The Reviewers comments (Reviewer 1 and Reviewer 3) are reproduced below in red. We reply in black. The line number corresponds to the line in the track-change file

Reviewer 1
First of all, my apologies in the previous review comments that I made typos in acronyms, "IWM" (Intermediate Water Mass) by IMW. Hope the comments was clear enough to explain the points. I thank the authors for conducting a suite of additional simulations addressing the impact of the atmospheric forcing, model integration time, and oxygen forcing simulations.

The authors have done substantial amount of additional simulations and revision and I think the additional results (figures in response and supplementary materials) addressed the questions from my comments. They showed that the impact of atmospheric forcing and spin-up state exists but does not affect the question and conclusion at least in this study (i.e. strong coupling between subtropics and tropics).

The message in the summary session is organized and I think the results support the conclusions.
We thank the reviewer for her/his positive evaluation.

I still have few comments that I would like to ask the authors to address before publications. The comments are mainly about the presentation and clarification of the paper and most of them are relatively minor comments.

1. I thank the authors for stating "conceptual reasoning" in the response. I understand why the authors choose to utilize a suite of models and I think it is useful to combine a subset of the models and a single model sensitivity simulations to dig into further mechanisms. The basic presentation style of this paper is to present analysis from a subset of models (UVIC, NEMO2, and GFDL models) to explore the representations of IWM, EICS, and dissolved oxygen levels and then show sensitivity simulations (NEMO2) to explore mechanisms, which I think is good.
We thank the reviewer for her/his comment.

The only part I have got stuck a bit is the section 4.3, which comes back to the result from the subset of models. The authors stated in the comment "investigating mechanisms in a heterogeneous set of models by performing dedicated sensitivity simulations" but the analysis and discussion I see is the analysis of MKE (Mean Kinetic Energy) and dissolved oxygen (i.e. more of the analysis rather than sensitivity simulations).
The MKE discussion itself is good but I also had to look into the caption in Figure 9 carefully to see the results from UVIC GD13 (and this was not completely clear from just reading the section 4.3).
I am not sure what the authors meant by "performing dedicated sensitivity simulations" from a heterogeneous set of models (because I only see sensitivity simulations from NEMO2) and I kind of see a slight jump from the previous sections.

We indeed stated in our reply to the reviewer "In summary, we investigate the mechanisms impacting tropical oxygen levels at intermediate depths in a very heterogeneous set of models, by performing dedicated sensitivity simulations that are easy to interpret". This sentence does however not appear in the manuscript. Rather than applying specifically to part 4.3, this sentence was meant to summarize briefly our conceptual thinking. We perform sensitivity experiments using NEMO2 (part 3), NEMO05 and NEMO01 (part 4). The above sentence could be rephrased: "In summary, we analyze the tropical oxygen distribution at intermediate depth in a very heterogeneous set of models. In order to better understand the mechanisms at play, we use a single model framework to perform additional sensitivity experiments."
Section 4.3 has been rewritten for clarity reasons (see below)

As the authors stated in L413 in a short statement, a suite of tracer release and Lagrangian tracking simulations could help interpreting the subset of the models so I suggest to include more statement on this part (perhaps including MKE and dissolved oxygen from NEMO2 sensitivity simulations help along with Figure 9?) Also, please make it more clear that you are showing the results from UVIC GD13 (perhaps pointing out the specific panel and mention blue contours, label all the panels alphabetically or just point out "UVIC" panel on the left column). I think the material presented is nice, it is more about the presentation and discussion to make a nice flow from sensitivity simulations back to a subset of models. I also think including results from UVIC GD13 is useful for discussion so it will be good to keep it in (it was just a bit unclear in the text and figure captions).

The part 4.3 has been completely rewritten. We took in account reviewers's comments (in particular regarding the clarity of the text)

"The experiments discussed in 4.2 were not coupled with biogeochemical cycles for computational cost reasons. In order to assess the robustness of our findings (EICS plays a large role in setting tropical oxygen levels), we next analyze equatorial oxygen in a set of climate models similar to CMIP models. To this end we use the GFDL model suite, characterized by a resolution increase (GFDL1, GFDL025 and GFDL01 - see Table 1).

The striking difference between GFDL01 and GFDL025 / GFDL1 are the high oxygen levels in the eastern part of the ocean below 1000 m in GFDL01 compared to GFDL025/GFDL1 (Fig 2). The oxygen levels show weaker zonal gradient in GFDL01, consistent with the tracer experiment that we performed in 4.2. and a more ventilated intermediate equatorial ocean. High values of mean kinetic energy are associated with higher oxygen values (Fig 9). This is particularly clear in GFDL01 at around 1500 m depth, where strong values of MKE are present and form the "bottom" of the low oxygen volume (oxygen lower than 50 mmol.m-3). Conversely GFDL025 and GFDL1 do not present high MKE values below 1000 m in the eastern part of the basin; the low oxygen volume extends till depths greater than 2000 m. It suggests that intermediate currents participate in the ventilation of the eastern tropical ocean and thus in limiting the vertical extension of the OMZ.

