

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 #1

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GENERAL COMMENTS ON MANUSCRIPT bg-2020-279

This multi-model, downscaled projection of changes in the carbonate system by Siedlecki et al. represents a substantial and timely contribution to the biogeosciences community. It fits nicely within the scope of the journal and should be of interest to a large group of readers. The authors put their own work in the context of the previous literature and they use a sound methodology. The structure of the manuscript is intuitive, and the results are appropriately presented and discussed (for the most part; see below).

The "multi-model" aspect of the manuscript is both a curse and a blessing. While

C1

we learn quite a bit from this intercomparison, there are multiple instances where the manuscript lacks crucial clarifications. I make several suggestions below on how to clarify the parts where I stumbled, and I believe that the manuscript will be substantially improved if the authors address those points.

I also note that the manuscript submission was rushed. Sections 3.6,3.7,4 repeatedly refer to a "Fig.8" that doesn't exist in the manuscript PDF. Line 85 refers to a "model evaluation provided as supplemental material" but there are no model evaluation in the Supplement. Line 195 refers to colors in Table 1 while Table 1 is in grayscale. Considering that there are 11 authors on the manuscript, I don't understand how none of them was willing to re-read the manuscript before submitting it? Reviewers are supposed to review the science, not proofread.

SPECIFIC COMMENTS: MAJOR

(1) The comparison between the projections of the 1-degree, 12km, and 1.5km models is a major focus of the manuscript. However, I'm still unclear as to what is compared to what. If I interpret lines 178-184 correctly, each model (1-degree, 12km, 1.5km) is considering a different time period:

1-degree: 1971-2000 (present) versus 2071-2100 (future) 12km: 1994-2007 (present) versus 2085-2100 (future) 1.5km: 2002-2004 (present) versus 2094-2096 (future)

If I'm correct, then the differences in time periods contribute to the differences in model projections. I don't consider this mismatch in time periods a deal breaker, but I certainly think that it should be emphasized and discussed up front in the manuscript?

To add to the confusion, line 185 says: "Comparisons between the 12 and 1.5km-resolution simulations were made with the same year span". What "comparisons" are we talking about? Are we referring to the present state shown in Figure 1? Or are we referring to all figures that show the 12km and the 1.5km together? (Figures 1-7?) What about Table 1 (which includes a "comparison" between the 12km and the 1.5km)?

C2

Again, this sort of comparison is supposed to be a major focus of the manuscript and thus these points must be clarified. I would recommend that the time span used for each model configuration be clearly stated in the caption of Table 1 and Figure 1-7.

(2) Figure 8, referenced multiple times in the text, doesn't exist in the PDF document.

(3) Line 146: "The 12-km historical simulation forcing is described in Renault et al (in review)..."

This reference does not exist in the "References" section and therefore is not available to the reviewers.

(4) Lines 155-156: "Initial and boundary conditions had the same kind of centennial trend addition for temperature, salinity, and all biogeochemical tracers (O₂, nitrate, phosphate, silica, iron, dissolved inorganic carbon, alkalinity)."

This statement is vague and leaves much to interpretation. What about adding one little table in the "Supplement" that details what "centennial trend" was assumed for each of those variables? Were the trends assumed constant in time (linear trend)? Constant in space?

(5) In figure 3a,b,c, the authors are comparing temperature changes across 3 depths (surface, 0-200m, bottom) using 3 different colorscales. The fact that they use 3 different colorscales for the same variable (temperature) makes it unnecessarily difficult to compare Figure 3a,b,c. The same problem arises in Figures 4 and 5 (pCO₂, pH, Omega). It should be possible to find a compromise, meaning find a colorscale that works reasonably well for the three depths. If you don't make such a modification, it becomes unnecessarily hard for the reader to get a sense of how the changes vary along the vertical dimension.

SPECIFIC COMMENTS: MINOR

(6) The text of the abstract uses the symbol Omega without defining what it represents.

C3

(7) Lines 48-50: "Warming impacts O₂ in other ways, for example by raising organismal metabolic rates and accelerating O₂ consumption, and by increasing water column stratification and thus reducing mixing and ventilation"

Is temperature really playing a dominant role in water column stratification in this coastal system? Aren't river inputs and salinity playing a more important role?

