Biogeosciences Discuss.,
https://doi.org/10.5194/bg-2020-279-RC2, 2020

Biogeosciences
Discussions

Interactive comment on "Coastal processes modify projections of some climate-driven stressors in the California Current System" *by* Samantha A. Siedlecki et al.

Anonymous Referee #2

Received and published: 12 October 2020

I think that this manuscript is an extremely useful contribution to the field, using a collection of projections at different spatial scales to identify how future biogeochemical change will be impacted by coastal processes operating at finer spatial scales. The analysis also benefits by its inclusion of a range of future climate projections, which allows for a quantification of the differences between models that is in excess of variations within the future climate projections beyond that of a difference from a mean future state. Below I make some general comments on the level of detail that the analysis includes – especially with regard to a more detailed quantification of why the local changes occurred and the specific effect of the coastal processes. I also make some specific comments on the discussion.

General comments: (1) I think that this paper is a little more descriptive than I was expecting, or maybe hoping for. The aim of the paper is to describe how models that include higher spatial resolution of "coastal processes" lead to amplified or dampened responses to future change in comparison to coarser-scale models. The paper successfully describes how a model comparison can achieve this useful goal, but I think it falls a little short of clearly and specifically describing what processes and mechanisms lead to the amplified or dampened changes, based on the simulation results.

Let me try to give some specific examples: (a) Starting on line 442, there is a discussion of the differences in freshwater inputs and the related TA effects that reads "The TA changes are driven in part by the altered timing of the freshet in the N-CCS as well as the presence of a river plume in an upwelling regime. Freshwater in the region is known to be corrosive due to naturally low TA, which impacts the buffering capacity of the surface waters. This result can be seen in the surface difference plots for pCO2 near the Columbia River plume (Fig. 4) and the surface TA change (Fig. 8). The 12-km projections include climatological freshwater fluxes as precipitation along the coastline instead of resolving river plumes like the 1.5-km projections, but despite these different freshwater parameterizations, both models indicate modification of carbon variables in the N-CCS Cascadia domain with different directions for different variables." What exactly is the reader to take away from this? How is TA actually changing on the N-CCS (up or down), how does the altered representation of the freshwater effect TA and why is it important that a river discharges into an upwelling region? These questions are not addressed, and the paragraph ends with "both models indicate modification of carbon variables in the N-CCS Cascadia domain with different directions for different variables." It is not clear what "different directions for different variables" actually refers to – pH? TA? One goes up and the other down? And how does the freshwater control this specifically? The text in the next paragraph beginning on Line 451 gives a specific example, so perhaps this example can be clearly wrapped into the paragraph before it starting on line 442, especially if that paragraph is more explicit about the nature of the climatological freshwater input versus the river plume, what specific effect on TA this

change brings, perhaps describing what exactly the results in Fig 4 and 8 illustrate to support this conclusion. (b) Starting on line 485, downwelling is raised as a factor in modulating the response of the N-CCS to future change, especially the fact that it may not change in the future. But it is not clear how the specific details that are subsequently mentioned, namely winter mixing, higher hypoxia, and seasonally persistent corrosiveness, relate to downwelling (or upwelling). Perhaps this might be obvious to a reader with detailed knowledge of the region, or that one is expected to assume how upwelling or downwelling might modulate the coastal response to climate change, but I think it needs to be more clearly organized. Furthermore, the paragraph begins with stating how the shelves and the N-CCS are projected to have greater change, but I don't think the association of these greater changes with upwelling/downwelling is clearly articulated in the paragraph. I also don't see how a specific mechanism is quantitatively related to the greater change based upon the results of the simulations the authors ran, and what the processes were in the better-resolved models that represented the process well enough to generate these changes.

(2) There are many places in the manuscript where statements like this are made: Line 362: "These changes in these depth ranges contribute to the results for the carbon variables in Table 1, impacting different carbon variables differently." These statements are too vague to be helpful, and in the case of this specific sentence, I expected the authors to then elaborate on what variables were different, how they were different, and where they were different, but that is not really achieved in the following sentences. This may sound picky, but I encourage the authors to examine these types of statements and see how they can make them more specific, more informative, and more quantitative.

