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Interactive comment on "Oxygen minimum zones in the tropical Pacific across CMIP5 models: mean state differences and climate change trends" *by* A. Cabré et al.

Anonymous Referee #1

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This manuscript focuses on the oxygen minimum zones (OMZs) and their representation in current climate models (CMIP5). OMZs directly constrain the abundance of marine life and impact biogeochemical cycles (denitrification). Mechanisms leading to OMZs formation and variability are still not completely understood.

In this manuscript, the authors used the outputs of 11 CMIP5 models that they compare to observed data. Simulations present recurrent biases, which can be explained either by physical (underestimation of the undercurrent, off equatorial currents, ventilation from high latitudes regions, mixing) or biological (POC export and remineralization) processes. The authors assess the mean state, interannual variability and long term projection.

I find the manuscript interesting and well written; the biases in CMIP5 need to be better understood to increase our confidence in projections. I particularly appreciated the discussion of processes leading to the incorrect representation of oxygen concentration.

Please find below my comments (remarks / relatively minor issues) :

Page 6552, line 12 : “A reason for this is a bias in the modeled Southern Hemisphere OMZ, systematically larger than observed” This sentence is not very clear as the IPSL model presents more oxygen than observed.

Page 6534, line 4 : “In general, all equatorial jets are too weak or inexistent in non eddy-resolving models with the exception of the EUC” EUC tends to be much too slow as well in non eddy resolving models (at least 30 percent) !

Page 6534 line 17: “We suggest that consequences of the too slow lateral ventilation of these regions in CMIP5 models include too low subsurface oxygen concentration” I agree, slow lateral ventilation leads to less oxygen transport. However, the argument that you use for the EUC above (= less transport of oxygen but also less transport of nutrients and then less production) might apply here too. Or if not, why is the case ?

Page 6535 line 4: Actually Duteil et al (I think you mean 2014) focus on the role of both the EUC and the off equatorial currents in the Atlantic Ocean (and not specifically on the role of deep currents).

Page 6535 line 12: “However, both versions of MPI-ESM model show similar biases in oxygen distribution and a too deep tropical OMZ (Fig. 1), which suggests that the too deep modeled OMZ is partly caused by biases in biological processes” Maybe another possibility is that in these models the deep OMZ is set by processes occurring in high latitude (too low oxygen in intermediate water) and is not linked with the strength of equatorial currents. Is the MOC or the extra equatorial currents similar ?

Page 6635: section ’Low spatial resolution’: I think that it would be more consistent to distribute the content of this section in the sections above or below. Indeed resolu-

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tion issue it is not really a 'physical cause for OMZ biases', but impacts of course the representation of processes.

Page 6536 : section 'inadequate ventilation . . .' Maybe computing the MOC would quantify the role of water originating from the Southern Ocean ? Indeed a strong MOC should foster high oxygen concentration at depth in tropical ocean.

Page 6537 line 5: You emphasize the role of isopycnal mixing, but do you think that differences in diapycnal mixing could play a significant role ? Diapycnal mixing appears in your figure 10 (schematic) and should be at least briefly discussed here.

Page 6537, line 16 A potential important point is also the -O2:P(N) ratio as it impacts the amount of oxygen consumed / nitrate remineralised and then ultimately primary production and denitrification. Do all the models present the same -O2:P(N) ratio ? In the introduction, you also introduce DOC (page 6528, line 7) but it is not discussed here : could it maybe help to understand the differences in O2 concentration ?

Page 6538, line 20 "The remaining models predict POC flux values in accordance to Dunne et al. (2005) and Siegel et al. (2014)" For IPSL as well ? (you state that IPSL underestimates POC : p6533 – line 17)

Page 6542, line 15 It is possible to use parameterizations as well. For instance intermediate jets can be parameterized by using an anisotropic diffusion scheme (Getzlaff and Dietze, 2013). Increasing resolution alone is however not sufficient (see MPI-ESM-MR and LR)

Page 6542, line 23 "(...) would improve the representation of the OMZs even before the representation of equatorial ventilation is improved" A general issue is to compensate biases in circulation by biases in biology. To assess better the models maybe an approach using preformed and regenerated quantities would be a possibility (eg. Ito and Follows, 2005; Marinov et al., 2006) (but this is maybe not the scope of the study).

Page 6545, line 19 "negative correlation between AOU and O in Fig. S7b" it seems

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there is a region at 5-10N/500m where the correlation is significantly weaker, do you know why ? I compared with fig 8b, and it seems that it is not correlated with the zones where the oxygen are very low (which seem to be at the equator). Maybe you could trace the oxygen concentration on top of FigS7b ?

Page 6545, line 27 “Decreased ventilation (Fig. S6) is accompanied by decreased AOU” Maybe this is linked with the amount of saturation of surface water, as the assumption O2 = 100% saturated at surface is not always true (Ito et al., 2004). If the percentage of surface saturation increase you might bury more oxygen in the interior ocean (which might ultimately decrease AOU even if respiration increases)

Page 6546, line 10 “O2sat decreases in zones of deep water-mass formation (Fig. 8f) due to 21st century warming, contributing (...) to the decrease in oxygen levels” Changes in Intermediate waters will affect tropical OMZs but I don’t know if deep waters (AABW) have an impact on tropical regions at a 100 year time scale : is it the case ?

Page 6554. line 18 “(...) For example driven by changes in trade winds associated with the Pacific Decadal Oscillation (Deutsch et al., 2011, 2014; Czeschel et al., 2012)” More mechanistically, trades winds regulate the strength of the subtropical-tropical cells (eg. Luebbecke et al., 2008) and then the amount of oxygen transferred from the gyres to the eastern Pacific Ocean (Duteil et al., 2014b).

Table A1, A2, A3 I didn’t understand exactly the difference between HadGEM2-ES and CC / IPSL-CM5A-MR and LR. These models are discussed together and from the tables A1,A2,A3 it seems that the circulation and biogeochemistry are identical. However, there are some large difference in nutrients between IPSL-CM5A-MR and LR (figure 3). Maybe some forcing or parameterizations are different (could it be stated anywhere in Table A1,A2 or A3) ? HadGEM2-CC does not appear in figure 3.

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