Ocean Science Discussions

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Comment on os-2021-54

Anonymous Referee #2

Referee comment on "Decomposing oceanic temperature and salinity change using ocean carbon change" by Charles E. Turner et al., Ocean Sci. Discuss., https://doi.org/10.5194/os-2021-54-RC2, 2021

Summary:

The work by Turner et al. uses patterns of ocean carbon to evaluate contributions of forcing and changes in ocean circulation to the storage of heat and salt. This research area is highly relevant to understanding patterns of regional sea level. The work makes use of forced ocean-only numerical experiments to decompose spatial patterns of heat and salinity as excess (due to forcing) and redistributed (due to ocean circulation changes) using a relationship between natural carbon and temperature and salinity. Based on my reading, this relationship, which relies on spatial correlations between the tracers, is new. The diagnosed relationship allows for the decomposition of excess and redistribution with patterns and relative contributions in line with the previous literature. As such, the novelty of the method warrants publication. However, the paper does not document the method well enough (see below) and needs an extensive rewrite. Therefore, the manuscript cannot be published in its present form and requires major revisions. I realize that this is a student-led paper, and I hope that the lead author is not discouraged by the comments; I believe that after revisions and additional work, this study will make a valuable addition to the literature.

Given the extensive issues with the manuscript, I will not give section-by-section or line-by-line comments but will try to focus only on some of the major issues:

- The methodology, which is the key novelty of the paper, is not well explained, documented, tested or reproducible. Therefore, while potentially novel, I cannot judge it based on the material provided. Here is a list of necessary (though probably not sufficient) additions required:
  - Please explain the forcing used in the simulations. Due to the lack of details, it was impossible to understand how the carbon and other tracer decompositions worked (i.e., equations 1 onward). Are you using fluxes for heat, freshwater, and wind? Are using restoring as well? To help the readers, also make sure to appropriately relate

your simulations to ocean-forced simulations already documented in the peer-reviewed literature (e.g., Saenko et al. 2015; Marshall et al. 2015; Huber & Zanna, 2017; Zika et al., 2018; Todd et al., 2020; Dias et al. 2020). In what you referred to as coupled simulations, you mean the biogeochemistry is coupled, not the atm-ocean? Clarify if the atmospheric CO2 is evolving (e.g., Munday et al., 2013).

  - None of the assumptions are validated (e.g., v_s>> v_l or v_s // to v_ll, the latter is not explained so I am not sure what it means). None of the estimates are tested (e.g., the kappa's or the decomposition itself). I didn't understand most of section 2.2 and its assumptions. Given that the authors are using a model they must show how and why their assumptions hold. What are the errors between the truth and the decompositions; why does the relationship hold for natural carbon (note that given the lack of details about the simulations, I am not sure what natural carbon was exactly) but not other biogeochemical tracers? Is it random or is there a reason for it? Why is it considered the other extreme of existing T/C relationship (apparently Bronselaer & Zanna, 2021 is the other extreme)?
  - Why binning and PCA are needed (why not one or the other?); explain the sensitivity of your results.
  - The method for using C is argued to be useful when using observations. Since I struggled understanding the decomposition of the carbon (and why carbon as opposed to other tracers), I couldn't quite understand how this could be applied to observations. I would suggest the authors, in addition to clarifying the method, could explain how one would use observations.
- The writing is hard to follow: many redundancies, lack of appropriate references, lacking definition of variables (e.g., are velocities 2D or 3D, resolved or residual?), many equations are not needed (e.g., eqn 6 and 7), while the key ones are not well explained (see above - what does it mean for contributions to be "scaled away"). Please, take a step back and rethink what is needed and when. Prioritise what is useful and important and validate it. Make sure that every variable used is defined with the correct notation (e.g., distinguish scalars, vectors, etc).
- The "results" regarding the reconstruction and mechanisms are not shown and I believe some of the comments are misrepresenting the literature. Here are some examples (this is not the complete list):
  - There is a discussion about lead/lag and salinity: please calculate it and show the lead/lag in a figure. Explain the mechanisms and show it.
  - The role of AMOC is mentioned several times. Yet, there are other possibilities. In observations, we cannot distinguish the different mechanisms. However, in models, we can. The authors should assess the mechanisms and show that it is AMOC (since they mentioned many times) or not show it but give equal weight to all other possibilities (with appropriate references).
  - I am pretty sure the result of Zika et al. 2018 is inappropriately discussed in the manuscript (they showed that a heat-flux alone can lead to pattern amplification).
  - The y-axis in many of the figures keeps changing (e.g., Fig 2. Or 3) which means that one cannot assess the relationships discussed. How large are the signals, how significant are the relationships (e.g., in Fig 3 the slopes are clearly changing and in many regions I don't believe there are any relationships emerging with times. Is the emergence model-dependent? How/why? Is it related to the redistribution which is a key source of uncertainty in models (Couldrey et al. 2021, Todd et al. 2020, Gregory et al 2016, Huber & Zanna et al. 2017, etc)?

A few additional points that the authors should consider while rewriting the manuscript:

- Avoid redundancies: Many concepts are explained multiple times within the same section (e.g., excess / redistribution in the introduction twice and again in the next section, and again in the following one).
- Large numbers of references on heat/carbon are missing: papers by Katavouta & Williams; papers by McKinley & colleagues; Keith Rodgers & colleagues. Same for ocean-only forced experiments (see above)
- The formatting of the references must be fixed.
- Figures: some comments are provided above but in addition, I would suggest avoiding using the native grid for plotting maps and using appropriate projections and masks.
- Please be precise in your writing to allow the reader to assess your results and associated explanations/arguments. Here are a couple of examples which makes the paper hard to understand:
  - variability: it can be spatial or temporal;
  - long(est) timescales: depending on what you consider short timescales, long can be seasonal, interannual, decadal, etc …
  - There are quite a few more but I will leave it to the authors to carefully address the writing.

In summary, I think the paper could be an extremely valuable contribution to the literature and the field but the authors need to substantially work on all components of the manuscript (the writing, the method and the analysis).