

# Answer to anonymous referee #3

September 14, 2018

We thank the referee for his/her comments and requests for further clarification of various parts of our paper, especially its main aims. In response, we have rewritten most of the abstract, and expanded some of our arguments in the unclear parts of the paper indicated by the referee. We have also removed most of the mathematical contents from the introduction to put it in subsequent sections or in the Appendix to facilitate understanding of the main aim and objectives of the paper. We hope that the referee will find our response satisfactory and the revised paper sufficiently improved to warrant publication. Note that the parts highlighted in blue in the revised version of the paper are the ones corresponding to new material not present in the previous version of the paper. Material that has been simply shuffled around or simply rephrased in only minor ways for clarity has not been highlighted in blue.

**‘One cannot define a density variable that is both a material surface, and along which parcel exchanges are energetically neutral. Thus ocean models that rotate the diffusion tensor (a la Redi 82) to a neutral frame, in order to separate large isopycnal from much smaller diapycnal diffusivities, will incur inadvertent diapycnal diffusion due to the isopycnal part. This paper seeks to make this clear, and compare the relative spurious mixing errors incurred by the use of various neutral density variables. The analysis is done not with an ocean model, however, but with hydrography. The results are that it doesn’t matter too much, except in about 5% of the locations.’**

## **Response:**

We disagree that our paper is about comparing the relative spurious mixing errors incurred by the use of various neutral density variables. Indeed, the idea that spurious mixing would result from constructing a rotated diffusion tensor based on mixing directions other than the isoneutral and dianeutral directions relies on two main assumptions: 1) the measured diapycnal values of  $O(10^{-5}m^2/s)$  pertain to the neutral rotated diffusion tensor  $\mathbf{K}_n$ , not  $\mathbf{K}_\gamma$ ; 2) that an ocean modeller would change the mixing directions without altering the values of the mixing coefficients. Physically, it is key to realise that each density variable  $\gamma$  comes with its own distinct diapycnal diffusivity  $K_\gamma$ . As a result, changing the mixing directions in a rotated diffusion tensor without changing the mixing coefficients is what would cause spurious diapycnal mixing; the correct approach of changing both the mixing directions as well as the mixing coefficients in a consistent way would a priori not introduce spurious mixing. As a result, we believe that the contamination of diapycnal diffusivities  $K_d^\gamma$  pertaining to density-like variable  $\gamma(S, \theta)$  by isoneutral mixing should be a real physical effect if we accept that the dianeutral and isoneutral mixing coefficients entering neutral rotated diffusion tensors are the correct ones. The results are important because the diapycnal diffusivity  $K_d^\gamma$  pertaining to a globally-defined density variable  $\gamma$  is a priori easier to constrain from observations using Walin-type water mass analyses than  $K_d^n$ , even if it is the latter one wants to know about in practice. To that end, however, one needs to understand how the various diffusivities are inter-related. Rather than saying that the above issue does not matter too much except in about 5% of the locations, it seems more accurate to regard our results as stating that the problem matters whenever closeness to neutrality is hard to achieve, such as in the Southern Ocean for instance, which is a key region controlling many aspects of the large-scale ocean circulation. In other words, the potential importance of the problem is not necessarily well quantified by apparent smallness of the number 5%, although it is hard to say much more at this stage.

**Action:** We have rewritten the abstract and parts of the conclusion to make the above ideas clearer.

My chief concern is that, as implied above, while this paper seems to be directed toward ocean models, no attempt is made to connect their results to what is actually done in models, beyond the idea of the rotated Redi diffusion tensor. What neutral vectors do MOM, NEMO and the MITgcm use, for example? How would the present results depend on model resolution? Are the implied errors significant relative to other errors, or are they negligible? I very sincerely have no idea what to do with the information the authors have given me, or whether it is something a real modeler needs to worry about, or not.

**Response:** We agree that the paper is indeed directed toward ocean models, but not in the sense assumed by the referee and only to some extent. Indeed, the underlying question addressed by our paper is how can we use observations to constrain the dianeutral and isoneutral mixing coefficients entering neutral rotated diffusion tensors, given that such coefficients do not a priori relate to the mixing of any globally-defined density variable? In this regard, our paper should be of interest to ocean modellers, because it is concerned with the fundamentals of how to construct and constrain neutral rotated diffusion tensors. But our paper is also, perhaps more importantly, directed towards the oceanographer interested in constraining the dianeutral mixing by means of a Walin-type water masses analyses. However, because such an approach yields information about the diffusivity pertaining to the particular density variable  $\gamma$  used to do the analysis, one needs to understand how to relate the  $\gamma$ -diapycnal diffusivity to the dianeutral and isoneutral mixing coefficients. The main point of the paper is that it is not really possible for Walin-type inverse methods to constrain the dianeutral diffusivity without knowing something about the isoneutral diffusivity. The same ideas pertain to the estimation of spurious diapycnal mixing by means of the Winters et al. approach, because such an approach can be viewed as a Walin-type water mass analysis based on the use of Lorenz reference density. Re-reading the manuscript, it seems to us that the referee's confusion might be due to the failure of the abstract to convey our message sufficiently clearly, so we decided to rewrite it entirely. Given that our paper is not directed toward ocean models, we feel that the questions of what neutral vectors are used by MOM, NEMO or the MITgcm are irrelevant to our argument as is model resolution. **Action:** We have rewritten parts of the abstract, introduction and conclusion to clarify the issues.

