Ocean Science Discussions

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Reply on RC1

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-AC1, 2021

- Major Comments

1. Accuracy
The novelty of the carbon-based methodology described here is that it can be used to derive estimates of excess and redistributed heat (extended to salt) for both model outputs and real-world observations.
This latter application will enable ocean temperature time series observations to be deconvolved into that driven by ocean circulation and that caused by uptake of atmospheric anthropogenic heat, something that is simply not possible using PAT.
We demonstrate the merits of the approach in a model context as it first allows us to look at global patterns of excess heat, something which is well studied, and also avoids problems associated with observational data sets, namely problems with temporal and spatial resolution, and the lack of a PAT.
The reviewer is correct to request better characterisation of the validity of our outputs and given the constraints of our model runs we have given further explanation.
Unfortunately due to the setup of our model (simulations were primarily motivated for study mechanisms of carbon uptake) no PAT was included, meaning it is not possible to directly validate our outputs against such an experiment.
While this is not optimal we have however now performed a cross validation with the method of Bronselaer & Zanna, 2020, who 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 264-347).
We show that our results strongly agree, and that the differences between the two methods closely resemble the differences between their method and their fixed circulation heat uptake; we are able to explain this in terms of subduction of carbon through the mixed layer base, indicating differences are likely due to a relaxation of the assumption of a global mean alpha value.
This is described in (section 3.1, lines 327-336).
As the key motivation of this study is to produce a technique which allows us to estimate excess heat observationally, rather than to model it, this necessitates the use of an alternative methodology to PAT / fixing ocean circulation.
We feel we have now better explained how we go about validating its outputs.

2. Consistency
We think the Winton et al. paper cited by the reviewer is in fact Winton et al. 2012: "Connecting Changing Ocean Circulation with Changing Climate", which looked at the

patterns of heat and carbon storage under freely evolving and fixed preindustrial ocean circulation experiments.
In this paper it is shown that a substantial fraction of redistribution heat transport is compensated for by changes in surface fluxes (around 1/3), indicating that redistribution heat transport is a driver of surface heat uptake from low to high latitude.
Following this, the reviewer is correct to question the impact of flux and circulation perturbations on our outputs.
Our ocean only simulations have an AMOC strength that is approximately 50% weaker than that in HadGEM2-ES (~8Sv as compared to ~15Sv at 26N in the control simulation; text amended at section 3.2 lines 396-403) to include discussion of this).
This would imply that a possible bias in surface heat fluxes associated with redistribution exists due to inconsistency with the forcings.
However, we find realistic heat transports by the AMOC in our simulations (when compared to observations - see revised section 3.2 lines 399-402), as well as an AMOC decline in our simulation that is proportional to AMOC decline in HadGEM2-ES, with AMOC strength being approximately half that of HadGEM2-ES at all times in our simulation (section 3.2 lines 402-404).
Therefore, although there are likely to be inconsistencies in the forcing and the circulation response, comparisons of the AMOC decline between the ocean-only model and HadGEM2-ES are due to a systematic bias rather than a bias which varies over the course of the runs.
Similar such biases resulting from inconsistencies in forcings and sea surface temperatures also exist in FAFMIP type experiments, and are not unique to this study.
However, there will still be an excess component of temperature and salinity as a result of surface forcing, and a redistributed component due to changes in circulation; these will be internally consistent within the ocean-only model run, even if the surface forcings are somewhat inconsistent with the atmospheric forcing.
In summary, in this study we are looking to describe the utility of using a new method based on carbon data to reconstruct excess and redistributed heat.
While the reviewer is correct that the use of an ocean model forced by a coupled model introduces biases, due to the lack of a feedback from the ocean model on the coupled model's atmosphere the existence of these biases does not undermine the demonstration of the method.
The text has been amended to account for this discussion (section 3.2 lines 398-400).

