Ocean Sci. Discuss., author comment AC2
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Reply on RC2

Charles E. Turner et al.

Author 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-AC2, 2021

1.1 We have now significantly rewritten the text to describe the experimental specification for individual runs in far greater detail (lines 109-124) separating this out into sections for CTR (lines 117-119), COU and RAD (120-124).
With regard to the specific questions/points raised by the reviewer: surface heat, momentum, freshwater fluxes and atmospheric chemistry from HadGEM2-ES combined with RCP8.5 atmospheric CO2 concentrations was used to force NEMO, with no restoring used – description of this has been included in lines 111-116.
Additional references as suggested by the reviewer to make the experimental specification clearer have been added (line 137).
To describe the different runs we follow the terminology employed by Schwinger et al. 2014, namely COU, CTR and RAD.
Description of each of these have been added to the text (lines 109-110, 117-122) to aid the reader.
For the COU simulation, this specifically refers to a coupled climate change run that imposes both physical and biogeochemical change (line 120-122).

1.2. Following the reviewers recommendations section 2.2 has been completely rewritten to more clearly explain how the decomposition works (lines 142-260) and to show that that it is producing an estimate of how the preindustrial states of temperature and DIC covary.
We have removed the confusing formulation about how velocity perturbation project into temperature carbon space and focussed on the key principle of estimating how temperature and DIC covary spatially, adding references as appropriate (lines 142, 144, 200).
As detailed in response to Reviewer 1 (Comment 1) the simulations we use here were performed to study mechanisms of carbon fluxes and variability in the carbon cycle.
As such, no PAT tracer was included nor fixed circulation experiments were performed, meaning it is not possible to directly validate our outputs against such an experiment.
We have now performed a cross validation with the method of Bronselaer & Zanna, 2020, who like we said, approximated the excess heat field with the anthropogenic carbon field, and were able to validate their approach directly against fixed circulation ocean heat uptake experiments and a PAT (section 3.1, lines 263-348).
We quantify our method against the alternative carbon proxy of Bronselear & Zanna 2020 (lines 26
4-348) and explain the differences between the two methods in terms of oceanographic features.

Williams et al. 2021 explained and defined excess and redistributed temperature and DIC in terms of the correlation of the excess and anticorrelation of the redistributed components (lines 86-90).
We have amended the text to clarify that our decomposition is a method for specifying the anticorrelation between redistributed components, much as the method of Bronselaer & Zanna 2020 specifies the correlation between the excess components (lines 90-96).
In this study natural carbon is defined as that component of the total inorganic carbon signal that would have existed in the absence of an anthopogenically-forced atmospheric CO2 concentration, essentially a preindustrial carbon field (lines 126-136)
We define how this is a useful tracer in our technique (lines 95-96, 187-194), qualifying that the choice of natural carbon is not unique (lines 190-194).

1.3. We have clarified that we use PCA in order to obtain a total least squares regression (rather than an ordinary least squares regression) as this allows the relationship defined to be independent of the choice of dependent variable (lines 211-215).
As described at lines 206-209 decadal data binning is performed in order to eliminate the small effects of model drift and using repeat surface forcing cycles that are misattributed as excess temperature/DIC.

1.4. We thank the reviewer for this advice.
As well as improving the description and application of the decomposition (detailed above, lines 142-260), we have amended the text to clarify that using local relationships between temperature and natural carbon derived from observations we can deconvolve estimates of excess and redistributed temperature (and by extension salinity) (lines 507-511).
Carbon is useful observationally as we have global observations of temperature and carbon dating back to 1990 (GLODAP) that enables the development of the excess/redistributed fields to be tracked with time.
We have added text to explain that that the choice of carbon is not unique, though carbon is useful because excess/redistributed carbon and anthropogenic/natural carbon are similar, with reference to Williams et al. 2021 (lines 68-69).

2. We thank the reviewer for these constructive suggestions.
We have rewritten the methodological derivation section to more effectively describe how and why we define the covariability of the preindustrial states of temperature and carbon (lines 142-260).
As detailed in a previous response above, we have removed all descriptions of how velocity perturbations project into temperature-carbon space, as this was an unhelpful formulation which did not make our approach clear.
We have removed repetitions, added further references (lines 141, 170, 200).