## Specific points

**P 3, L18-28: This argument is indecipherable. What is  $\hat{\rho}$ ? Where does (7) come from? (the authors say 'The expression for the neutral vector becomes...' 'becomes' from what? And there is an error below (8): that  $\mathbf{J}$  must be  $\partial\gamma/\partial S$ , not the Jacobian term, which is already in (8).**

We believe that our derivations are correct as they stand, as this is no more than standard calculus associated with working with different kinds of coordinates. However, the reviewer's comments suggest that your presentation of the derivation could be made more transparent. To this end, we have spelled out the different steps of the derivations in more details, which we hope will help the reader reproducing the derivations.

**Regarding Speer's Keff, is (20) correct? It would say  $K_{eff} = K$  if  $K$  is constant... surely that's not right.**

First note that in (20)  $K$  is a tensor so that

$$K\nabla\gamma.n = |\nabla\gamma| \underbrace{(K_i \sin^2(\nabla\gamma, d) + K_d \cos^2(\nabla\gamma, d))}_{=\kappa} \quad (1)$$

$\kappa$  can be a constant only if the mixing is defined as being along the direction of  $\nabla\gamma$  (which is not the case here, plus the mixing coefficient would need to be spatially constant). Now let's assume that it is the case,

for instance in a turbulent flow with only molecular mixing. Then we would indeed have  $K_{eff}^{speer} = \kappa$  with a total diffusive flux across the  $\gamma$  surface equal to  $\kappa \int_{z_r=const} \nabla \gamma \cdot \mathbf{ndS}$ . However we believe that formula 20 ( which is formula 12 from Speer 1997) is meant to be applied on a climatological (time averaged fields) quantities and not on fully turbulent flows for which we agree that it would be completely useless. In our article we compared our formulation to Speer’s formulation in case the reader is more familiar with the latter however it is not used in the remainder of the study and thus not discussed further. We will remove the term ‘effective diffusivity’ when citing Speer 1997’s definition and rather refer to ‘average cross isopycnal mixing’ which is hopefully less confusing.

**Conversion to density (or any other tracer) coordinates has been done over and over in the literature. Admittedly, it’s a messy literature, but it doesn’t need to be repeated here. In particular, there is only one effective diffusivity. I haven’t read Speer 97, but I’ve read WD96, Nakamura 96 and Shuckburgh and Haynes 03 (SH03) in great detail — the latter is the most useful. There is an error in the effective diffusivity formulation of WD96 that stems from their incorrect eq. (9), and it seems to have made its way to this paper in (18), though I am not certain because I’m a little uncertain about  $\nabla_{z_r}$ . I would in any case recommend the authors look at SH03, equations (6) & (7) (and compare to WD96 eq. (12)). That said, I doubt it will affect the results much.**

**Response:** Thank you for the reference to SH03 that we were not aware of, and which we added to the manuscript. However, we disagree that there is only one effective diffusivity. In WD96 or Nakamura 96 for instance, the effective diffusivity is related to the net diatracer flux integrated over an isotracer area divided by a reference laminar area, and scales as  $K_{eff} = \kappa(A_{turbulent}/A_{laminar})^2$  where  $\kappa$  is the molecular diffusivity, and  $A_{turbulent}/A_{laminar}$  is the ratio of the turbulent isotracer area over a notional laminar value. In that case, the approach is essentially equivalent to the Osborn-Cox (1972) approach to defining a turbulent diffusivity. In our paper, the effective diffusivity however refers to the (turbulent) diffusivity experienced by some material density variable  $\gamma$  given a rotated neutral diffusion tensor with isoneutral and dianeutral turbulent mixing coefficients unrelated to the mixing properties of  $\gamma$ . Moreover, each definition of effective diffusivity tends to have their own specificity and twists, see for instance that of Lee and Nurser, which is not directly related to that of WD96 or Nakamura 96. We feel that our approach is sufficiently context-specific as to warrant going into some details about how we define and compute our effective diffusivities, as we feel that not doing it would be assuming too much from the reader. We were indeed aware of the error in WD96, but a priori, our derivations do not follow that of WD96 and therefore believe that they are correct.