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

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General Comments

The authors explore the projected physical and biogeochemical state of the Northern Humboldt Current System (NHCS) under future climate change using a regional circulation model (RCM) forced by three global earth system models (ESMs). They describe changes in a range of ocean properties from temperature to zooplankton biomass, focusing on trends relative to historical conditions as well as the differences among the different ESM and RCM projections.

Future conditions in eastern boundary upwelling systems like the NHCS are of considerable interest due to the biogeochemical, ecological, and socioeconomic importance

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Discussion paper

of these regions. It's also well known that fine scale dynamics in these regions are important and are not well captured by coarse resolution global models, so there is interest in the potential added value provided by dynamical downscaling. Therefore, this is valuable work and is at the cutting edge of regional ocean projection. The inclusion of biogeochemistry, the use of multiple ESMs to force the regional model, and the bias correction of the forcing are all notable and positive elements of the research.

The manuscript is mostly descriptive; the authors note that further mechanistic analysis is left to future research. In my view, the most important results are the comparisons of projected changes between the global and regional models. We know global models have biases, but it's when the projected change is altered by downscaling that a stronger case is made for the need to downscale. The authors find that this is the case for biogeochemical, but not physical, variables. I have a number of specific comments below, but my main concerns are with several choices in the methods, detailed below.

Specific Comments

I have three main concerns on the methods:

1. (Section 2.3). The choice of which ESMs to use has been justified based on historical comparisons with observations. However, there is a growing body of research arguing against this method, since these historical model evaluations do not necessarily correspond to how well a model captures the response to future climate forcing. "Emergent constraints" have been offered as a more relevant method for evaluating climate models (Hall et al. 2019). In the absence compelling reasons why a model is unrealistic for the future change, the default should be to pick a suite of models that capture the range of potential futures.

2. (Section 2.4). As I understand it, this method produces forcing with no high-resolution (sub monthly) variability. High frequency wind variability can be very important especially to the BGC in Eastern Boundary systems. For example, Gruber et al (2006) attribute model chlorophyll biases to the use of monthly forcing. For future

projections, one can add representative high frequency variability (e.g., from historical reanalysis) as a third term on the right hand side of equation (1). Similar has been done for historical sensitivity analyses (Frischknecht et al. 2015, Jacox et al. 2015).

3. (Section 2.5): First, it's unclear why one would not bias-correct the physical ocean boundary conditions. For consistency they should be treated like the surface and ocean BGC fields. Second, oxygen should be treated the same as the other biogeochemical variables. While I understand the concern about unrealistic oxygen values, the oxygenation trend is inextricably linked to the trend in nitrate concentration (Fig. 11) and presumably other nutrients, and in turn with trends in productivity. It doesn't make sense to deem the oxygen trend unrealistic and the others realistic. Furthermore, since oxygen and nitrate variability are closely coupled, imposing the ESM change in one but not the other introduces biogeochemical inconsistencies that may compromise the RCM findings. The analysis of oxygen using climatological boundary conditions is still interesting as it allows one to separate different contributions to the regional change, but it's not consistent with the rest of the analysis. Therefore, the main text should include the GCM change, with the context that you are trying to bound the range of possible futures, not to predict exactly what happens in the future. The oxygen analysis using climatological boundary conditions can move to discussion.

Detailed Comments:

L20, 428, 547: Suggest removing "business as usual": See Hausfather and Peters (2020).

L87: Unclear what "in the following paragraphs" refers to. The whole rest of the paper?

L115-121: The differences are described and are stated to be important, but it's not clear what is the motivation for these changes.

Section 2.6: The temporal coverage of these data sets is quite short for evaluating historical model performance, given that the decadal variability in the ESMs should not be expected to align with nature. Something like 30 years would be more appropriate, but in any case the authors should be wary of caveats associated with using short observational records.

