# Final Response

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We thank Prof. McDougall, Dr. Groeskamp and anonymous Referee 2 for their generally positive assessment of our paper and for their comments/suggestions for clarifying it. In this final response, we synthesize the outcome of the discussion/exchange of views and explain how we plan on revising the paper to address these. This is done point by point for the comments by anonymous Referee 2, as these are addressed here for the first time, but thematically for the comments raised by Prof. McDougall and Dr. Groeskamp, since these have already answered point by point in previous responses published on the discussion page.

# 1 Response to particular points

## 1.1 Isotropic character of the dianeutral component of rotated diffusion tensors

Both Prof. McDougall and Dr. Groeskamp have suggested to make the dianeutral component of rotated diffusion tensors isotropic as per a recommendation by McDougall et al. (2004). In a previous response, we counter-argued that as far as we understood the issue, whether to make the dianeutral component of the rotated diffusion tensor isotropic or not depended on one's definition of the isoneutral mixing coefficient $K_i$. In our paper, we adopted the formulation of rotated diffusion given by Eq.(7.9) of Stephen Griffies textbook 'Fundamentals of Ocean Climate models', which we believe is the one currently in use in the majority of current numerical ocean climate models. As explained in a previous response, McDougall et al. (2014)'s suggestion is physically equivalent to replace our definition of $K_i$ by $K_i + K_d$; since $K_i$ is about 7 orders of magnitude larger than $K_d$, McDougall et al. (2014)'s suggestion amounts to argue that it is somehow possible to know the normalized value of $K_i$ up to its $7th$ decimal, which is clearly not true given current uncertainties on mixing measurements. In a previous response, we also expressed our surprise at the view that the dianeutral component of rotated diffusion should be made isotropic, given that it seems to conflict with our interpretation of the results by Shafer-Smith and Ferrari (2009). Indeed, in this paper as well as in others, turbulence spectra are usually found to be very different in the dianeutral and isoneutral directions, which in our view is the definition of anisotropy. It is to be noted that McDougall et al. (2014) appear to regard their suggestion as self-evident, as they do not provide any reference in support of their view. *Action: Discussion of the above points will be added in the revised version of the manuscript.*

## 1.2 Definition of salinity. Potential temperature versus Conservative Temperature

We will clarify our definition of salinity in the revised manuscript along the lines mentioned in our previous response to comments. Regarding the use of Conservative Temperature, we accept Prof. McDougall's result that CT is indeed more conservative than potential temperature. While it would be relatively easy to redo the computations with CT, this is not needed here because the concept of effective diffusivity does not require our temperature variable to be as conservative as possible. This is because the non-conservation of potential temperature only enters the expression for the nonlinear production of the density variable considered, not the effective diffusivity.

## 1.3 Other comments

All remaining comments and points raised by Dr. Groeskamp and Prof. McDougall will be addressed in the revised manuscript along the lines discussed in our responses to comments already posted. One important application

of our results is in the construction of a simplified representation of ocean heat uptake for use in simple climate models, as explained in the preprint posted on https://arxiv.org/abs/1708.02085

## 2   Comments raised by Anonymous Referee 2

We thank the referee for his/her comments and for being generally supportive of our paper. We are also very grateful for his/her many correction of our English that will be included in the revised manuscript. Our response to his/her comments are provided below.

## General Comments

*I think the results presented in the paper are interesting and novel, but it isn't clear to me exactly what the practical recommendations of the study are. You have shown that the non-zero angle between the gradient of the density variable and the direction of diffusion give significant leakage from isoneutral to dianeutral mixing, which is generally smaller with the neutral and reference densities than with the potential densities, and you make the point that the neutral density is less useful in this context since it is non-material. You demonstrate that this leakage creates global mean effective mixing coefficients substantially larger than $10^{-4}$ m$^2$/s below 3,000m depth, but when the largest 5% of the angles are removed from the sum the global mean Keff is reduced by an order of magnitude. I would have thought that the latter is the more interesting and useful number, since it is more typical of the ocean outside the Southern Ocean. In addition, when this pesky 5% are removed the profiles for the different density variables also get much closer together, implying that the choice of density variables has a stronger influence on the extreme values of the angle than on the more typical regime in the other 95% of the ocean. Although the discussion in the Conclusions section is relevant and valid, I feel that the qualifications above should be included in the overall conclusions*

okay, thank you, we will discuss further on the 5% in the conclusion.

## Specific corrections

- *It would help the clarity of this section considerably if a sentence or so summarising what McDougall and Jackett mean by "fictitious mixing" were included here, as well as a clear statement of how it differs from the effective mixing discussed here.*
  The distinction between McDougall and Jackett's concept of fictitious mixing and our concept of effective mixing is discussed in details in the introduction of our paper. Perhaps the referee missed this?

- *It would be appropriate to mention here that Megann (Ocean Modelling, 2018) recently showed that the Lee et al approach gave diapycnal transformation rates in a 1°NEMO model that were not especially sensitive to the choice of potential density coordinate used.*
  Thank you for pointing out the relevant reference to Megann (2018). The lack of sensitivity to the choice of potential density is interesting, for it suggests that the effective diffusivity computed by Megann (2018) might then be dominated by spurious numerical mixing. We will add a comment to this point in our revised manuscript.

