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Ocean Science Discussions

OSD

Interactive comment

Interactive comment on "Response of O_2 and pH to ENSO in the California Current System in a high resolution global climate model" *by* Giuliana Turi et al.

Anonymous Referee #3

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1. General Comments: Turi and colleagues conduct here a well-detailed and interesting analysis of how ENSO impacts the temperature, O2, and pH field structures of the California Current System (CCS). The paper's focus on temperature, O2 and pH and driving mechanisms is highly relevant to attribution and descriptive studies of the CCS, given ecosystems' vulnerability to changes in these variables, and thus should generate a broad and interested audience. Specifically, Turi et al. reveal significant model improvement in representing ENSO physical variability of the CCS in a coupled high-resolution model (vs. CMIP5-type resolution), which they use to evaluate the diversity and mechanisms driving ENSO impacts off the California coast. The authors uncover large variations in the CCS response, a point that is somewhat under-developed in

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the paper and should be further elaborated on given its high relevance to CCS-ENSO studies. Using a composite analysis of the 3-D spatial structure and component decomposition of O2 and pH anomalies from their simulated ENSO events, they suggest different mechanisms driving O2 and pH anomalies at different depths, with changes in temperature as a major driver of surface O2 anomalies, while changes in isopycnals depth and upwelling accounting for most of the variability in pH and O2 at depth. Overall, the paper by Turi and colleagues is well written, the approach is novel, and results are thought provoking, though I felt the discussion section could be further developed given their interesting results and their relevance to other CCS studies. This paper is suitable for publication in the Journal of Ocean Science and I recommend strengthening it with the following comments, revision, and suggestions below.

2. Specific Comments:

1) The paper is appropriately and well titled, but since temperature is so prevalently used in figures and discussed throughout the paper, and since temperature is also an important ecosystem stressor, perhaps it ought to be in the title as well?

2) The introduction provides a thorough review of previous work, and could perhaps be improved by adding a few lines on processes driving O2 and pH variability in the upper 150 m of the CCS (i.e. upwelling, solubility, and productivity and respiration, etc.). This would help putting the processes section in context.

3) The method section could use more detailed description of the model and its configuration, e.g. : what is the model's vertical resolution? How long as the model been spun up for? what is the general structure of the BGC model?

4) It would also be helpful to explain the choice of using a coupled configuration vs. a hindcast simulation (CORE2/NCEP-forced run) of the high resolution model. Wouldn't a hindcast run provide a more realistic representation of ENSO impacts on ocean biogeochemistry and physics? This would also allow for more appropriate comparison to observations.

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5) The authors extensively uses FMA anomalies without justifying the choice of this season/ period. Is this associated with the time scales (2-3 months) of coastal wave propagation from the equatorial region post the maximum equatorial SST anomaly typically observed in DJF? Or is this simply based on the timing of the maximum CCS impact as shown in the mean response in Fig 5? This is especially confusing as some variables are plotted in FMA (SST, O2, pH) while others are shown for DJF (e.g. SLP). FMA is also described as spring, but spring is typically MAM, and winter is DJF. Please explicitly state the choice for FMA, and describe acronyms somewhere in paper/figures (FMA=February-March-April, etc.).

6) Fig A1 ought to be within the paper rather than a supplementary or appendix since this seems to be a major deficiency in the model and should be made more visible and relevant. Additionally, the method section could also benefit from a comparison of simulated BGC fields to the WOA climatologies, i.e. how large are the BGC biases, and how do they differ in the high resolution vs. low resolution version of the GFDL model, at least for the CCS. A discussion of the implications of model biases on the paper's results could help provide a more thorough overview of the potential and limitations of the authors' approach, especially when relating their results to observations.

