

Review of “Isonutral control of effective diapycnal mixing in numerical ocean models with rotated diffusion tensors”

Hochet et al., Ocean Science.

By Sjoerd Groeskamp, 03-11-2017.

Analysing the neutrality of density variables is an important check to understand which variables can be used in which situations. This paper expresses the non-neutrality of a given density variable as an effective mixing, meaning as a diapycnal mixing that is a result of the misalignment between the given density surface and the actual neutral direction. This paper needs to address some important issues. However, after some work, this is publishable. Below are some general and some detailed comments that will need to be addressed to make the paper better and suitable for publications.

General Comments

It is good to search for a materially conserved variable that is as neutral as possible, but I feel this paper oversells the need for such a variable (for example P13-L5, but also other places). Density variables are either a global surface, or they are neutral. They can't be both. For some studies, it is better to have a variable that is perfectly or near neutral, but not materially conserved. Other studies may be better off with a variable that is materially conserved, but not so neutral. This depends on the research question. So, when advocating for a conserved surface, be clear it must give up neutrality and that both kinds have a role to play in the oceanographic community and that this $\gamma(S,T)$ is not the holy grail.

Please specify the salinity variable. There are many different types of salinity variables and they all have a different interpretation and meaning to different communities. If it is practical Salinity, then use S_p . However, when using WOCE data, one should provide the data in Conservative Temperature and Absolute Salinity variables (that should be an editorial decision from a couple of years ago). Please use correct symbols along with it. If it is “model salinity”, please be specific about that.

Please provide a simple graph that illustrates the angles and vectors discussed in the introduction, specifically regarding equation (1)-(3), 11 and 15.

I'm a bit confused about the use of a horizontal and vertical diffusion tensor (Eq. 1). Common practice is to define a isoneutral (mesoscale) and isotropic (small-scale) mixing tensor, not a horizontal and vertical one. Not for convenience, but because of the different physical processes that are related to it. This part should be rewritten to include an isotropic and isoneutral mixing tensor and relate the related physical processes. If not, it should be clearly explained here how the horizontal and vertical mixing coefficient are justified, related to the physical processes that allow for the definition of vertical and horizontal mixing instead of isoneutral and isotropic mixing coefficients.

In addition to the above, Eq (9) diffuses along a density surface. Hence I disagree with the use of K_d in this equation. This is essential small-scale mixing, which is isentropic. This means that it does not require a rotation into the diapycnal direction and should not be in

this equation. I refer to section 4 of McDougall et al 2014 for further reading. Please adapt this in Eq. 9 up until the discussion of this term below eq. 11. If this term is removed from Eq. (9), then Eq. (9) represent the gradient of $\gamma(T,S)$ along the actual isoneutral direction. Of this, the component across its own surface is taken, which then provide the diapycnal transport, due to dianeutral diffusion of itself.

P3-L23-28: Defines isoneutral, isopycnal, dianuetral and diapycnal. I like that. However, it would be worth moving this to an earlier stage in the manuscript. On top of that, throughout the manuscript there are places where isoneutral, isopycnal, dianuetral and diapycnal are confused. For example,

- P3-L32: Second sentence: Isonneutral contribution to diapycnal? Should this not be isopycnal to dianeutral?
- P4-L26, Isonneutral or isopycnal?
- P4-L31, Again, isopycnal not isoneutral. According to your definition, isoneutral mixing is by definition along neutral surfaces and has no diapycnal component.

Please read carefully through this manuscript and make sure this is corrected at every point.

How exactly is the neutral direction calculated? Calculating the exact neutral direction (\mathbf{d}) can be tedious. Details why so, can be found in Appendix B of Groeskamp et al (2016) and probably also in Griffies et al (1998), although I'm not 100% sure of that. This has not been clarified in this paper and I'm wondering how this is done, as this is a major part of the analyses.

Specific Comments

- P2-L24, reference McDougall for Helicity.
- P3-L34: I'm not sure if this statement is correct. If you choose to calculate WMT due to σ_2 , but you know the neutral direction everywhere then I think you may be able to calculate the WMT due to non-neutrality by exploring the σ_2 gradient in the neutral direction. For interpretation of the WMT it is still better to have an "as neutral variable as possible", but I think the statement here is too strong.
- P4-L13: Please explain "Binary Fluid".
- P4-L16: What density are you talking about here? Density depends on pressure to. Are you here talking about $\gamma(S,T)$?
- P4-L33: Neutral density is categorized as a surface of $\gamma(S,T)$, but it also depends on P , x and y .
- P5-L10: This Tensor is defined in Redi(1982), but was not correct for the small slope approximation (GM90). This is explained in McDougall et al (2014). Another version of this tensor is given by Griffies 1998. Few references are at place here.
- P5-L20 I'm perfectly happy if you do not include the nonlinear terms, specially because they are their own form of flux. But a little sentence arguing why these are excluded would be nice.
- P6-L20 I think it should be $V(\gamma_0,t)$.
- P6-L20-25, this was a bit hard to understand. A simple schematic would help.
- In Fig 3., why is the black line smaller than all the others, at great depths?
- P11-L35 - Maybe not Neutral density, if you remove the 5%.

- In section 3, 10^{-4} is chosen as a sort of a reference diffusivity. This seems a bit random. First of all, 10^{-4} is actually very diffusive for an ocean, as in most places it is way lower. Recent inverse and observationally based methods have shown this is probably much smaller, for global values (Zika et al 2010, Groeskamp et al 2017, Waterhouse et al 2014, Lumpkin and Speer 2007, etc). Also, I'm wondering, does the error not depend on what the local rate of dissipation is? If the local rate of dissipation is 10^{-3} (near topography) and the error is 10^{-4} from this method, does that not mean the error is still relatively small? I suggest a deeper discussion of the use of 10^{-4} and the relation to local diapycnal mixing.
- P13-L5: This discussion should include a reference to the attempt of McDougall and Jacket 2005 to quantify the terms in the material derivative of Neutral Density, as some of the terms are small. What does that mean for Neutrality?
- P13-L24: Which exact developments of this paper need to be taken into account and for what purposes exactly? This paper provides no information on how to reduce mixing by changing advection schemes. Please specify.

Figures

For all the figures, improved titles and perhaps some annotation, could help make them more "stand alone". This would improve your paper. A few examples:

- Fig. 1,2 and 3: add grid lines to improve readability of results.
- Fig. 3: Put "A, B, and C" in the figure itself and give clear titles.

Citations

- McDougall, T. J., S. Groeskamp, and S. M. Griffies (2014), On geometrical aspects of interior ocean mixing, *J. Phys. Ocean.*, 44(8), 2164–2175, doi:10.1175/JPO-D-13-0270.1.
- Waterhouse, A. F., et al. (2014), Global patterns of diapycnal mixing from measurements of the turbulent dissipation rate, *J. Phys. Ocean.*, 44(7), 1854–1872, doi:10.1175/JPO-D-13-0104.1.
- Zika, J. D., T. J. McDougall, and B. M. Sloyan, 2010: A tracer contour inverse method for estimating ocean circulation and mixing. *J. Phys. Oceanogr.*, 40, 26–47
- Groeskamp, Sloyan, Zika, McDougall, (2017), Mixing Inferred from an Ocean Climatology and Surface Fluxes, *JPO*.
- Groeskamp, R. P. Abernathey, and A. Klocker, 2016: Water mass transformation by cabbeling and thermobaricity. *Geophys. Res. Lett.*, 43, 10 835–10 845.
- Griffies, S. M. (1998), The Gent–McWilliams skew flux, *J. Phys. Ocean.*, 28(5), 831–841