Oxygen levels do not increase linearly with the currents strength, i.e while currents strength increase in GFDL1, GFDL025 and GFDL01, oxygen levels are relatively similar in GFDL1 and GFDL025 (see Fig 5 and Fig 9). The relatively small net balance between large fluxes of respiration and oxygen supply (Duteil et al., 2014) may be responsible for this behavior. If the supply is slightly higher compared to the consumption by respiration, it will lead to an increase of oxygen concentration. If it is slightly lower, the oxygen levels will decrease. A small difference in supply (e.g slightly weaker currents) may therefore lead to a large difference in oxygen levels when integrated over decades. For this reason, the impact of the EICS is more visible below 1000 m as the respiration decreases following a power-law with depth (Martin et al., 1987) and is therefore easier to offset even by a moderate oxygen supply.

Resolving explicitly the EICS results in a similar oxygen distribution to what Getzlaff and Dietze (2013) (GD13) achieved with a simple EICS parameterization (Fig 9a): to compensate for the "missing" EICS in UVIC, a coarse resolution model, they enhanced anisotropically the lateral diffusivity in the equatorial region. The oxygen levels from UVIC GD13 are shown in blue contours on top of the UVIC oxygen distribution (black) in Fig 9. Implementing this approach tends to homogenize oxygen levels zonally, with an increase of the mean levels by 30-50 mmol.m^{-3} in the eastern basin and a decrease of oxygen concentrations in the western basin. While this approach may be useful to better represent the oxygen mean state, it however does not take in account the potential variability and future evolution of the EICS. "

We chose not to include NEMO2 as the message here is specifically on the role of the intermediate current system and its impact on intermediate oxygen levels. The intermediate currents system is not represented in NEMO2. Instead we focus on two model subsets to address the question how changing the resolution or including a parameterization affects the equatorial oxygen transport in climate models.

As stated below by the reviewer, an issue in inter-model comparison are the compensating effects between oxygen supply / respiration. As a result, when physics are deficient, one could tune the biogeochemistry to achieve a realistic field (Duteil et al., 2012). This effect is limited when comparing solely GFDL1, GFDL025 and GFDL01 models as the biogeochemical model is the same. Similarly comparing UVIC and UVIC-GD13 highlight the role of the increased transport at depth. It is however not straightforward to compare NEMO2 and the GFDL models suite in this context. NEMO2 is characterized by a weak MKE but a relatively well oxygenated bottom layer (despite low oxygen levels at 30°S), pointing out an important role of biology to maintain the strong OMZ around 90°W.

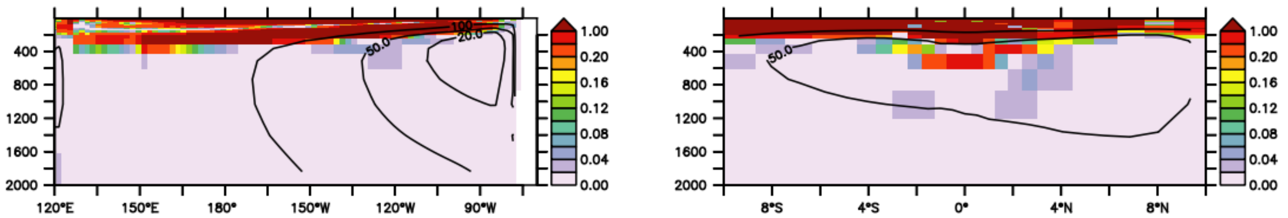

Fig : a - Mean Kinetic Energy (m2.s-2 x 1000) (average 10°N-10°S) in NEMO2, b - similar to a. but average 160°W- coast. Oxygen levels (mmol.m-3) are displayed in black contour.

2.I would like to ask the authors to include a short statement (or suggestions) for analyzing CMIP class multi-models in summary section. I think the authors have done nice analysis on heterogeneous set of models (with the aid of sensitivity simulations exploring further mechanisms) and the results can possibly point out what we should analyze to explore the multi-model characteristics (or bias) understanding tropical OMZs. I understand the additional difficulties digging in multi-models since the models include biological effect (compensation authors discussed) but do you think analysis like in Figure 2d or 9 supports on understanding IWM and EICS impacts (or if authors have other suggestions or ideas from their simulations I would be interested to know).

We added the following paragraph L546 :
"This study shows that there is a need to look with greater care into IDW properties to understand the tropical oxygen distribution in models, in particular in CMIP class models. As shown by Kwiatkowski et al. (2020), CMIP6 models (typical horizontal resolution of 1°) do not agree on the future change in tropical oxygen levels (mean 100 – 600m, their Fig 2). This may partly originate in a misrepresentation of the properties of the IDW in the different models and the strength of the connection between western and eastern Pacific Ocean. Simple analyses, similar to our Fig 2 (oxygen levels at 30°S and oxygen levels in the eastern tropical Pacific) and Fig 9 (Mean Kinetic

Energy at intermediate depth) may give some insight into the mechanisms at play. In addition, analyses of experiments performed in the context of the High Resolution Model Intercomparison Project (resolution greater than 0.25°) (Haarsma et al., 2016), part of CMIP6, will give a more complete insight on whether a significant Equatorial Intermediate Current System develops at higher resolution. While HighResMIP are not coupled with a biogeochemical module, velocity fields are available at a monthly resolution, which allows to perform "offline" tracer or Lagrangian particle experiments"

