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Further comment on gmd-2020-426

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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-RC4, 2021

I find Prof. Fox-Kemper's positive review intriguing and unexpected. Since it stands in quite sharp contrast with my own review, it is likely that many readers will find our contrasting views confusing and making it difficult to decide where to stand. As a result, I thought it might be of interest to complement my technical review by a more informal one attempting to restate what I understand of McDougall et al's paper in plain and straightforward English as a reading guide.

Thus, as far as I understand their argument, McDougall et al. essentially say there are two possible ways to interpret a standard EOS80-based numerical ocean model:

- Interpretation 1, a.k.a. the standard interpretation: as a model carrying potential temperature that does everything correctly apart from: a) neglecting non-conservation effects in its conservation equation; b) neglecting the spatial variations of the specific heat capacity in its estimation of the surface fluxes.
- Interpretation 2, a.k.a. the new interpretation: as a model carrying Conservative Temperature that does everything incorrectly apart from: a) correctly using a conservative equation for it; b) using the correct surface fluxes. In other words, in interpretation 2, everything that is done correctly according to interpretation 1 becomes incorrect and conversely, meaning that an EOS80-based model: a) wrongly initialises CT with observations of potential temperature; 2) use the wrong equation of state for evolution pressure and forces in the momentum equations; 3) incorrectly compute all aspects of the surface fluxes that depend on the surface temperature.

Logically, it is true that the authors' new interpretation 2 provides a logical basis for comparing the temperature variable of an EOS80-based model with Conservative Temperature. However, by the same logic, it is similarly possible to interpret a TEOS-10 model as a bad model for potential temperature, which also provides a logical basis for comparing its temperature with observations of potential temperature. In the latter case, however, everybody would agree that it would not make sense to pursue interpretation 2. The question is therefore why should we consider that it makes more sense for an EOS80-based model?

A key issue with interpretation 2 relates to the point I raised in my former review, namely the fact that the construction of CT requires the specification of three arbitrary constants, a crucial piece of information that a standard EOS80-based model does not possess in general. The proposition that it is possible to interpret the temperature variable of such a model as CT therefore conflicts with the fact that an EOS80-based model has no way to know anything about which determination of CT the authors have in mind. Indeed, obeying a conservative equation and being forced by surface heat fluxes divided by a constant heat capacity does not provide sufficient information to specify the three arbitrary constants entering the construction of CT. This is sufficient to refute the validity of interpretation 2.

Nevertheless, note that the authors' proposition is in principle testable. Just take a new TEOS-10 model that does everything correctly as reference solution. Now, run the same simulation using an EOS80-based model. If the authors were correct, the temperature variable of the latter model should compare more closely with the CT of the TEOS-10 model than with the potential temperature inferred from CT. At the very least, the authors should perform such a comparison in order to test the validity of their ideas.

Some other non-scientific reasons why one should be careful in putting interpretation 2 out there are due to the fact that:

- Interpretation 2 casts EOS80-based models in an extremely bad light and ocean modellers as basically incompetent, since it essentially consists in saying that EOS80 models initialise their simulations with the wrong field, use the wrong equation of state, and erroneously compute air-sea interactions, which sounds a very stupid thing to do;
- Given the key role of the ocean on climate change, Interpretation 2 also casts doubt on the scientific integrity of all IPCC assessments so far, as I don't think how it is possible to trust an ocean model making so many elementary mistakes.

Finally, it seems useful to point out that although Griffies et al. (2016) present the development of TEOS-10 model as posing new challenges for model intercomparisons, the ocean model component of the GISS model has been carrying potential enthalpy as its prognostic heat variable for over 25 years (Russell et al., 1995), see https://simplex.giss.nasa.gov/gcm/doc/ModelDescription/GISS_Dynamic_ocean_model.html for details, long before McDougall (2003). As far as I am aware, the GISS model is included in all existing coupled-model intercomparison projects, which means that if the errors associated with the neglect of the non-conservation and spatial variations of cp are as large as the authors claim, one should expect the GISS model to do systematically better in its simulation of the temperature than all other EOS80-based models. Is that the case? As regards to recommendations, how has the GISS model been compared to other models so far? Shouldn't this form the basis for the recommendations discussed in this paper and in Griffies et al. (2016)?