Ocean Science
Discussions

Interactive comment on "Isoneutral control of effective diapycnal mixing in numerical ocean models with neutral rotated diffusion tensors" *by* Antoine Hochet et al.

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Received and published: 22 December 2017

Potential temperature versus Conservative Temperature

McDougall (2003) pioneered the idea that 'non-conservativeness' is central to understanding how to ascertain the usefulness and accuracy of various heat variables for defining heat content and heat transport. His pointing out that potential temperature θ is much less conservative than previously assumed is a significant result with important implications. Historically, however, McDougall (2003) suggested two solutions to the problem. 1) Either make the existing equation for potential temperature more

accurate by adding the neglected non-conservative terms, or 2) replace the equation for potential temperature by one based on a more conservative heat variable. Prof. McDougall subsequently devoted a large part of his career promoting solution 2, centred on the use of potential enthalpy and Conservative Temperature, whereas I spent a large fraction of the last 10 years or so working on solution 1, centred on understanding how one might diagnose the non-conservation of potential temperature in order to make the potential temperature equation used in numerical ocean models as accurate as feasible. My main result was to show in Tailleux (2010) that the non-conservation of θ is what is needed to render total energy conservative, which Tailleux (2015) exploited to improve the accuracy of the equation for θ, as well as for improving total energy conservation in numerical ocean models.

It seems important to point out that although the introduction of potential enthalpy by McDougall (2003) was an important stepping stone towards understanding ocean heat and energy conservation, it was clear from day one that it could not be the ultimate and definitive solution to the problem of how to define heat in the ocean. Indeed, using potential enthalpy as our definition of 'heat' requires defining dynamic enthalpy as our definition of 'work', which is inconsistent with Lorenz theory of available potential energy. Moreover, potential enthalpy and Conservative Temperature are still significantly non-conservative, especially in the deep ocean, so that it has always been clear from day one that a heat variable more conservative than Conservative Temperature should exist. As it turns out, such a variable — which is significantly more conservative than Conservative Temperature — has finally been found, and a paper discussing it will be submitted in the next few months. Unsurprisingly for those familiar with the issues involved, such a variable is closely related to the background potential energy entering Lorenz theory of available potential energy.

Prof. McDougall seems to have convinced himself that the temperature variable to be used in numerical ocean models should be the same as the variable used to define heat content and heat transport. But this does not need to be the case. Indeed, it

is perfectly possible to use potential temperature as many numerical ocean models still do, improve its accuracy by retaining its non-conservation as proposed in Tailleux (2015), and diagnose heat content and heat transport in terms of whatever quantity we think defines heat content best. In fact, this is the approach adopted in numerical atmospheric climate models, where there is no direct link with the quantity used to define heat (based on moist enthalpy for instance) and the temperature variable used (potential temperature or liquid potential temperature for instance). The main problem with formulating a numerical ocean model (or the Gibbs Seawater library) in terms of Conservative Temperature is that such a model or software library will become obsolete as soon as a more conservative heat variable is found, that is in 2018.

There is of course nothing wrong with Tailleux (2015), which Prof. McDougall recommended for publication at the time. I can only hope that badmouthing my paper will encourage — rather than put off – readers to decide for themselves what Tailleux (2015) is really about

Isotropic mixing of potential temperature and salinity

We are happy to stand corrected on the issue, and of course support the elimination of confusion about the nature of small scale mixing, but it would be more helpful if Prof. McDougall could explain in simple physical terms to the readers of Ocean Science and to us how having significantly different turbulent spectra for density-compensated θ/S anomalies versus non-compensated θ/S anomalies, which is Smith and Ferrari's finding, is consistent with the idea of isotropic mixing for potential temperature and salinity. Would Prof. McDougall care to explain how does he define 'isotropic' mixing in terms of turbulence spectra?

References

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