Haarsma, R. J., Roberts, M. J., Vidale, P. L., Senior, C. A., Bellucci, A., Bao, Q., Chang, P., Corti, S., Fučkar, N. S., Guemas, V., von Hardenberg, J., Hazeleger, W., Kodama, C., Koenigk, T., Leung, L. R., Lu, J., Luo, J.-J., Mao, J., Mizielinski, M. S., Mizuta, R., Nobre, P., Satoh, M., Scoccimarro, E., Semmler, T., Small, J., and von Storch, J.-S.(2016). High Resolution Model Intercomparison Project (HighResMIPv1.0)forCMIP6, Geosci. Model Dev., 9, 4185–4208, https://doi.org/10.5194/gmd-9-4185-2016

Kwiatkowski, L., Torres, O., Bopp, L., Aumont, O., Chamberlain, M., Christian, J. R., Dunne, J. P., Gehlen, M., Ilyina, T., John, J. G., Lenton, A., Li, H., Lovenduski, N. S., Orr, J. C., Palmieri, J., Santana-Falcón, Y., Schwinger, J., Séférian, R., Stock, C. A., Tagliabue, A., Takano, Y., Tjiputra, J., Toyama, K., Tsujino, H., Watanabe, M., Yamamoto, A., Yool, A., and Ziehn, T.: Twenty-first century ocean warming, acidification, deoxygenation, and upper-ocean nutrient and primary production decline from CMIP6 model projections, Biogeosciences, 17, 3439–3470, https://doi.org/10.5194/bg-17-3439-2020, 2020.

3. I would like to ask for clarifications on "oxygen forcing" instead of "oxygen restoring". Actually the new term "forcing" raise additional question (at least to me). Does this mean the authors experimental design is NOT "restoring" but just replacing the oxygen values, for example at 30N and 30S to observed values (and always fixed to observed values during the model integration)? What will be the difference? The forcing sounds to me like the atmospheric forcing (such as wind stress) changing with time but I assume that the dissolved oxygen forcing at the boundaries does not change with time (just fixed to the observed climatology) correct?

Indeed we replace the oxygen values at 30°S and 30°N which is why we label this experiment "forcing" than "restoring". We use a time varying "forcing" (= we use oxygen values from monthly mean climatological observations at 30°S/N) to reproduce the basic aspects of the seasonal cycle. We use this strategy as the goal of our experiment is to test what would be the impact on tropical oxygen of an "observed" oxygen boundary. In a typical "restoring" approach, a term is added to the prognostic equation to match (or push toward) the observational values. The resulting tracer concentration is not necessarily equal to the concentration toward which it is restored (depending on the strength of the restoring). A "forcing" is actually an extreme case of "restoring". The section is now called "2.2.1 Forcing of oxygen to observed values in the subtropical regions".

4. L98: Is the CORE-II climatological forcing "normal year" forcing or you constructed climatology based on CORE-II forcing from 1948-2007? Also, I think this does not impact the result but is there a reason why you used CORE-II forcing from 1948-2007 instead of 2009 (since CORE-II extend to 2009)?
The CORE-II climatological forcing that we use is the Normal Year Forcing. This is now stated explicitly (L132)

5. What is the initial condition of sensitivity simulations (60 years simulations) by NEMO2? Do all these sensitivity simulations start from spun-up simulation from the mean state comparison (i.e. 1000 years integration from NEMO2)?

Yes, all the sensitivity simulations start from the spinup state (otherwise the simulations would not be exploitable due to the strong drift in oxygen levels in the first hundred of years of integration). This now stated explicitly in the text.

6. L39: Is it semicolon here? I would use period and separate the sentences but I will leave this to the author (including checking with native speakers).
We agree and separate the sentences.

7. L126: "eNEMO" should be "NEMO"?
Yes.

Reviewer 3

General comment : we would like to thank the reviewer for her / his comments, which helped us to improve the manuscript. Thanks to these comments, we realized that the scope of our manuscript was not well defined. Rather than a study focusing on the characterization of the subducting water masses, we aim here to understand the oxygen distribution in the tropical ocean and the role of the intermediate depth waters (defined here as the 500 - 1500 m) in modulating this distribution. We choose here a depth range rather than a density criterion as the strong attenuation of respiration with depth introduces difficulties when comparing different density layers. The layer 500 - 1500 m is now called “Intermediate Depth Water” instead of “Intermediate Water Masses”.
Our focus on tropical oxygen levels was not clear from the previous version of the manuscript and we clarified this point (introduction and along the text), in particular for readers originating from another community than the “oxygen community” (this manuscript is part of a joint special issue “Ocean deoxygenation: drivers and consequences – past, present and future” but its scope should be of course clear for any reader outside of this community).

The authors present a variety of analyses of the controls on O2 at intermediate levels in the Equatorial Pacific. Despite some interesting results, the manuscript was remarkably scattered and unfocused, and thereby difficult to read. Part of the problem stems from an inadequate analysis of water masses, and part of the problem stems from a confusing set of experiments with a disparate set of models.

