean simulations" by "In ocean-only simulations".

Line 550: Remove "in comparison to REF".

551-552: This sentence can be shortened. Replace "compared to that of the control run REF" by "compared to REF".

Thank you for considering my input to your research.

Rémy Asselot