

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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Response to a short comment by Prof. McDougall

We thank Prof. McDougall for his comments and generally supporting comments. A detailed response can be found below.

- **This paper quantifies the extent of non-neutrality for a few candidate density variables. This paper has some interesting things to say and it should be published in Ocean Science.**

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We thank the referee for his support. Note though that the way we see it, the main aim of the paper is to quantify the effective diapycnal mixing experienced by a few candidate density variables acted upon by neutral rotated diffusion, rather than quantifying the extent of their non-neutrality. Although the two issues are strongly linked, they are nevertheless quite different conceptually.

- **Throughout the paper, it would be more convincing if Conservative Temperature Θ and Absolute Salinity S_A were used in place of potential temperature and practical salinity, since these are the variables that have been adopted by IOC under TEOS-10 and because (1) Conservative Temperature Θ is many times more conservative than is potential temperature, and (2) Absolute Salinity S_A takes into account the varying seawater composition, and in particular, the effect of this varying composition on the specific volume of seawater**

The concept of effective diapycnal diffusivity that our paper focuses on is a priori independent of the particular thermodynamic standard chosen, so that it is unclear to us what aspect of our study could be made more convincing by a different choice of thermodynamic variables. Moreover, it seems important to stress out that our study, like the overwhelming majority of oceanographic studies, relies on the use of reference composition salinity S_R , which is one of the particular form of Absolute Salinity discussed in TEOS-10. The fact that the reference composition salinity S_R that underlies the climatological data and software used for our computations is expressed in practical salinity units does not mean that we are not using Absolute Salinity. Indeed, the fact that S_p and S_R are linearly related to each other means that S_p and S_R are interchangeable in practice, in the same way that the use of degrees Kelvin, Celsius or Fahrenheit represent equivalent ways to express in-situ temperature. The referee's suggestion to use Density Salinity in order to account for spatial variations in composition is interesting, but arguably not feasible in ocean climate studies until numerical ocean models in-

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clude prognostic equations for the other various constituents modifying seawater density at second order. From a mathematical viewpoint, a single salinity variable is not sufficient to describe seawater with varying composition, so that the equations used in numerical ocean models only make sense for reference composition salinity. Since both the referee and TEOS-10 use the term 'Absolute Salinity' in place of Density Salinity, it would be misleading for our study to use the term since it does not account for spatially varying composition. As to the recommendation of using Conservative Temperature in place of potential temperature, our understanding is that it is only a personal recommendation of Prof. McDougall that is not part of the new standard for seawater endorsed by UNESCO. Indeed, our understanding is that what UNESCO endorsed is expressing the thermodynamic properties of seawater in terms of a Gibbs function, whose natural variables are Absolute Salinity, ITS-90 absolute temperature, and pressure. As discussed in Tailleux (2015), it would be very easy to retain the non-conservation of potential temperature in the evolution equation for heat carried by numerical ocean models, thus making it as accurate as that for Conservative Temperature. Using one or the other is therefore purely a matter of personal preference, and should be determined by considerations of the added computational cost of diagnosing the non-conservation of potential temperature on the one hand, versus that of the back-and-forth conversions between Conservative Temperature and in-situ temperature and its concomitant loss of significant digits.

- **Just below Equation (7), and throughout the following equations (particularly Equation (11)), the small-scale turbulent mixing coefficient is not represented correctly. Small-scale mixing is isotropic. It does not diffuse stuff only in the diapycnal direction, but rather it diffuses stuff isotropically. McDougall et al. (2014) discuss this. The equations in this paper should be changed accordingly, in keeping with how mixing actually work in the ocean**

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We do not understand this comment. Indeed, as far as we understand it, the standard rotated diffusion tensor $K = K_i(I - dd^T) + K_d dd^T$ describes isotropic mixing as it is, at least as far as the locally-reference potential density ρ_{lr} is concerned, since the form of K implies for the latter:

$$\frac{D\rho_{lr}}{Dt} = \nabla \cdot (K_d \nabla \rho_{lr}) + N.L., \quad (1)$$

where N.L. refers to the terms arising from cabelling and thermobaricity. As far as we are aware, small-scale mixing of potential temperature (or Conservative Temperature) and salinity (S_R or S_P) is not isotropic, based on the study by Smith and Ferrari (2009). However, even if one were to accept the idea of isotropic mixing for θ and S , it would only mathematically amount to modify the above tensor as follows: $K^* = (K_i + K_d)(I - dd^T) + K_d dd^T = K_i^*(I - dd^T) + K_d dd^T$, where $K_i^* = K_i + K_d$ is a modified isoneutral turbulent mixing coefficient. Clearly, the modified diffusion tensor has exactly the same structure as the non-modified one; moreover, since K_i is about 7 orders of magnitude larger than K_d , with both coefficients having large uncertainties, K_i^* and K_i are clearly indistinguishable from each other.

- **Figure 2.** I think that the x -axis of this figure is correctly labeled, but the caption and its description in the text (line 4 of page 9) is not correct. That is, is this axis the log of the square of the sine, or is it half this? Also, please check this issue for what is plotted in Figure 4: is it the square or not. The referee is right, it is the decimal logarithm of the sinus for figure 2 and 4 (now 3 and 5). We corrected figure 2's caption accordingly.
- **Page 8, Line 11.** here it says "has a gradient much smaller than all ...". I think that this should read "has a gradient much larger than all..." The referee is right, we have corrected this sentence.

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- Page 13, line 3. "depths deeper than -2000 meters". This negative depth would be 2000 m above the sea surface, in the atmosphere. So I think what is meant is "depths deeper than 2000 meters".

Agreed, thanks for pointing this out.

Rémi Tailleux and Antoine Hochet

References

- Smith, K. S and R. Ferrari, 2009: The production and dissipation of compensated thermohaline variance by meso-scale stirring. *J. Phys. Oceanogr.*, 39, 2477-2501.
- Tailleux, R., 2015: Observational and energetics constraints on the non-conservation of potential/Conservative Temperature and implications for ocean modelling. *Ocean Modelling*, 88, 26-37.

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