The authors really need to state somewhere the abstract conclusions something like “biases in the modeled O2 concentrations in intermediate layers in the Equatorial Pacific (which we carefully define in density space) obviously reflect issues with the formation and fate of intermediate waters within the interior. This study uses A/B methods to disentangle the causes of unsatisfactory IWM O2 in the tropics, and arrives at the conclusion that the problem is X% formation, Y% fate/ventilation, and Z% bad biogeochemical modeling. The limitations of our analysis are C, and we recommend that D be used to look at this future work”. Otherwise this somewhat sprawling mess of a manuscript won’t be comprehensible and won’t contribute to broader community efforts.

We want to make clearer that this study focuses on better understanding the biases in oxygen levels in the tropical Pacific Ocean and reformulate the abstract as well as the title of the manuscript
“Title : The riddle of eastern tropical Pacific ocean oxygen levels : the role of the supply by intermediate depth waters”

“Abstract : Observed Oxygen Minimum Zones (OMZs) in the tropical Pacific ocean are located above intermediate depth waters (IDW). Typical climate models do not represent IDW properties and are characterized by a too deep reaching OMZ. We test here the role of the IDW on the misrepresentation of oxygen levels in a heterogeneous subset of ocean models characterized by a horizontal resolution ranging from 0.1° to 2.8°. First, we show that forcing the extra tropical boundaries (30°S/N) to observed oxygen values results in a significant increase of oxygen levels in the intermediate eastern tropical region. Second, the equatorial intermediate current system (EICS) is a key feature connecting the western and eastern part of the basin. Typical climate models lack in representing crucial aspects of this supply at intermediate depth, as the EICS is basically absent in models characterized by a resolution lower than 0.25°. These two aspects add up to a “cascade of biases”, that hampers the correct representation of oxygen levels at intermediate depth in the eastern tropical Pacific Ocean and potentially future OMZs projections.”

Otherwise this somewhat sprawling mess of a manuscript won't be comprehensible and won't contribute to broader community efforts.
Most of models display 2 biases : 1- not enough oxygen in the extra tropical region (an incorrect quantity is transported toward the eastern Pacific Ocean) 2- bad representation (absence) of the currents at intermediate depth (incorrect transport). These biases "cascade" as an incorrect quantity is incorrectly transported, leading to a large underestimation of oxygen levels in the tropical Pacific Ocean. This result is new and useful to the modeling community, especially the large community focusing on oxygen minimum zones (OMZ).

Our ms gives specifically 2 directions for future work : 1- improving the representation of the water masses subducting in the southern ocean is fundamental to represent correctly tropical OMZs, 2- the intermediate current system is basically "missing" in most of ocean models and there is a need to quantify more precisely its impact on biogeochemical cycles (especially when performing future projections). We have outlined these implications in the discussion.

The most important problem with the study is that it approached intermediate water masses (IWMs) with very little consideration of the associated water masses. Defining IWM as waters spanning 500m-1500m is scientifically flawed, and if the authors insist on having their analysis focused on that horizon then the words "intermediate water masses" should not appear in the title. There are a number of places where this problem arises.

We agree with the reviewer if the goal of this study were to assess the formation processes of the (generally speaking) "intermediate water masses" in high latitudes. However, we focus here on the region 30°S-30°N, far away from the regions where intermediate water masses subduct. The isopycnals are mostly flat meridionally in the latitude band 30°N-30°S. We show here that at 30°S models show too little oxygen at intermediate depth (500 – 1500 m). Identifying precisely the water masses based on density characteristics is not the scope of this study. We want to make clearer that the focus of this manuscript is to understand the tropical oxygen bias in ocean models in this intermediate depth range, rather than to quantify precisely the water masses composition, which is why we have replaced intermediate water masses (IWM) throughout the text by intermediate depth waters (IDW).

Our decision to focus on a depth range is not least due to that there is no unique way to define intermediate water masses based on density in a model environment. In a model intercomparison (CMIP3), Downes et al. (2010) used the density of the Potential Vorticity minimum to characterize the core of the SAMW and the Salinity minimum to characterize the core of the AAIW. As a result the SAMW and AAIW present density ranges varying in between models. Sallee et al. (2013) used a similar approach when comparing CMIP5 models. Lower and upper boundary of the water masses are determined by using an arbitrary density range. The arbitrary density range is either fixed (+/-0.03 kg.m-3 as in Downes et al., 2010) or adjusted (Sallee et al., 2013 state "manually adjusted to best capture the five water masses in each model analyzed"). Kwon (2013) used a different approach and solely define SAMW based on potential vorticity and state "the definitions of Subtropical Mode Water and Antarctic Intermediate Water (AAIW) are not strict in that they are defined here as two bordering water classes that are lighter and heavier than SAMW ". All these studies focus on the Southern Ocean (Southern of 30°S) and water mass formation processes.

Using this kind of methodology (note that there are several definitions of the intermediate water) in our study to understand oxygen distribution in the region 30°S-30°N may complicate the picture as the density structure of the models differs from observations and between each other, i.e the density of AAIW salinity minimum will be different.