- *P4L19: This section would be clearer if the Lorenz reference density were defined earlier in the paragraph, so that its relevance to the Lorenz reference state were more obvious.*
  Thank you for the suggestion, which we will implement in our revised manuscript.

- *P4L10: "There is no question that. . ."; I would dispute that, since the APE method has generally only been used for a model that is unforced (spinning down, that is), so does not give the complete picture of the numerical mixing that occurs when the model is run in a more "normal" and useful way.*

Note here that we were careful in our paper to restrict our statement to the case of a linear equation of state. Even though we agree that Winters et al. 1996 rigorous approach to quantifying diapycnal mixing has been primarily restricted to freely decaying simulations, there is in principle no problems with applying the method in a forced-dissipated context as shown by the following formula:

$$\frac{\partial \gamma_r}{\partial t} = \frac{1}{A(z_r)} \frac{\partial}{\partial z_r} \left( A(z_r) K_{\text{eff}}(z_r) \frac{\partial \gamma_r}{\partial z_r} \right) + \text{forcing} \tag{1}$$

with:

$$\text{forcing} = -\frac{1}{A(z_r)} \frac{\partial}{\partial z_r} \left( \int_{V(z_r)} f_\gamma dV \right) \tag{2}$$

The derivation of this equation is shown in appendix B. So if $f_\gamma$ is known for each time step, it can be integrated in each $z_r$ classes and then used in equation (2) and (1) to obtain $K_{\text{eff}}$. Thus in theory unforced spinning down experiments are not required to use this method. The method is considerably more difficult and subtle to use for a nonlinear equation of state, however, because then cabelling and thermobaricity introduce an additional diapycnal flux that is hard to estimate and not easily separated from the 'linear equation of state' part of the diapycnal diffusive flux. In any case, these considerations are not directly relevant to our paper, because our approach to estimating effective diapycnal mixing is not based on monitoring the evolution of the reference state, but on numerically estimating the analytical expression obtained for it. In this respect, our approach is very similar to that used by Jackett and McDougall for estimating fictitious mixing. APE theory only enters the problem here for the purpose of introducing Lorenz reference density as a generalised form of potential density that is more neutral than any other existing potential density. In contrast to Winters et al. approach, we do not use sorting at all.

- *Figure 3: Is there a mistake here? Panels A and B appear to be identical, where I would expect the values in B to be quite a bit different, since Ki is a multiplier in the expression for Keff., and presumably the former is quantitatively rather different in the two cases?*
  We have checked that there is no mistake. The results in the two panels are indeed very similar but not identical (which is undeniably hard to see from panels A and B). The values on panels A and B are mostly set by only 5% of the points on each surface and it appears that for this 5% (located mostly in the ACC) the isoneutral mixing coefficient is of the order of 1000 m$^2$/s which leads to very similar $K_{\text{eff}}$ curves when displayed on a log scale. We did the same calculation for $\sigma_2$ without the 5% of the largest values on each $z_r$ surface and obtained much more contrasted $K_{\text{eff}}$ between the case with $K_i = 1000 m^2/s$ and $K_i$ of Forget et al. 2015.
  We will add a sentence in the manuscript to explain why their is this similarity between panel A and B.

- *Figure 4: The colour legend would be easier to interpret if the annotations of the log scale were in integer increments, rather than the apparently uneven ones (approximately, but not exactly, 0.9!) used here.*
  okay, we will improve this.

- *As I mentioned earlier, the analysis that flows from Equation 7 is only strictly valid in the absence of surface forcing. It should therefore be noted, particularly in the discussion of Figure 4, that much of the Southern Ocean - as well as the Atlantic north of 50N - is directly ventilated and so a good argument could be made for excluding it from the global mean in this calculation. I would guess that this might be a physically-based argument for the exclusion of the 5% of points that have large angles; perhaps coincidentally corresponding roughly to the directly ventilated regions.*
  We disagree with the referee's interpretation of our approach, which, as stressed above, is very different in its principle from Winters et al.'s sorting approach. We insist that our analytical expression for the effective diffusivity is valid for a forced/dissipated ocean with a nonlinear equation of state, as is the case of Jackett and McDougall's expression for the fictitious diffusivity.

- *It would also be informative if the profiles obtained for $K_{eff}$ using the various density definitions were at least qualitatively compared and contrasted in this section with those estimated from observations, with those used in model mixing schemes, and also with those diagnosed for numerical mixing in models by the studies*

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*already cited here (which can be an order of magnitude larger than the former). This comparison would put the calculated Keff values in context, and would also illuminate the importance (or not) of the 5% of extreme values for the angles in the global means.*

Ok thank you for this idea. We will add in the revised manuscript a figure showing the same $K_{eff}$ but calculated from an estimation of local dianeutral diffusivities (from the same Forget et al. 2015 database) to show how it compares with the one obtained from the isoneutral diffusivities and discuss qualitatively the values with those found in the literature.