Geoscientific Model Development Discussions

Geosci. Model Dev. Discuss., referee comment RC2
https://doi.org/10.5194/gmd-2020-426-RC2, 2021

Comment on gmd-2020-426

Baylor Fox-Kemper (Referee)

Referee comment on "The interpretation of temperature and salinity variables in numerical ocean model output and the calculation of heat fluxes and heat content" by Trevor J. McDougall et al., Geosci. Model Dev. Discuss.,
https://doi.org/10.5194/gmd-2020-426-RC2, 2021

This paper covers topics needed to better understand what to do in the present situation of partial adoption of TEOS-10 and partial reliance on EOS-80 in the CMIP6 ensemble. The authors review the basic differences between these schemes, with an emphasis on how best to estimate the energy changes in the ocean under models using both equations of state. The paper improves on earlier treatments of these and related issues and forms a basis for future model evaluations with higher physical consistency.

The paper is admirably quantitative in its comparison of different techniques. It furthermore takes on a more pragmatic explanation of the utility of various metrics, especially potential enthalpy and Conservative Temperature. Many of the questions surrounding the interpretation of salinity are also clarified. The preformed salinity interpretation of modeled salinity is also a helpful pragmatic step.

The key insight of the new approach--proved elsewhere but clearly stated here--is that potential temperature is not actually a conserved variable under advection, which means that the standard method of estimating ocean heat content anomaly (or the energy anomaly in the earth system held by the oceans) of using surface referenced potential temperature and then a heat capacity based on surface properties where the water can exchange energy, so is not an accurate estimate of the energy that has been added to or can be extracted from the ocean. The paper is explicit on this point, for the first time I am aware of making a specific estimate of how the air-sea fluxes affecting potential temperature **should** be calculated rather than how they **are** calculated.

There are two aspects of this paper that are not stated, which I would recommend the authors consider adding:

1) One aspect that is not covered in the paper is whether Option 2 involves more data downloads or disk storage. As the authors are aware, the OHC (and steric sea level) calculations require large amounts of 3D data fields from each model under consideration. Is one method or the other lighter in terms of data access?

2) Steric sea level is also of interest, and has a quite similar set of issues in calculation. By Landerer et al. (2007, DOI: 10.1175/JPO3013.1) the steric calculations depend intimately on the correspondence between the modeled variables and the equation of state for in situ density. I would like to see a small additional discussion on this point, related to the discussion of isobaric density gradients in Section 4.2. The steric anomalies are nontrivially different, as they are vertical integrals of the density, so it matters if the ~1% density gradient errors or 2.7% thermal wind errors accumulate or are random. This would be a valuable addition to this discussion.