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Discussions

Interactive comment on "Intermediate water masses, a major supplier of oxygen for the eastern tropical Pacific ocean" *by* Olaf Duteil et al.

Anonymous Referee #1

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1 Main Objective of this Study and General Comments

This study investigates the impact of intermediate water masses (IMW) and it's pathway and supply along Equatorial Intermediate Current System (EISC) on dissolved oxygen content in the Pacific Oxygen Minimum Zone (OMZ) (in the eastern tropical Pacific ocean). The authors utilized a suite of simulations to address these questions.

The manuscript consists of $i)$ mean state diagnostics and evaluations from suite of models (NEMO (ocean stand-alone simulation), UVIC (coupled, energy moisture balance model, forced wind stress), GFDL (coupled)) and $ii)$ sensitivity simulations (or transient simulations over 60 years) (oxygen restoring, conservative tracer release, and Lagrangian tracking of tracers)elucidating the role of subtropical IMW on dissolved

oxygen supply (through EICS) in eastern tropical Pacific ocean.

Despite the limitations (or discrepancies) in simulating properties of IWM in the current climate models, the authors did a nice set of simulations tackling how bias in IMW and EICS could impact on dissolved oxygen (and possibly impact on projections of OMZs due to climate change). This could provide insights on improving ocean biogeochemistry in ESMs and I think the work contains interesting and important results. However, I have several comments and some sections and figure presentations should be revisited before publication. Therefore, I suggest a major revision. I state specific comments below and hope this helps to improve the manuscript.

2 Major Comments

$[1]$ The heterogeneous subset of models (simulations) will be an advantage exploring model and resolution dependencies (as author stated in $L116 - 118$) on IMW characteristics and tracers (here dissolved oxygen) but also makes the results difficult to interpret to some extent. I still think the results will have impacts from not only the differences in model structures and resolutions, but also the forcing (forcing dataset, prescribed vs. coupled) and model integration time (spinup states) (some specific comment on forcing dataset is stated below). I would like to ask authors to discuss further on these points since for example, the wind and buoyancy forcing bias could be one of the reasons introducing errors in climate (and ocean) models as stated in the introduction.

$[2]$ Regarding to sensitivity of tropical IWM oxygen to subtropical and deep dissolved oxygen levels, the authors refer AAIW, NPIW (and the upper part of the PDW) as IWM in this study. I was wondering what will be the relative contributions of each water masses to dissolved oxygen supply, ventilation in the eastern tropical Pacific ocean (particularly North (NPIW) vs South (AAIW)). My impression is that AAIW could be more dominant

(e.g. Talley, 2013) but I would like to know what sensitivity simulations indicates. At least, I think it is possible to obtain insights from the Lagrangian tracking diagnostics (or if possible, conducting additional restoring simulations with 30°S boundary only for example).

$[Reference]$ Talley, Lynne D., (2011), Descriptive Physical Oceanography: An Introduction, Academic Press.

$[3]$ The core of the study is based on a suite of sensitivity simulations from NEMO (NEMO2). In the first reading, I struggled a bit on connecting aim and each sensitivity experiments. The dissolved oxygen restoring simulations aim on investigating sensitivity of tropical IWM oxygen to subtropical and deep dissolved oxygen levels (as stated in section 3.2) and the conservative tracer release simulations are more dedicated to investigate spreading of tracers towards the eastern tropical Pacific (transport by the EICS, as stated in section 4.1).

While the standard structure of the manuscript is to introduce overall data and methods in the beginning, (section 2), I suggest to move some of the objective and details of sensitivity experiments to each corresponding sections (referring to sections 3.2 and 4.1) so it is much easier to follow the aim bridging to sensitivity experiments (I think it is still fine to keep brief general descriptions in section 2 including Table 1). Alternatively, the methods section could be revised to include additional descriptions connecting to corresponding result sections. I will leave this decision to the authors regarding to the structure of the paper but I think the flow could be improved.

$[4]$ Another major issue is the figures. Figure labels and captions are not easy to interpret (and in some part, the authors are referring to figure does not appear, $e.g. L267, Fig.4i$). Therefore, I suggest the authors to carefully revisit all the figures and add necessary caption, labels for better presentation. For example, for time series plots ($e.g. Fig.2, 3g-i, 8$), the difference in color (models, configurations etc.) should also be informed in the label (not just in figure captions) because it is not easy to follow.

