Reply to comments of BG Discussion
Reviewer #1

We thank the reviewer for his/her constructive comments. Due to a comment from Reviewer#2 arguing that the results from the regional simulations forced by ESM oxygen trends (labelled RCM' in the previous version of the manuscript) should be presented before the simulations forced by climatological oxygen boundary conditions, the paper has been thoroughly reorganized. The figures and tables presenting the biogeochemical trends have been modified. This does not change the general message of our paper, but large portions of the text have been modified.

Comments:
C1) I think that a more quantitative and critical evaluation of the regional model perfor mance after the spinup is missing. A clear discussion of how well the "baseline", or present day is being represented in the regional simulation is necessary, in particular for sensitive parameters such as thermocline and oxycline depth.

R: We have added new figures showing cross-shore sections of mean state and bias of temperature and dissolved oxygen (DO) for the three regional simulations (period 2006-2015). The model is compared to CARS climatology (interpolated on the model's grid). These figures, included in the supplementary material, are briefly described in the text (lines 254-256 for temperature and lines 366-371 for DO)

C2) Some measure of uncertainty in the percentage of change by the end of the century for each variable is needed. This percentage values form most of the base of the whole discussion and are calculated on the basis of linear trends. A quantitative estimate of how well a linear model fits the timeseries examined, or perhaps an estimate of the actual temporal variability around the trend could make the interpretation of the long-term changes more robust.

R: We have estimated the trend uncertainty based on a bootstrap method. We construct 10 000 synthetic time series by randomly removing data points in the annual series. We converted the trend uncertainty into a percentage uncertainty, now reported in Tables 3,4,5. We also have computed the R^2 from the least square estimation in the tables. Most of the trends are significant at the 10% level. The significant trends are reported in bold font in the tables. We now explain how the uncertainty is computed in the methodology section 2.8.

Specific comments

C: Section 2.3: To choose the global model for regional downscaling, the authors use averaged vertical profiles of a meridional section and compare the bias with an observation-based gridded product (World Ocean Atlas 2009). It is not clear to me if the model was sampled to represent the time period of WOA09, which years is the WOA09 climatology representing?

R: The temperature and salinity from the CMIP5 historical simulations were averaged between 1950 and 2005 to compare with the WOA2009 climatology, which includes observations mainly collected between 1950s and 2009. The nutrient and oxygen profiles from the CMIP5 historical simulations were averaged between 1980 and 2005. They are compared to the WOA2009 which includes biogeochemical observations mostly in recent decades (i.e. after 1980) in the equatorial pacific.

C: *Some ideas in section 2.3 need to be more quantitative. E.g. phrases like "too low", "realistic enough" are somewhat subjective. The authors mention that the temperature and salinity biases are weak, but what does weak mean? How do we compare the weak salinity and temperature biases to the biogeochemical biases?*

R: The reviewer is right. First we added temperature and salinity profiles in Fig.1 to allow for visual comparison between the ESMs. Second, we computed a normalized bias, defined as:
$NB(z) = |X_{model}(z) - X_{obs}(z)|/X_{obs}(z) \times 100$ for each variable X(=T,S, nutrients, O_2). This allows to quantify the amplitude of the normalized bias between the ESMs and compare the normalized bias of different variables. The depth-averaged values of the normalized bias are reported in Table 1. We find that the normalized bias for temperature and salinity are weaker than those for nutrients and oxygen. We corrected the text to avoid vague terms and be more quantitative (see section 2.3).

C: *Lines 293-298: The authors describe a shoaling of the mixed layer depth in all simulations and the agreement or disagreement with a gridded product. I find this confusing since this idea comes after they mention that the "thickness of the surface layer more than doubles" (line 287).*

R: By surface layer we did not mean the mixed layer in this paragraph, but the surface layer with waters warmer than 20°C. We defined D20 in lines 303-304 and rephrased the sentence (line 312)

C: *Also, they note that the mixed layer is calculated differently in the model and in the gridded product. How is the mixed layer calculated in the model then?*

R: The model surface boundary layer is computed from the value a critical Richardson number computed using the KPP formulation, whereas the observed mixed layer depth was computed from individual temperature profiles. However previous modelling work show that the surface boundary layer thickness is very close to the model mixed layer (Liu and Fox-Kemper, 2017). We added this information and this reference (lines 319-321)

C: *Line 279: The term thermocline depth needs to be clearly defined as the isotherm of 20C, as is indicated in figure 6 and as was done with the oxycline (line 341) or nitracline.*

R: We agree with the reviewer that this is unclear. As noticed by another reviewer, D20 and thermocline may be located at different depths. We now no longer refer to the thermocline, simply D20.

C: Lines 333-339: In the text, they mention that figure 10 shows the evolution of nearshore DO concentration, but the trends in this figure are calculated over a region that differs from the coastal box used through the analysis. There is no mention or explanation of why these trends were calculated in an oceanic box that differs in size and distance from the coast than the rest of the analysis.

R: We agree with the reviewer that some clarification is needed here. In this section we compare the nearshore DO content in the RCMs and ESMs between 100 and 200m depth. However, the coarse resolution and topography of the ESM implies that few grid points are present in this depth range in the 100 km band (in particular in GFDL). We believe that the comparison is thus more accurate in the 150km-300 km offshore band. In the same way as the oxycline is quite deep in R-GFDL we had to extend the width of the box to 200 km. This was not the case for the nitracline (depth of nitrate isosurface 21 umol) which was shallower and could thus be computed in the 0-100 km band (Fig.11d).