L205: This is probably fine as a proxy, but it's worth noting that it doesn't explicitly represent upwelling, including the curl-driven component. If so inclined, one could get a more accurate upwelling metric by integrating the Ekman and geostrophic components over the region of interest or by using the vertical velocity at the based of the Ekman layer (Jacox et al. 2018). It would also be helpful here to describe the calculation of the cross-shore geostrophic transport.

L221: Thanks to its high spatial resolution and the bias correction of the forcing.

Fig. 3: I think it would be more appropriate to show the bias corrected ESM change (i.e., remove the mean ESM SST bias so that they all start from the same place). I also don't think a % change is best for SST, at least if the units are Celsius. In Fig. 3 the ESM % changes are lower because they are starting from a warm-biased state. But the magnitudes of projected temperature changes are as large as or larger than the RCM. Lastly, throughout the manuscript some indication of significance should be added to the trends.

L248-249: Did Bakun actually project cooling, or just intensified upwelling? There could be intensified upwelling but still warming due to dominance of the surface heating.

L252-253: It's not clear to me the evidence that this pattern results from the upwelling and subsequent lateral transport/damping of subsurface anomalies.

Fig. 4: Would be informative to see the net longwave and shortwave radiation (not just downwelling).

L266: Initially it seems strange that the offshore transport trend is 2x greater than the wind stress trend, since the transport is linearly related to the wind stress. But, it does make sense because when you calculate the Ekman transport (i.e., Fig. 5a minus Fig.

5b), the change is ∼10%, consistent with the winds. This should be explained in the text, and I suggest adding the Ekman transport as a third panel to Fig. 5.

L269-270: It's also interesting that since there's a long-term trend in Ekman transport but not in geostrophic transport, the relative contribution of the geostrophic transport increases over time

L275: Do you mean they are locally influenced by the passage of waves? Or are you suggesting the waves actually propagate (advect) the anomalies somehow?

L279: I would be careful about equating the depth of an isotherm (D20) with the depth of the thermocline (i.e., the depth of maximum temperature gradient). Temperature biases (or changes) will alter D20 but not necessarily the thermocline depth.

L297: How is MLD calculated?

Figure 8: I would like to see the ESM tendencies on here as well

L308-309: I don't understand why this statement is here. I would delete it

L351 and elsewhere: "deemed realistic enough" isn't very convincing. I don't think you have to argue for the realism of the ESM changes, rather you are looking at the regional impact of the ESM changes as one potential future scenario.

L352: Since the 95W location is discussed a number of times, it would be helpful to show it on a map (e.g., Fig. 2a along with the coastal region)

L360-365: It's hard to compare a concentration is one place (Fig. 11a) with a depth level in another place (Fig. 11c), especially when arguing that one is the driver of the other. Can these be presented in a more consistent way?

L389-397: I must say I'm surprised to see trends of opposite sign in the upper 10m. Surely one can't have opposite trends at different depths within the mixed layer. Perhaps this is a seasonal signature, e.g., in summer the mixed layer is very shallow (∼5m) and increased chlorophyll in the seasonal mixed layer is driving the overall trend. But the authors should look at this in more detail to explain how chlorophyll at 2m can have an opposite trend from chlorophyll at 7m.

L430: This may be true, but without a heat budget it's speculative. There will be other contributions as well (e.g., local surface fluxes). The text at L456-460 is good.

L547-552: There is also a summary statement like this in the previous section (L427-429). One of them should be cut – probably the earlier one.

L561-565: I think this is all speculation, so should be presented as hypotheses rather than fact (unless there is evidence to support it)

Technical Corrections

L117-118: Does the period in a.T indicate multiplication?

L368-369: Quasi-absent doesn't make sense. Maybe negligible? Insignificant?

Table 1: Suggest including in the caption the meaning of abbreviations (mainly Pg and Zg) and the meaning of (10m) in the number of vertical levels column. Also I don't think the full references are needed in the table, they can be in the reference list.

Figure 4: in caption (d) should be (c)

Figure 6, 7, 13, 16: Values should be positive for depth

Figure 17: Top panel should be ESM?

References

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