Furthermore the oxygen content of a water parcel is very sensitive to its depth due to vertical mixing from the surface ocean (Duteil and Oschlies, 2009) we prefer to use a depth range rather than a density threshold that will vary in depth between the models.

We agree with the reviewer, the depth horizon 500 – 1500 m encompass several different "intermediate" water masses : see the table by Emery, 2003. Emery (1986, 2003) pragmatically separated the ocean into 3 depth horizons: upper waters (0 - 500 m), intermediate waters (500 – 1500 m), deep waters (> 1500 m). In our study we use this basic classification. We do not think that it is fundamentally “scientifically flawed” as the goal of this study is 1- to highlight the sensitivity of tropical oxygen levels to subtropical oxygen concentration (30°S boundary). 2- to assess the role of the equatorial deep jets on oxygen levels. We do not focus on water mass formation and fate.

Layer	*Atlantic Ocean*	*Indian Ocean*	*Pacific Ocean*
Upper waters (0–500 m)	Atlantic Subarctic Upper Water (ASUW) (0.0–4.0°C, 34.0–35.0‰) Western North Atlantic Central Water (WNACW) (7.0–20.0°C, 35.0–36.7‰) Eastern North Atlantic Central Water (ENACW) (8.0–18.0°C, 35.2–36.7‰) South Atlantic Central Water (SACW) (5.0–18.0°C, 34.3–35.8‰)	Bengal Bay Water (BBW) (25.0–29°C, 28.0–35.0‰) Arabian Sea Water (ASW) (24.0–30.0°C, 35.5–36.8‰) Indian Equatorial Water (IEW) (8.0–23.0°C, 34.6–35.0‰) Indonesian Upper Water (IUW) (8.0–23.0°C, 34.4–35.0‰) South Indian Central Water (SICW) (8.0–25.0°C, 34.6–35.8‰)	Pacific Subarctic Upper Water (PSUW) (3.0–15.0°C, 32.6–33.6‰) Western North Pacific Central Water (WNPCW) (10.0–22.0°C, 34.2–35.2‰) Eastern North Pacific Central Water (ENPCW) (12.0–20.0°C, 34.2–35.0‰) Eastern North Pacific Transition Water (ENPTW) (11.0–20.0°C, 33.8–34.3‰) Pacific Equatorial Water (PEW) (7.0–23.0°C, 34.5–36.0‰) Western South Pacific Central Water (WSPCW) (6.0–22.0°C, 34.5–35.8‰) Eastern South Pacific Central Water (ESPCW) (8.0–24.0°C, 34.4–36.4‰) Eastern South Pacific Transition Water (ESPTW) (14.0–20.0°C, 34.6–35.2‰)
Intermediate waters (500–1500 m)	Western Atlantic Subarctic Intermediate Water (WASIW) (3.0–9.0°C, 34.0–35.1‰) Eastern Atlantic Subarctic Intermediate Water (EASIW) (3.0–9.0°C, 34.4–35.3‰) Antarctic Intermediate Water (AAIW) (2–6°C, 33.8–34.8‰) Mediterranean Water (MW) (2.6–11.0°C, 35.0–36.2‰) Arctic Intermediate Water (AIW) (−1.5–3.0°C, 34.7–34.9‰)	Antarctic Intermediate Water (AAIW) (2–10°C, 33.8–34.8‰) Indonesian Intermediate Water (IIW) (3.5–5.5°C, 34.6–34.7‰) Red Sea–Persian Gulf Intermediate Water (RSPGIW) (5–14°C, 34.8–35.4‰)	Pacific Subarctic Intermediate Water (PSIW) (5.0–12.0°C, 33.8–34.3‰) California Intermediate Water (CIW) (10.0–12.0°C, 33.9–34.4‰) Eastern South Pacific Intermediate Water (ESPIW) (10.0–12.0°C, 34.0–34.4‰) Antarctic Intermediate Water (AAIW) (2–10°C, 33.8–34.5‰)
Deep and abyssal waters (1500 m-bottom)	North Atlantic Deep Water (NADW) (1.5–4.0°C, 34.8–35.0‰) Antarctic Bottom Water (AABW) (−0.9–1.7°C, 34.64–34.72‰) Arctic Bottom Water (ABW) (−1.8 to −10.5°C, 34.88–34.94‰)	Circumpolar Deep Water (CDW) (1.0–2.0°C, 34.62–34.73‰)	Circumpolar Deep Water (CDW) (0.1–2.0°C, 34.62–34.73‰)
		Circumpolar Surface Waters	Subantarctic Surface Water (SASW) (3.2–15.0°C, 34.0–35.5‰) Antarctic Surface Water (AASW) (−1.0–1.0°C, 34.0–34.6‰)

Table 1 from Emery (2003).