We added explanation in lines 366-367.

C: *Line 382: Positive trends in surface biomass were found in R-GFDL and R-IPSL, but the nitracline only deepens in R-IPSL, in R-GFDL the nitracline gets shallower. The increase in surface biomass would be surprising only in R-IPSL.*

R: The reviewer is right. We corrected the text accordingly.

Typos and minor issues

Line 21: The resolution of the model is not consistent through the text, In the abstract is 10 km, but in the description of the model (line 100) is ∼ 12 km.

R: The resolution is 12 km. We corrected the error in the abstract.

Line 31: "small pelagic fisheries"
R: Corrected.

Line 50: IPCC is not defined
R: We replaced by CMIP5 and wrote out the meaning of the acronym line 51.

Line 52: "Oyarzún"
R: Corrected.

Line 58: AR is not defined
R: Corrected.

Line 76: change 2017 for 2018.
R: Corrected.

Lines 82-83: The phrase "most recent climate scenarios" is not clear to me. Do you imply that the RCP's are recently developed scenarios? that we are following these scenarios? please clarify.
R: We modified the sentence: "..under climate scenarios taking into account economic and population growth assumptions (e.g. RCP8.5) and over longer time periods (e.g. 100 years)." (lines 85-86)

Lines 117-118: Is it possible to fix the exponential with the symbol and superscript?
R: Corrected.

Line 124: CMIP5 is not defined
R: It is now defined line 51.

Line 141: Needs a comma after "However"
R: Corrected.

Line 171: There is no entry on the reference list for Echevin et al., 2010.
R: Thank you for noticing this error, we added the correct reference (Echevin et al. 2012).

Line 200: Section 2.7 is missing
R: Corrected.

Line 216: The number of the figures they are referring to is missing.
R: Corrected.

Lines 255-262: This section is described as if the trends where those of the ESMs, when figure 4 shows the change in the RCMs. Also, there is no consistency with the use of "R+model" to indicate the downscaled simulation.

R: We corrected the text and figure title to clarify what comes form the ESMs (downward longwave flux, net downward shortwave flux) and what results from the RCM bulk formulae computation (net longwave, wind stress) (lines 275-281; Fig.4).

Line 316: Another example of a subjective phrase "weak dissolved O2 concentrations".
R: We modified the sentence, cited a value for the oxygen concentration, and added a reference (line 347).

Line 318: There is no entry for Espinoza et al., 2019 in the reference list.
R: We modified the reference (Espinoza-Morriberón et al., 2019).

Line 321: You mean the RCM eastward surface flow?
R: No, this is actually the ESM eastward subsurface flow, as 95°W is the location of the RCM western boundary. We modified the sentence as follows: "we first evaluate the ESM eastward subsurface flow (which enters the western boundary of the RCM) at 95°W" (line 353).

Line 328: "The trend is relatively weak. . ."
R: This sentence has been changed due to changes in the figure (see our general comment above).

Line 330: I find that the use of parentheses to indicate the opposite of an idea in a paragraph is confusing and inefficient. I invite the authors to use parentheses for clar ification and citations only and not to save space. See Robock, A. 2010. Parentheses are (are not) for references and clarification (savings space). Eos, Trans. Amer. Geo phys. Union, 91(45): 419).
R: The sentence has been modified due to changes in the figures (lines 360-365).

Table 1. Needs a better description of terms. What does 10 m mean? 10 m wind?
R: 10 m indicates the thickness of the ESM ocean surface layer. The legend of the table (now Table 2) has been modified .

C: Fig. 1. For clarity, I would suggest to make the vertical axis of each subplot equal and visualizing the extent of the influence of the OMZ on nitrate is not evident.

R: Fig1. Vertical axis is now 0-500 m for oxygen panel in Fig.1 Note that it is 0-250 m for temperature and salinity to better highlight differences of the thermocline and subsurface salinity maximum structures.

C: Also, the thickness of the lines representing the selected ESM's is not really different from the rest. Perhaps the legend should refer to these as "solid colored lines" instead of "thick colored lines."

R: We have increased the thickness of the lines in Figure 1.

C: Fig. 2. The description of the legend is not consistent with what is being showed and what is described on the text. i.e., b) and d) should be output from the RCM (downscaled).
R: Corrected.

Fig. 3. The word "value" is missing in the legend just before (c).
R: Corrected.

Fig. 4. In the legend (c) is missing.
R: Corrected.

Fig. 11. In the legend, fix the superscript in µmol L-1.
R: Corrected.

Fig. 16. The legend is wrong, there are no figures 16d-f.
R: Corrected.

Fig. 17. In a) the title of the figure is wrong. These should be the trends of the ESMs not RCM as mentioned in the legend and in the text (line 508). It should be indicated somewhere in the legend that the trends in b) and c) correspond to the R-GCM' sensitivity experiments.

R: Figure 17 has been modified. The results from the simulations forced by dissolved oxygen climatological boundary conditions. Thus they correspond to the RCM' values and not to ESM values. The legend and the text have been modified accordingly.