7) The diversity of the ENSO SST and SLP anomalies shown in Figure 4 is very interesting, and so is the diversity of the averaged O2 and pH changes shown in Fig 5. It would be useful and highly relevant to see similar maps as shown for SST and SLP (as Fig 4) for O2 and pH for different events (perhaps in Appendix, but preferably in the paper). This is perhaps most useful to inform observations-based studies which are often limited to few or single ENSO events. Generally, the diverse response in BGC should be detailed further and reasons for this diversity could also be explored, especially since this is one of the paper's main stated and novel research questions. e.g. What were the initial conditions prior to each event? Do similar patterns emerge in the CCS from different ENSO events (eastern vs. central El Niño)? Do both O2 and pH show the same degree of variability as SST and SLP? The diversity of SST and SLP

to ENSO events could also be shown for the observations, and would be interesting to assess whether such high variations across ENSO events differs in obs. vs model.

8) At the same time, the diversity of the CCS response to ENSO questions the use of the composite mean difference to evaluate "typical" ENSO impacts; i.e. how representative is the composite mean of the ENSO anomalies used in Fig 6-10. Perhaps adding a statistical test/stipplings to show which of these patterns are significant could help address this?

9) In page 9 line 5-6, the authors propose deepening of the thermocline during El Niño to explain the increase in O2 at 100m all along the coast, but for pH changes, they invoke a dipole in upwelling north vs. south 40oN (Pg9 L 25). This is confusing since changes in intensity or source of upwelling and isopycnal depths should impact pH and O2 similarly. How do the authors reconcile this discrepancy?

10) The process analysis conducted here is valuable in understanding the CCS biogeochemical response to ENSO physical changes. Important questions on which of these physical processes drive these biogeochemical anomalies however remain unclear, and perhaps could be discussed further. e.g., what is the role of "remote" wave propagation vs "local" atmospheric forcing of upwelling on the biogeochemical anomalies presented here? This could be addressed using existing figures or editing figures, e.g. superimposing SLP anomalies on BGC anomalies to assess role of atmospheric forcing effects on spatial anomalies in pH and O2. The analysis of Frischknecht et al (2015) regarding the roles of remote vs local forcing in driving physical and biogeochemical anomalies could also be discussed in relation to Turi et al's regionally distinct imprints of ENSO on CA CCS.

11) Another important question that belong to the mechanisms section and discussion but is unclear is what is the role of changes in transport vs. changes in biological production and respiration rates on O2 anomalies? In an MITgcm hindcast simulation, Ito and Deutsch (2013) decompose O2 changes due to ENSO to changes in respira-

tion rates, transport, and solubility in the northern tropical Pacific OMZ and show that a warmer thermocline is also more oxygenated, in agreement with Turi et al's model results. They argue however that during El Niño, declines in O2 respiration rate in the thermocline associated with reduced carbon export that result from a deeper thermocline, reduced nutrients export to surface and reduced productivity, is the main driver of O2 changes. The heaving of isopycnal shown and suggested by Turi goes in the same direction but doesn't preclude reinforcing biological effects from being a contributing or dominant component.

12) Generally, the discussion section could benefit from expanding on how these results fit in the context of other studies' findings. The diversity of ENSO events is especially relevant to past and future studies of ENSO and the CCS, mainly that a generic CCS response to ENSO shouldn't be expected given effects of initial local conditions, different teleconnections, etc.

13) The figure titles and captions are hard to read for quick readers, and could really use more attention to explaining acronyms, reducing repetitions, and clarifying what the figure is trying to convey. e.g. the terms "high-pass filtered standardized" is already stated in methods and needs not be repeated in each figure.

3. Technical Corrections: 1) Pg 4 Line 9, what is vertical resolution?

2) Pg 4 Line 18: Do authors mean "observed climatologies of O2, nitrate, etc."? To my knowledge, WOA doesn't include modeled fields.

3) Fig 2. Caption Line 2: "ROMS Climatology"? Shouldn't it be an anomaly rather than a climatology?

4) Fig 5, "gray box", do authors mean Fig 5g?

5) Pg 6. L20." magnitude of +/- sigma". Sigma from area average?

6) Page 7 "Fig 5b and e" or "5b" only?

7) Figure 3 and chlorophyll seems less relevant to the paper's theme and could be delegated to Appendix/supplementary.

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