(3) I would like to see the authors try and articulate some clear and specific conclusions of the paper. I understand their main point that resolving coastal processes matters for future projections, but there are places where the specifics of how they "matter" for the CCS for a given variable in the future could be more clearly stated. For example, in the concluding paragraph, it is written "Changes in pCO2 concentrations, Ω, and pH

are modified in the downscaled projections relative to the projected global simulation, suggesting downscaled projections are necessary to more accurately project future conditions of these variables." So, how are they modified? How do you think carbonate chemistry will be different in the CCS now that you have more resolved models, in contrast to what the global models say? More OA? Less OA? More seasonally variable OA?

Specific comments:

Line 22: The abstract sentence that begins with "These processes. . ." is a little confusing. Are the waters just generally low in oxygen and nutrients, or are the oxygen and nutrient concentrations in those waters projected to change, and thus work in concert with solubility change to alter future conditions? I think a small edit to the sentence will help clarify this.

Line 24: "coastal process resolving projections" is a mouthful and unclear, (and I understand word limits), but how about "projections that resolve coastal processes"?

Line 41-42: So is the SST decline from Lima and Wethey 2012 predicted from a model? Or from a global-scale analysis that may not include local observations? It is not clear why this would differ from the Chavez record. Please add a sentence that describes why these two records give contrasting results.

Line 83-87: This text seems out of place here, and perhaps should be moved to the section of the methods where you describe the three models.

Line 125: What do you specifically mean by "spatially-weighted" means? Why calculate them?

Line 126: "mode" should I think be "model"

Line 155: It is not clear that it is clear what "centennial trend addition" means here. Are there clear trends in these boundaries? Where do they come from?

Line 163: the text ". . .,2007 with a one year. . .." just reads awkwardly and is confusing. Was the year 2007 run for a year and then compared to observations from 2007?

Line 168: You make the argument, perhaps fairly, that any bias in the simulation generated by the configurations here will be the same in the two periods you compare. It would be more convincing, and help the reader if the reason for this bias was identified. Can this be evaluated? Is it simply a bias in the forcing?

Line 186: Can you add a brief rationale for why you did not, for consistency sake, use the same 3-year future timeframe to compare all of the three models (used 30 years for coarse-scale model)? It seems unnecessary to add in this potential bias, but if bias is not an issue or there is another rationale for using all 30 years of the coarse scale run, please describe.

Line 197: I think it would help to state the stressor variables parenthetically in this sentence.

Line 346: Perhaps there is a convention in the language of this upwelling system that I am unfamiliar with, but why is higher NO3 associated with more O2 drawdown? Is it because the NO3 increase is a tracer of upwelling that can be linked to O2 source water that has a certain, lower O2 signature? Or is this NO3 assumed to be taken up by phytoplankton growth and subsequently used to drawdown O2 at depth?

Line 374: The word "modified" is used to describe the relative pH changes, but the text that follows seems to consistently describe dampening. Can't you replace "modified" with "dampening" to be clearer?

Line 426: It is unclear if "values" refers to the delta pH or the mean pH when comparing to the other studies.

Line 428: Please clarify that you are describing your "downscaled projections" here, and not those of Dunne

Line 458: Here is a place where some specific model details might provide quantitative

information to support the discussion. Denitrification is raised as a process that can affect TA, but the actual differences in denitrification (and its TA effect) in the model simulations are not shown. I understand that there is a limited amount of information to be shown in any paper, but this discussion would be more compelling if the potential denitrification change was reported. Maybe it is a weak effect, maybe strong, and it would be helpful to know.

Figure 4: Have you considered plotting these deltas on a percentage scale? I understand why they are plotted the way they are, to show absolute changes, but the scales are different (necessarily?) across the depths and this makes it a little harder to compare them, if one wanted.

Interactive comment on Biogeosciences Discuss., https://doi.org/10.5194/bg-2020-279, 2020.