Reviewer #2: Comment on bg-2021-285
Anonymous Referee #2

Referee comment on "Observed and forecasted global warming pressure on coastal hypoxia" by Michael M. Whitney, Biogeosciences Discuss., https://doi.org/10.5194/bg-2021-285-RC2, 2022

The global analysis of SST and oxygen declines in coastal areas on a global scale is relevant to the persistent issue of oxygen depletion and helps contrast warming effects on the coastal ocean from the open ocean. Although the paper is relatively clearly written, and makes a couple of relevant points about more rapid coastal warming and vulnerability in the far northern hemisphere, I found the paper to lack a detailed discussion of the results that would make the findings compelling. It is really a descriptive summary of the SST and oxygen changes, and almost no mechanistic insights are gained. I include some "minor edits" at the end of this review, but immediately below I try and articulate the much bigger issues and where I think the paper must expand or provide more detail and analysis to be a new contribution to the literature.

(1) Because only the RCP8.5 high emissions scenario was used, I found the analysis to lacking in its representation of multiple future possible outcomes for oxygen. Might the observed trend be similar to the forecasted trend if another, less high emissions scenario was used? Can the sensitivity of the results to a different scenario be included to expand the scope of the analysis?

(2) The model forecasts that are used are based on a relatively coarse global model. I can't tell from the information presented in the paper if this model represents the biogeochemistry of the coast well enough to predict anything other than a warming effect on respiration and solubility. If not, then the use of a complex global model doesn't really help, and this analysis could be done using only the SST predictions.

(3) Are the documented coastal areas (illustrated as green in the figures) just coastal ocean model cells nearest to estuarine hypoxic areas from the Diaz database? I am not sure how helpful it is to associate coastal model cells with adjacent hypoxic estuaries which, as the author states, have oxygen variability and controls that are different than the adjacent coastal ocean.

(4) Is it an issue that the surface oxygen capacity trends are largest in the Arctic where there is the most uncertainty? The regression of modeled versus observed capacity show large discrepancies (20-30 mmol/m3) that exceed the predicted ~5-10 mmol.m3/decade. This issue is not discussed in a way that would build confidence in the approach.
(5) Why would oxygen concentration go up, as it did in Fig 6? This seems like a big question but it is not discussed at all.

(6) Line 216-217: Influences of “ocean circulation” are mentioned here without any further analysis, description, citation, or discussion. Seems like they should be removed as no scientific insight is gained and it seems these are throwaway statements at this part of the paper.

(7) I found the discussion to be lacking. There is almost no discussion of why the regional differences in warming or oxygen decline would emerge. It is difficult to understand if this analysis has an bearing on semi-enclosed estuaries, or how adjacent coastal deoxygenation might matter for them. It is not clear how this analysis really contributes to new thinking about hypoxia and future change.

Minor edits:

Line 23: oxygen concentrations where? I think you mean in coastal zone, but need to state clearly here.

Line 25-27: how is coastal acidification a driver of hypoxia? They are linked, but I don’t see a clear cause and effect.

Line 49: “Oshlies” should be “Oschlies”

Line 57: Maybe say “is to quantify” instead of “is stydying”?

Methods: Why using only Aug and February? Warming trends are season specific in some regions, and not necessarily the peak temperature periods.