3. Representativeness
Models vary in their circulation response to climate change forcings (as well as their uptake of excess heat) that manifest themselves in a range of different temperature changes in various locations in response to increasing radiative forcing.
We recognise that we present the outputs of only one model, and we agree with the reviewer that this therefore does not represent the full spread of potential scenarios (for example of AMOC decline) that models predict - we acknowledge this in the revised section 4 lines 515-520, 571.
However, we don't feel it impacts the utility of the method that we present, or its possible wide application.
While we feel applying our decomposition for a range of CMIP5 models is outside of the remit of this particular study, following the reviewer's excellent suggestion we have however subsequently compared the circulation changes in our model with those of HadGEM2-ES, and of HadGEM2-ES with a range of other CMIP5 models, as well as the representation of the AMOC in HadGEM2-ES with observations (section 3.2 lines 398-403).

- Minor comments
1. We agree that further clarification is necessary for the lines highlighted. Additional context and explanation has now been added (lines 43-48).
2. A number of techniques for estimating anthropogenic carbon from observations have been derived. These are based either on the inorganic carbon system (ΔC*, TrOCA, phi) or

using measurements of transient tracers as proxies of anthropogenic carbon invasion (TTD), and have been used to quantify oceanic anthropogenic carbon accumulation (Khatiwla et al (2013), and its decadal variability (Gruber et al, 2019). As anthropogenic carbon is closely analogous to excess carbon (Williams et al 2021), we thus have better estimates of excess carbon observationally than excess heat. We have amended the text at lines 59-72 to better explain this.

3. The reviewer makes a good point, we have now significantly expanded the experimental specification in the revised draft, with details of all these included (section 2.1, lines 106-140).

4. Thank you for highlighting this error, this has now been fixed

5. As detailed above when addressing major comment 1 (accuracy) we have now completely rewritten this section (section 2.2, lines 142-261) to make it clearer how our estimation technique works in the revised draft, removing details about how velocity perturbations project into temperature-carbon space, and focussing on how the redistribution of temperature and carbon are linked. We have also cross validated our decomposition against an alternative carbon-based proxy method.

6. We thank the reviewer for this suggestion. We have added observational global excess heat uptake from Zanna et al 2019 to Figure 1b. For excess salinity, the signal is dominated by changes in air-sea freshwater fluxes (evaporation-precipitation) rather than ice melt so comparison to observational ice-melt budgets will not be as useful/powerful as hoped. However, we have adapted the text at lines 351-353 to consider this.

7. The reviewer makes an interesting point. However, although global mean redistributed temperature is constrained to be zero, this is not the case locally or regionally. We therefore think that comparing the scale of positive only and negative only regions is useful, as it is a measure of the degree to which excess and redistributed temperature contribute to local temperature changes.

8. We have adapted the revised draft (lines 391-393) to clarify that the AMOC does declines continually throughout the COU run, with the decline not slowing.

9. We have reworded the text to make this aspect clearer (lines 443-450). Rather than proportionality existing between the excess and redistributed pools and SST anomalies, it is the rate of change in the redistributed heat pool that is proportional to SST change/AMOC change.

10. We have removed this discussion from the revised draft, as we have concluded that it is too speculative and ambiguous.

11. While our model setup does includes sea ice it does not include ice sheets. This means that unfortunately it is not possible to directly examine the relationship between excess salinity changes and glacial inputs in our model. Following this and as mentioned in point 6 above, the excess salinity signal is dominated by changes in air-sea freshwater fluxes (evaporation-precipitation) so comparison to observational ice-melt budgets will not be as useful/powerful as hoped. References to previous studies finding nontrivial ice sheet loss in the early 20th century was to highlight the different emergence timescales of excess heat and excess salinity. We have amended the text to improve clarity regarding this (lines 371-378, 534-554), and to better describe the model setup (lines 106-140). We have also removed the citation to modelled ice sheet loss, and instead cited Hetzinger et al. 2019 and Wadhams & Munk, 2004 as further support (line 535-537), as Hetzinger et al. reconstructed sea ice decline using an algal proxy and found decline in the Arctic started by the early 1910s. Wadhams & Munk calculated a 20th century mean sea ice decline equivalent to a global mean salinity change of ~10^4 PSU: our simulations give a global mean sea ice decline of 5*10^-4 PSU over the same period - of the same order of magnitude.