(8) Lines 81-83: "We produce multi-model regionally downscaled climate projections of multiple climate-associated stressors (temperature, O₂, pH, Omega, and CO₂) that resolve coastal processes to create 100-year projections..."

If I'm not mistaken, each model configuration (1-degree, 12km, 1.5km) is considering a different time span (lines 178-184), and only the 1-degree configuration corresponds exactly to a 100-year projection (it's more like a 90-year projection in the case of the 12km and the 1.5km models). I think you should include a ~ symbol in front the "100-year projection" to acknowledge these differences between the 3 model configurations. The same comment applies to Line 179.

(9) Line 85: "The model evaluation, provided as supplemental material..." I don't see a model evaluation in the Supplemental material. Please delete this passage, or add a model evaluation to the Supplemental material.

(10) Lines 101-103: On line 103, please add a note such as: "The CMIP models are further described in Section 2.3."

(11) Line 126: Typo (...between mode resolutions...)

(12) Lines 147-148: "The 12-km projection was forced by adding a monthly climatological difference between CMIP5 RCP 8.5 scenario forcing and the historical run forcing, averaged over 2071-2100 and 1971-2000, respectively"

Doesn't this correspond to the "Delta approach" (or "Delta method")? Wouldn't it be worth mentioning it since it is a common approach for downscaling?

C4

(13) Line 170: "For the future conditions, atmospheric CO2 concentration (800 ppm), and future atmosphere..."

Where is the 800ppm coming from? Is it the difference between 1971-2000 and 2071-2100 in the median of the CMIP models? Please clarify this statement.

(14) Lines 194-195: "is highlighted using pink (amplified) and blue (dampened) colors in Table 1"

Please update the sentence (Table 1 is in grayscale).

(15) Line 202: "1.2.1 Subsubsection (as Heading 3)"

Please delete.

(16) Lines 206-207: "The largest temperature increase, nearly 3 degrees C, occurs on the shelf in both regions of the projections (Fig. 3"

I think the sentence (as currently formulated) is misleading. The "shelf" is defined by bottom depths of *less* than 200m (lines 132-133). So the offshelf region in Figure 3b is representative of the depth interval 0-200m, while the "onshelf" region in Figure 3b could be representing something like 0-100m. Given a strongly stratified variable like temperature, the mismatch in depth can explain the contrast between the off-shelf/onshelf regions in Figure 3b.

(17) Line 252: "All projections show an onshore-offshore trend in pCO2..."

Since you are discussing spatial differences, I would suggest replacing the word "trend" by "gradient".

(18) Lines 334-336: "When nitrate is included in the upwelling measure, as in BEUT1, there is a slight decline in the upwelling of nitrate (1-2%), commensurate with a decrease in nitrate at the surface in the N-CCS (Fig. 6)"

Isn't this a circular reasoning? There is a decrease in surface nitrate (Fig.6), and when

C5

we take into account this decrease in our upwelling metric, we get a "slight decline in the upwelling of nitrate"?

(19) Lines 364-365: "On the shelves of the downscaled simulations, the source waters are further modified by coastal processes including increased productivity, freshwater delivery and denitrification."

I don't question the statement, but these are things that were not shown/demonstrated in the manuscript. Please add "(not shown)" at the end of the statement.

(20) Line 369: "3.7 Modification"

Please replace this heading by something less cryptic, e.g., "Differences between the global and downscaled projections".

(21) Line 401: "In our projections, the more realistic winds were different than in Dussin et al..."

Please clarify this statement. I don't understand what "more realistic winds" you are referring to.

(22) Lines 147-148 were describing a "Delta approach" where the 12km and 1.5km models use the same winds in "present" and "future" times, except for the addition of a "Delta" computed from a 1-degree resolution global model.

Now Line 497 suggests something completely different—that the 12km and 1.5km models were directly using the winds from the 1-degree resolution global model: "The downscaled projections are driven by the same forcing as the global simulation" Which one is right? This must be clarified.

(23) Figure 4a,b,c: Would it be possible to add a little "tick mark" on the colorscale, indicating the projected change in atmospheric CO2 concentration between the "present" (1971-2000) and "future" (2071-2100) periods (according to the median of the CMIP models)? This would provide some perspective on the magnitude of the changes in

C6

pCO₂ shown in Figure 4a,b,c.

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