

Interactive comment on “Response of O₂ and pH to ENSO in the California Current System in a high resolution global climate model” by Giuliana Turi et al.

Anonymous Referee #2

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General comments

Turi et al. use a state-of-the-art Earth System Model to address the effects of ENSO in O₂ and pH in coastal waters of the California Current System. They find that the mean drivers at surface differ for both O₂ and pH: the O₂ response extends for several hundreds of km due to temperature-related changes in solubility, while coastal upwelling affects DIC and drives pH changes within 100 km from the coast. Below 100m depth, the responses of O₂ and pH seemed coupled; both responded to changes in isopycnal surfaces (e.g., by coastally trapped waves). I found the approach and results very interesting and sound; I also appreciate the focus on two very important variables (O₂ and

C1

pH) and their connections (rather than looking at them in an isolated way). I'd recommend this manuscript for publication after my moderate comments are addressed; in particular, I think that including discussion/conclusions on the large variability between events would strengthen the manuscript.

Specific comments

*The abstract would benefit from introducing early on why we care about the effect of ENSO on coastal O₂ and pH (I'd suggest one or two lines at the very start).

*Section 2.1:

-Pag4 Line6: What is meant by “prototype”? It gives the impression of a model in the early stages of development – but I don't think it is the case. Please consider explaining better or re-wording.

-Line 18-19: I am wondering why WOA05 was used instead one of the more recent versions (2009, 2013). Also, I suggest referring to WOA data as “climatologies” rather than as “modeled” data.

*Section 2.2:

-Pag4 Line 23: Why is wintertime NDJ instead of the more common (and likely more winter-appropriate) DJF? (or even JFM).

-Line 24: This sentence makes one wonder: What are the drift issues in the carbonate chemistry? It would be useful to read a line or two about this, to keep the reader from wondering if there is anything wrong with the model and better justify the need of a filter. If the drift happens only at the beginning of the simulation as stated, why not consider those years as “spinup time” and remove them from the analysis? Please explain more or re-word the sentence.

*Section 2.3:

-P5 Line 10: Please cite source of the climatological SST and SLP

C2

- Line 19-21: The location of the sections needs to be described.
- Lines 24-29: In line 24, please remove “very” – one could argue that EMS2.6 and ROMS are not “very” similar. Also, while the qualitative description is useful, it would be great to also see a more quantitative comparison (eg, compare mean and ranges of warm-cold ENSO signal for the 3 models)
- Line 30: as asked for wintertime: why is springtime defined as FMA?
- Same line: mention source of CHL observations (SeaWiFS)
- P6 L3-5: I think the lag between ESM2.6 and observations should be mentioned. Furthermore, it would be useful to see the ROMS reanalysis in fig A1 (ideally, it would be closer to the observations and strengthen the justification of its use to evaluate both GFDL’s models). The latter is just a suggestion.
- *Section 3, Results:
- P7 L19: Please add a sentence or two to justify the assumption of linearity
- L31-34, From “This difference. . .”: This belongs to the discussion and should be removed from the Results section. It is actually proved later.
- P8 L9: I recommend to rewrite “The pH (Fig 7c, f, i, l) and O2 responses are. . .”
- L 14: “we next split . . . El Niño composite means into their individual components”. At first, I thought this meant that you were going to divide again in the individual ENSO events. Please consider re-wording (eg, “into their different drivers”)
- L23: I recommend citing page or table from Sarmiento and Gruber (2006), to enable the reader to find the equations easily. Otherwise, add the equations here or in an appendix. Also, while T has a dominant role in solubility, salinity also affects solubility and I suggest to mention it (it would be great if the S role could be quantified as well!).
- L32-32: The residual effect: does it also consider the effect of winds?

C3

- P9 L12: Comment only – it’s unfortunate that DIC and Alk couldn’t be saved beyond the surface. If these simulations are run again in the future, maybe those two variables could be saved instead of [H+] (if disk space allows for one extra output).
- L15-16: The partial derivatives of pH are key and deserve a more explicit description of how they were calculated. The cited CO2calc is just a calculator of the carbonate system. How did you alter DIC/Alk/T/S in order to calculate the changes on pH?
- L27-29: does this call for a reference to Fig 6a?
- *Section 4, Discussion and Conclusions:
- Most of the analysis was performed for the mean ENSO signal (ie, composite of all ENSO events), so the conclusions are mostly based on this mean. However, the manuscript also describes early on large differences between events (Section 3.1). It would be beneficial for the manuscript to expand the conclusions in terms of this large variability between events (e.g., are the processes identified as responsible for the mean signal be still dominant in all the individual events? Any suggestions on the causes of the variability?)
- Note that in the Results section there is a lot of discussion. You could either remove the discussion parts in the Results (as suggested above for a particular case, but there are more instances), or you could also move all discussions to Section 3 and rename it “Results and Discussion”. In the latter case, Section 4 would be a shorter Conclusions section.
- P10 L29-30: Could your differences with respect to Nam et al (2011) be based on the fact that you work with a mean ENSO signal and they focus on a specific event?

Technical comments

- *I’d suggest to rewrite “100 km” as “hundreds of kilometers” where “100” intends to mean “hundreds” rather than “one hundred” (e.g., in the abstract “reaches up to several 100 km offshore”; also in section 4)

C4

*Pag4 Line 18: I think it'd be better to refer to "the beginning of year 141 of *a* CM2.6 1900 control simulation", rather than *the*, because "the control simulation" makes me think of the control run being described (ESM2.6 control simulation)

*Pag4 Lines 30-32: I suggest re-ordering this sentence: first state the need to interpret patterns of ENSO-related signals, and then explain that to this end, you use standardized anomalies.

*P5 Lines 5-6: By now, the models have been introduced and are referred to as EMS2.6 and ESM2M. Please remove the "GFDL-"; same for figure captions.

*P5 L31: "does not" instead of "doesn't"

*P8 L10: should it be 32 and 36 degrees N?

*P9 L26: replace "seems to mainly act" by "mainly acts"

*Figures

-Remove titles in the figures; make sure captions capture all the information in the current titles. In Fig 2, make a legend for the density contours (if the latter is not possible, then keep only the "Density (contours)..." text in the title).

-Fig 5: Explain in the caption the zeros and ones found in the x labels. The caption says "gray" outline in g, but it looks like black. Also, note that the solid line is hard to distinguish from the dashed ones on the screen (it's ok in the printed version) – I suggest to make solid lines thicker if possible.

-Why there are two figures labeled A1 and A2, if there is no Appendix section? Shouldn't these figures be labeled consecutively, following the order in which they were mentioned?

*References: I did not check the references thoroughly, but spotted a mistake in Robbins et al (2010): it says "Max" instead of "Mac"

C5

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C6