3.1. We recognise the ambiguity relating to our discussion of salinity and lead/lag times. To account for this we have calculated times of emergence of signals of excess heat and salinity from noise for the basins discussed (lines 371-378) which are presented in Figure 6.
We have also clarified the text to reflect that the discussion primarily concerns the emergence of signals in excess salinity and excess temperature, rather than lagged correlations (lines 371-378).
We have significantly expanded the discussion relating to this (lines 531-540) and included extra citations to relevant literature (Stott et al. 2008, Terray et al. 2012, Pierce et al. 2012, Skliris et al. 2014, line 541).

3.2. Thank you for this suggestion, this is an excellent point.
In the revised draft, we have explicitly calculated the redistribution of heat and salinity in the Atlantic through the Equator (lines 404-419), showing that our method is reliably capturing the redistribution of heat and salinity in response to AMOC changes.
We have also added that AMOC is not the only relevant process at play (line 420-426).

3.3. The original manuscript noted the resemblance of the excess salinity fields to the sea surface salinity changes in Zika et al. 2018 in response to an imposed heat flux, and also in response to water cycle amplification.
We agree with the reviewer that there is some ambiguity in the text, so we have removed the reference to imposed water cycle amplification in the revised draft to improve this (line 534).

3.4. We appreciate the reviewer’s position and where appropriate we have maintained axis scales as constant; for example, in Figure 3 (Figure 7 in the revised draft), we have rescaled the plots in terms of mean temperature change in order to improve clarity.
In some cases however, due to the different scales of each component and in each ocean basin, keeping axes scales constant would result in a number of important features becoming indiscernible.
In these cases (Figures 6, 7, 8, 9) we have pointed out to the reader in the text and caption the different scales used.
Specifically for the new Figure 7, we have also rewritten the surrounding text (lines 431-435) to make it clear that relationships are not emerging in every ocean basin, and that it is the North Atlantic that is of interest due to its different behaviour to every other ocean basin (lines 442-451).

- In response to minor comments:

The manuscript has been fundamentally rewritten to account for the deficiencies identified by the reviewers.
We have attempted to identify and remove all redundancies, included additional description and information where this was lacking, and clarified the text where possible.
We have added a significant number of references:

Linking results to observations:
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Model setup and terminology description:
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- Madec, G. and Imbard, M.: A global ocean mesh to overcome the North Pole singularity, Climate Dynamics, 12, 381–388, 1996.
- Madec, G. and Team, N. S.: NEMO ocean engine, https://doi.org/10.5281/zenodo.1464816.
- Martin, T. H. D. T. G. M., Bellouin, N., Collins, W. J., Culverwell, I. D., Halloran, P. R., Hardiman, S. C., Hinton, T. J., Jones, C. D.,McDonald, R. E., McLaren, A. J., O’Connor, F. M., Roberts, M. J., Rodriguez, J. M., Woodward, S., Best, M. J., Brooks, M. E., Brown,A. R., Butchart, N., Dearden, C., Derbyshire, S. H., Dharssi, I., Doutriaux-Boucher, M., Edwards, J. M., Falloon, P. D., Gedney, N., Gray,L. J., Hewitt, H. T., Hobson, M., Huddleston, M. R., Hughes, J., Ineson, S., Ingram, W. J., James, P. M., Johns, T. C., Johnson, C. E.,Jones, A., Jones, C. P., Joshi, M. M., Keen, A. B., Liddicoat, S., Lock, A. P., Maidens, A. V., Manners, J. C., Milton, S. F., Rae, J. G. L.,Ridley, J. K., Sellar, A., Senior, C. A., Totterdell, I. J., Verhoef, A., Vidale, P. L., and Wiltshire, A.: The HadGEM2 family of Met Office Unified Model climate configurations, Geoscientific Model Development, 4, 723–757, https://doi.org/10.5194/gmd-4-723-2011, 2011
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Method description, context of decomposition methodology
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Expanding description of motivation/study context
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Specifically relating to ice
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Relating to AMOC:
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Relating to salinity:
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Regarding Figure projections, we have reproduced all maps using a Robinson projection, rather than a native grid. Finally, following the reviewer’s suggestion we have tried to improve the manuscript with respect to the terminology we use and its specificity, particularly regarding the type of variability we are considering (e.g. spatial/temporal etc) and the exact timescales being considered.