Downes, S. M., N. L. Bindoff, and S. R. Rintoul (2010), Changes in the subduction of Southern Ocean water masses at the end of the 21st century in eight IPCC models, *J. Climate*, 23, 6526–6541, doi:10.1175/2010JCLI3620.1.
Emery, W.J and J. Meincke. 1986. Global water masses: summary and review. Oceanol. Acta, 9, 383-391.
Emery, W. J. 2003. Water types and water masses. In: Encyclopedia of Atmospheric Sciences. 2nd ed. (eds. J.R. Holton, J.A. Curry and J.A. Pyle). Elsevier, Atlanta, GA, pp. 1556–1567
Kwon, E.Y (2013), Temporal variability of transformation, formation, and subduction rates of upper Southern Ocean waters, *J. Geophys. Res. Oceans*, 118, 6285– 6302, doi:10.1002/2013JC008823.
Sallée, J.‐B., Shuckburgh, E., Bruneau, N., Meijers, A. J. S., Bracegirdle, T. J., Wang, Z., and Roy, T. (2013), Assessment of Southern Ocean water mass circulation and characteristics in CMIP5 models: Historical bias and forcing response, *J. Geophys. Res. Oceans*, 118, 1830– 1844 doi:10.1002/jgrc.20135.

One confusing case was for the point made in Fig. 2b, namely the panel showing the observed and modeled vertical distribution of O2 at 30S in the Pacific.

The reviewer does not state explicitly the issue but we may understand her/his concern. Indeed, the reviewer may think that we average zonally waters with very different characteristics (density, T,S, O2) at 30°S. Consequently, her/his confidence in the meaning of a zonal mean is low. The figure below shows the zonal oxygen level at 30°S in the Pacific Ocean for all the models.

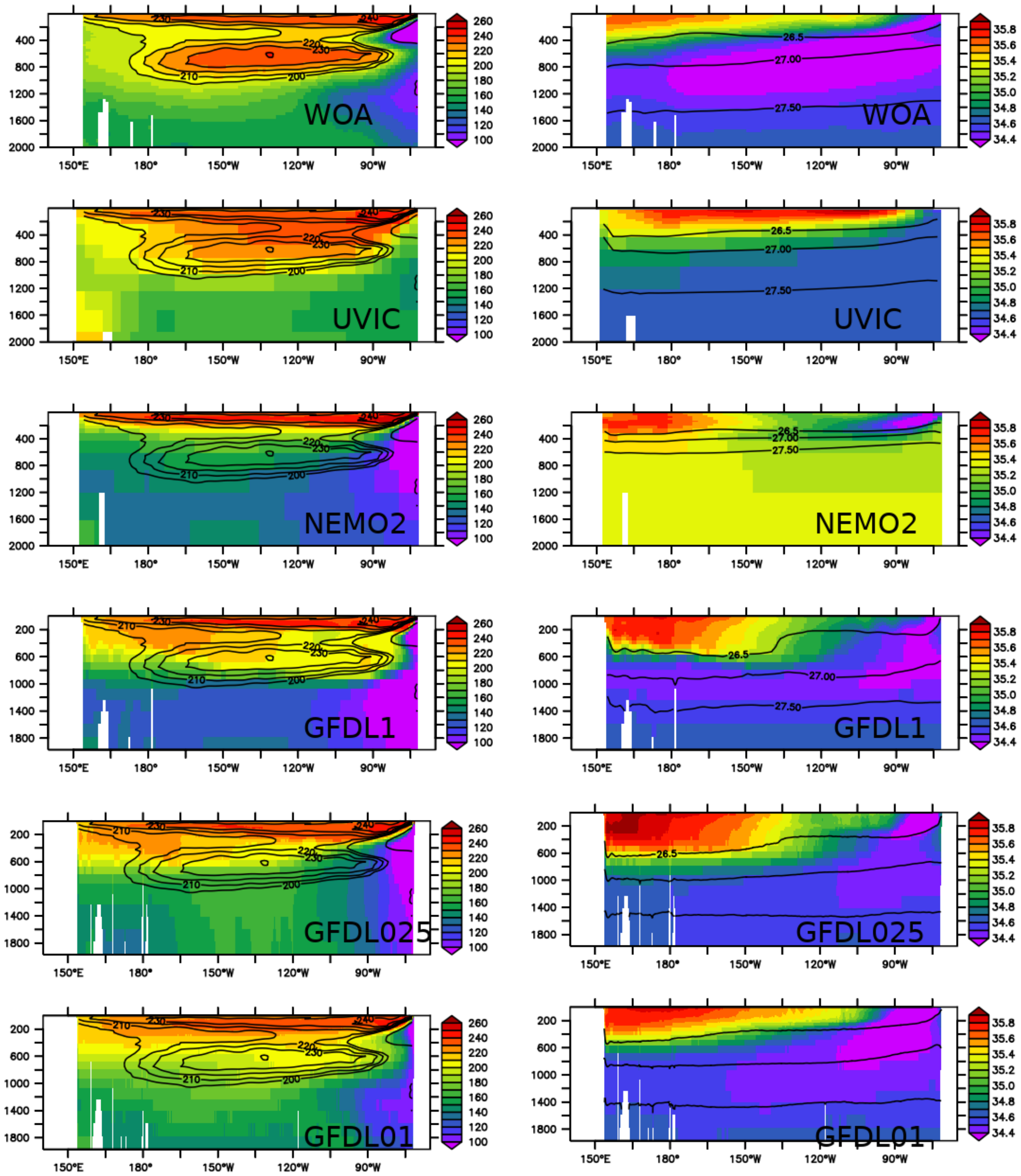

Figure : left column : oxygen levels in observations and models at 30°S. The WOA oxygen levels are displayed in contour. Right column : salinity in observations and models at 30°S. The density anomaly (26.5, 27, 27.5) is displayed in contour (now Figure A5)

Based on this figure, discussing the Fig 2b and a zonally averaged quantity does not present any major difficulty. Generally speaking, zonally averaging water properties in depth coordinates in intermodel comparisons efforts is very common (e.g Cabre et al., 2015 for oxygen).