Similar issues for multiple maps (such as $Fig.5$), it will be reader friendly to label maps with "zonal advection", "meridional advection" etc. Also, some of the model names (labels) are not obvious because those are overlaid on color shading ($e.g. Fig.9$). I put few more specific suggestions below and hope this helps to point out the difficulties I am referring to.

$[4.1]$ $Fig.1$ caption, ($L762-763$) oxygen levels (mean 500 - 1500m) at 160W, I think color shading in $b)$ is not vertical mean (because it is depth-latitude section). Also, is dissolved oxygen in $Fig.1$ from observations such as World Ocean Atlas?
$[4.2]$ Regarding to $Fig.4$, I have several suggestions to improve figure presentation. I am still a bit confused what is in color shading and contours. For example, in $L789$, it states the vertical current as contour in $c)$ but the contours do not look like vertical current values. Also the continent shading in $g)$ is missing (no gray shaded). Similar confusion occurred to me in other panels and I suggest to revisit and clearly state what is presented in color shading and contours for each panels with units. Also, why did you only present the results from NEMO2-30DEG (not including NEMO2-30DEG1500M or NEMO2-30DEG1500M minus NEMO2-30DEG)?
$[4.3]$ Add information labels for $Fig.7a)-c)$ the first release, and $d)-e)$ the second release, respectively.
$[4.4]$ Add information labels (like figure title) for $Fig.9$, zonal sections and meridional sections, respectively.

3 Minor Comments

$[1]$ I am curious whether CORE v2 climatological forcing (used for NEMO) and NCEP/NCAR climatological forcing (wind stress, used for UVIC) makes a difference in

spinup states. As far as I know, CORE v2 forcing is based on NCEP/NCAR reanalysis but it has several corrections and adjustments in the forcing and difference between the two could lead to different results, particularly after long-term spinup. Do authors think this is a minor thing?

[2] Are all the GFDL model simulations integrated for the same period following high-resolution (GFDL01) for comparison (I assumed 200 years from Busecke et. al., 2018) or the low-resolution configurations are integrated for longer durations?

[3] Because of the high resolutions configurations for GFDL01, the integration time is limited but does this impact on IWM (and upper part of PDW) characteristics and tracers (i.e. insufficient spinup, drift in certain properties etc.)? Upper ocean could be quasi-equilibrated (say few hundred meters) but I am wondering about mid $\sim$ deep ocean you are more focusing on in this study.

[4] Regarding to dissolved oxygen restoring, are the boundaries (and depth interface at 1500m) all in the Pacific ocean only (e.g. thinking of for example, 30°N and 30°S zonal walls and 1500m layer in the entire Pacific ocean) or globally? Also, how strong (i.e. timescale) is the restoring in these simulations?

[5] Regarding to the respiration rate (in $L144$), did you set all the simulations respiration rate (similar to fixing oxygen utilization rate I would assume) to NEMO2-REF?

[6] I am a bit confused by the locations of particle release and IETP/IWTP regions you were referring to ($L363 - 383, Fig.7 \, and \, 8$). While the the locations of particle release is in sections (shown as black bold lines (or dot) in $Fig.7$), I thought the IETP/IWTP are basins in specific rectangles and this is different from the locations of particle release (it contains of course) if I understand correctly. If that is the case, I suggest to revise the main text and Figure to include these information more explicitly (I think adding boxes in $Fig.7$ could help and you can refer to that interpreting $Fig.8$).

[7] Just for clarification: do ocean stand-alone simulations (i.e. NEMO and UVIC)

also use preindustrial pCO_2 for spinup (related to mean state diagnostics)?

[8] In section 2.1, Table 1, and part of the main text: The author mix use the NEMO and NEMO2 through the manuscript and I have got a bit confused. Since all the simulations use NEMO2, you should make the terminology consistent through the text after introducing (or just NEMO, I will leave this to the authors).

[9] For Table 1, I would suggest to include model integration time information.

3.1 Line Specific Comments

[L70] Cabre et l., : should be Cabre "et al.,"
[L85] eastern tropical (20°S-20°N): I think you should add longitude information since you mentioned "eastern" tropical Pacific.
[L104] (see Keller Keller 2012 for ... : delete "Keller" (duplicates).
[L124] more than 50 years: suggest to change to "60 years" (the same as the statement in latter section, $L160$).
[L167] 5 daily means: I think "5-day mean" is more common.
[L262 – 263] Where is the information (figure) of total advective term? Fig. 4g is the vertical advection term difference and I could not find specific information on total term in the figure (although it is possible to infer from all the terms).
[L301] Tsuchuya jets: should be "Tsuchiya jets".

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