

Interactive comment on "Physical and biogeochemical impacts of RCP8.5 scenario in the Peru upwelling system" by Vincent Echevin et al.

Anonymous Referee #1

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General comments

In this manuscript the authors analyze changes in the ocean physics and biogeochemistry of the Peru upwelling system using regional projections to the end of the century. They use downscaled output from three different earth system models to initialize and force one regional, high-resolution ocean model under the RCP 8.5 "business as usual" scenario.

Regional downscaling is an accepted methodology for assessing future impacts in regions like the upwelling region off Peru, where global scale models are unable to accurately resolve the process of upwelling. Additionally, model uncertainty in global projections can be substantial, and the use of three downscaled global models can give a clearer measure of such uncertainty in coastal regions, even though only one

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regional model was employed.

The work and analysis presented here can be a solid contribution to regional climate change literature and is worth publishing. However, I think that some work needs to be done in order to provide the reader with a measure of uncertainty on the model performance and the projected changes by the end of the century. Because I think that this is so necessary, I recommend major revisions before publishing in biogeosciences. Here my two main concerns:

1) I think that a more quantitative and critical evaluation of the regional model performance 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.

2) 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.

Specific comments

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?

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?

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). 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?

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.

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.

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.

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 \sim 12 km.

Line 31: "small pelagic fisheries"

Line 50: IPCC is not defined

Line 52: "Oyarzún"

Line 58: AR is not defined

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Line 76: change 2017 for 2018.

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.

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

Line 124: CMIP5 is not defined

Line 141: Needs a comma after "However"

Line 171: There is no entry on the reference list for Echevin et al., 2010.

Line 200: Section 2.7 is missing

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

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.

Line 316: Another example of a subjective phrase "weak dissolved O2 concentrations".

Line 318: There is no entry for Espinoza et al., 2019 in the reference list.

Line 321: You mean the RCM eastward surface flow?

Line 328: "The trend is relatively weak..."

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 clarification 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. Geophys. Union, 91(45): 419).

Table 1. Needs a better description of terms. What does 10 m mean? 10 m wind?

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. 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."

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).

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

Fig. 4. In the legend (c) is missing.

Fig. 11. In the legend, fix the superscript in μ mol L-1.

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

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.



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