First and foremost, the authors really need to be clear about water masses.

We choose here a very pragmatic depth horizon and state that explicitly.

I believe that the O2 subsurface maximum is located near the boundary of SAMW and AAIW, rather than squarely in AAIW densities. This should be checked by averaging across the basin first in density at 30S.

We do not agree with the reviewer, the O2 subsurface maximum is clearly located below sigma 26.8 (often used as a density criterion to define AAIW in observations, e.g Karstensen et al., 2008) in the WOA close to the salinity minimum, characteristic of the core of the AAIW. This is consistent with Russell and Dickson, 2003.

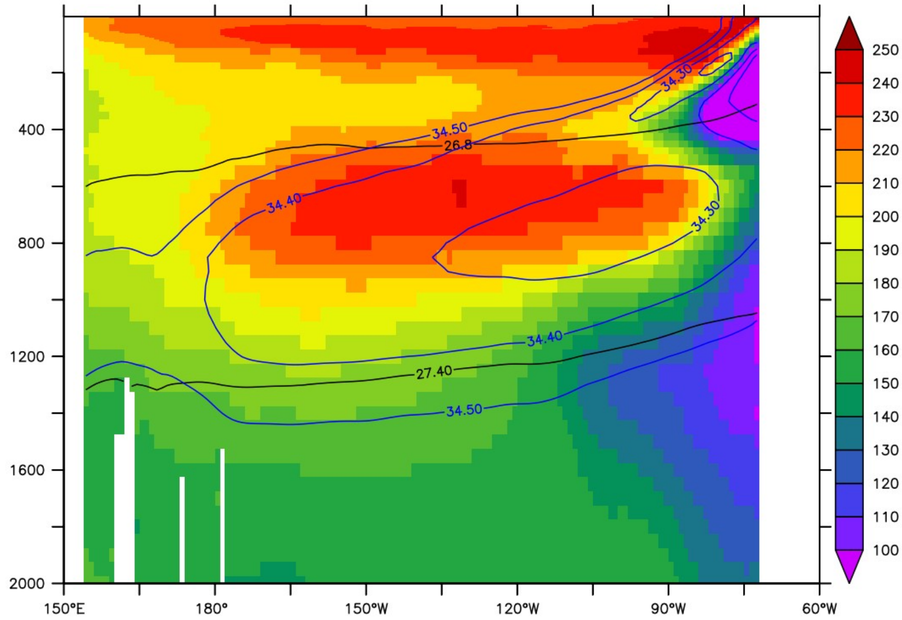

Fig : oxygen levels (mmol.m-3) in observations at 30°S in the Pacific Ocean. The density anomaly levels 26.8 and 27.4 (kg.m-3) are displayed in black contour. Salinity levels lower than 34.5 are displayed in blue contour.

Karstensen, J., Stramma, L., and Visbeck, M.(2008). Oxygen minimum zones in the eastern tropical Atlantic and Pacific Oceans, Prog.Oceanogr., 77, 331–350, doi:10.1016/j.pocean.2007.05.009

Russell, J. L., & Dickson, A. G. (2003). Variability in oxygen and nutrients in South Pacific Antarctic Intermediate Water. Global Biogeochemical Cycles, 17(2), doi:10.1029/2000gb001317

Second, with the text in lines 209-210, it wasn't clear what the authors were saying about the "large role" of IWM. Are they referring to the "formation process" that sets the O2 content of IWM waters? Or something else?

The sentence L209 – 210 (now L238-239 in the ms version including corrections) is "The basin zonal average of the mean oxygen level in the lower thermocline (layer 500 - 1500) m at 30°S and in the eastern part of the basin (average 20°S – 20°N, 160°W-coast; 500-1500 m) are positively correlated (Pearson correlation coefficient R=0.73) (Fig 2d, Appendix A), suggesting a large role of the IWM in controlling the oxygen levels in the tropical oceans".

It has been modified and now reads : "The basin zonal average of the mean oxygen level in the lower thermocline layer (500 - 1500m) at 30°S and in the eastern part of the basin (average 20°S – 20°N, 160°W-coast; 500-1500 m) are positively correlated (Pearson correlation coefficient R=0.73) (Fig 2d, Appendix A), suggesting that the oxygen levels in the tropical pacific ocean are partly controlled by extra-tropical oxygen concentrations at intermediate depths and the associated water masses".

There are a number of questions that also arise from the application of Lagrangian diagnostics. First, near line 170 in the text, it wasn't clear whether the Lagrangian analysis included the bolus-velocities from the mesoscale parameterization? Or not?
The paragraph L169 (now L207) reads : "In order to complement the tracer experiment we performed Lagrangian particle releases. Lagrangian particles allow to trace the pathways of water parcels due to the resolved currents, and to track the origin and fate of water parcels. They are not affected by subgrid scale mixing processes". We replaced the last sentence by "They are not affected by subgrid scale diffusive and advective processes".

Second, it wasn't clear from the text if the NEMO01 flow-fields were coarse-grained to the NEMO05 grid before running the trajectory analysis?
This point is stated explicitly :
L173-174 (now 208 :"The NEMO01 circulation fields have been interpolated on the NEMO05 grid in order to allow a comparison of the large scale advective patterns between NEMO01 and NEMO05"

But most importantly, over lines 395-401, it was puzzling that the authors invoke "qualitative mode" rather than "quantitative" mode in designing their Lagrangian experiments if their goal is to evaluate connectivity through source regions.
The experiment is quantitative ("qualitative mode" is not mentioned in the manuscript) as we used a large number of particles to quantify the origin of water. The experiment shows clearly that particles originate from a broader region in NEMO01 compared to NEMO05 (Fig 7, 8). The connectivity between the western / eastern Pacific ocean, but also between the surface / deep ocean is increased in NEMO01.

The choice of 1000m as a release horizon isn't justified. Why was this chosen?
1000 m has been chosen as it is a depth where the equatorial intermediate current system is relatively well developed in high resolution models and basically absent in coarse models (see Fig 5). Another depth horizon in the range 500 – 1500 m (intermediate layer depth) would not change significantly our results. We have now added in the manuscript : "A depth horizon of 1000 m has been chosen as it is a depth where the equatorial intermediate current system is relatively well developed in high resolution models and basically absent in coarse models (see Fig 5). Our results are not sensitive to the choice of another depth horizon in the range of 500 - 1500 m"

How does Fig. 7 help the reader to understand the critical scale and processes needed to represent IWMs in the Equatorial Pacific?
The Figure 7 is a quantitative evaluation of the role of the equatorial intermediate current system on the transport of particles originating from the eastern Pacific Ocean (a) or the western Pacific Ocean (b) after a time scale of 15 years. It shows clearly that the Eastern Tropical Pacific ocean, where the OMZ are located is better ventilated than in NEMO01. 15 years has been chosen as the decadal time scale corresponds to the time scale of response of the OMZ to climate forcings (Deutsch et al., 2010, Duteil et al., 2018)

To reiterate, what is missing here is a clear exposition of scientific objectives, or a clear motivation for the specific choice of models and dye/lagrangian tracer diagnostics.

We agree that our scientific objectives were not defined and motivated clearly enough. In particular we want to communicate more clearly that the objective of this study is to better understand the supply of oxygen in the lower thermocline, at intermediate depth, toward the tropical eastern Pacific Ocean, where the largest OMZ are located. We add in the introduction (L47) the following paragraph which makes the context / goal of this study clearer.

"Climate models tend to overestimate the volume of the OMZs (Cabre et al., 2015) and do not agree on the intensity and even sign of oxygen future evolution (Oschlies et al., 2017). In order to perform robust projections there is a need to better understand the processes at play that are responsible for the supply of oxygen to the OMZ. We focus here on the Pacific ocean, where large OMZs are located in a depth range from 100 to 900 m (Karstensen et al., 2008; Paulmier and Ruiz-Pino. 2009). Previous modelling studies have shown that the tropical OMZ extension is at least partly controlled by connections with the subtropical ocean (Duteil et al., 2014). In addition, the role of the equatorial undercurrent (Shigemitsu et al., 2017; Duteil et al., 2018; Busecke et al., 2019), of the secondary Southern Subsurface Countercurrent (Montes et al, 2014), of the interior eddy activity (Frenger et al., 2018), have been previously highlighted. These studies focus on the mechanisms at play in the upper oxygen levels (upper 500 m meter). The oxygen content below the core of the OMZ however plays a significant role in setting the upper oxygen levels by diffusive (Duteil and Oschlies, 2009) or vertical advective (Duteil, 2019) processes. Here, we focus specifically on the mechanisms supplying oxygen toward the eastern tropical pacific ocean at intermediate depth (500 – 1500 m), below the OMZ core.

The water masses occupying this intermediate depth layer (500 – 1500 m) (Emery, 2003) subduct at high latitude" (…)

A fundamental limitation of the study is the lack of a water mass set of definitions for analysis with IWMs, without this the manuscript is in my opinion would clearly need to state "we ignore water masses except in a very hand-wavy qualitative sense, and focus instead on aggregated (AAIW, SAMW, etc.) properties over 500-1500m. "

We stated L47-49 (now L80-83) "AAIW, NPIW and the upper part of the PDW are oxygenated water masses occupying the lower thermocline between 500 and 1500 m depth. We will refer to the waters in this depth range as Intermediate Water Masses (IWM) in the following"

Following the suggestions of the reviewers, we now state: "In this study we do not specifically focus on the individual water masses, but rather on the water occupying the intermediate water depth (500 – 1500 m) of the subtropical and tropical ocean. We will refer to the waters in this depth range as intermediate depth water (IDW)".

In my opinion the manuscript would require major revisions to remedy these problems.