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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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Discussion paper

Response to Sjoerd Groeskamp's review

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December 17, 2017

We thank the referee for his comments and for being generally supportive of our paper. Our response to his comments are provided below.

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.
As the referee says, the particular type of density variable of interest depends on the research question. In the present paper, the research question is the determination of

the effective diapycnal diffusivity for density variables acted upon by a neutral rotated diffusion tensor. Since the concept of effective diffusivity is physically well defined only for a density variable that is both globally defined and exactly material, it seems only normal that we should emphasize the need for a materially conserved variable as neutral as feasible, since this is the only way to make the effective diffusivity as close as possible to the specified diapycnal diffusivity. We don't understand how our paper can oversell a property that is essential for the present purposes. If the referee thinks that this is the case, he should take advantage of the public character of the discussion to explain to the reader the benefits of density variables emphasizing neutrality over materiality, as we do not think that we are sufficiently qualified to make such a case. In any case, the referee seems to make a very subjective judgment here, as nowhere in our paper do we make the claim that $\gamma(S, \theta)$ density variables are 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 Sp. 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.
Our study assumes fixed seawater composition and therefore relies on the use of Reference Composition salinity S_R, which is one of the particular type of Absolute Salinity discussed in TEOS10. As far as we are aware, this has been the default assumption in the overwhelming majority of oceanographic studies since the introduction of the last thermodynamic standard EOS81. It would seem therefore that there is only a need to be more specific in cases one would want to account for variable composition seawater by means of Density Salinity, another type of Absolute Salinity, as recommended by TEOS10. Note that Practical salinity is just one particular way to express Reference Composition Salinity, in the same way that degrees Celsius, Kelvins or Fahrenheit

represent different ways to express in-situ temperature. In all cases, there exists a one-to-one linear relationship allowing one to be expressed in terms of the other. As a result, even though the software used in our study to compute density takes practical salinity S_p as its argument, it could easily be converted with two lines of code to take reference salinity S_R as its argument. This one-to-one correspondence between S_p and S_R makes it possible to regard all oceanographic studies as using S_R as their salinity variable rather than S_p, contrary to what the referee seems to believe. As to the referee's remark that the use of Conservative Temperature and Absolute Salinity should be imposed by editorial decision, we believe that it would be a sad day for science and academic freedom if it ever were to happen, since it would be akin to a decision to ban the use of the Celsius and Fahrenheit scale in favor of the absolute scale to express in-situ temperature; we therefore can only hope that the referee's remark was made in jest.

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

I'am 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.
We are surprised by this comment because we make it clear in our paper that the

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use of horizontal/vertical diffusion is something that was done in early numerical ocean models, not in the most recent ones. As is well known, one of the main problem with horizontal/vertical diffusion tensors is that they are affected by the so-called Veronis effect, which historically is why they were subsequently replaced by the use of neutral rotated diffusion tensors. The main aim of this part is to demonstrating the usefulness of the concept of effective diapycnal diffusivity, by showing that it is naturally capable of quantifying the Veronis effect.

In addition to the above, Eq (9) diffuses along a density surface. Hence I disagree with the use of Kd 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.
This point was also raised by Prof. McDougall in a separate comment. Our reply is repeated here. 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:

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

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:

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$K^* = (K_i + K_d)(I - dd^T) + K_d dd^T = K_i^*(I - dd^T) + K_d dd^T$, 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.

P3-L23-28: Defines isoneutral, isopycnal, dianeutral 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: Isoneutral contribution to diapycnal? Should this not be isopycnal to dianeutral?**

- **P4-L26, Isoneutral 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.**

We believe that all example that you give are correct in our manuscript. For example "isoneutral contribution to diapycnal" means that we are calculating the contribution of the isoneutral mixing on the mixing across our γ variable. In numerical models it is customary to define the mixing according to the neutral and isoneutral directions. The question that we address here is: using a $\gamma(S, T)$ variable as defined in the manuscript to assess the dia-γ mixing (or diapycnal according to our definition) what part of it is due to isoneutral mixing ?

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Discussion paper

How exactly is the neutral direction calculated? Calculating the exact neutral direction (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.

The neutral vector is calculated from the gradient of the locally referenced density and used to calculate the angle between the neutral vector and $\nabla\gamma$ in formula (A1)(we have added this precision in appendix A). As the neutral vector is not calculated from the gradient of γ^n i.e. we don't use $s_x = -\left(\partial\gamma^n/\partial x\right)/\left(\partial\gamma^n/\partial z\right)$, we do not have to use an interpolation method as described in appendix B of Groeskamp et al 2016 to avoid spikes when $\partial\gamma^n/\partial z \approx 0$. Note that our results show that γ^n is not everywhere exactly perpendicular to the neutral direction. As a result calculating the neutral vector with γ^n might not be the best choice in some regions.

1   Comments

P2-L24, reference McDougall for Helicity.
ok, thank you

P3-L34: I'am not sure if this statement is correct. If you choose to calculate WMT due to sigma2, 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 sigma2 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.
The idea behind this sentence is to say that the total diapycnal diffusion through a

surface cannot be attributed only to dianeutral diffusion since neutral surfaces do not exist (it might be small, we don't know yet at this stage, but it cannot be zero).

P4-L13: Please explain "Binary Fluid".
ok

P4-L16: What density are you talking about here? Density depends on pressure to. Are you here talking about y(S,T)?
ok, we have replaced density by density-like variable

P4-L33: Neutral density is categorized as a surface of y(S,T), but it also depends on P, x and y.
you are right, we have added a sentence to explain why we have made this choice

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.
ok but we don't use the small slope approximation.

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.
yes, it sounds like a hazardous approximation to us to include the nonlinear terms in the effective diffusivity term since there is no reason to think that the NL behave like a diffusive one. Therefore we avoided doing so. Note that in our calculation we do not neglect or do any hypothesis on the non-linear terms. We have removed this sentence

so that it is now hopefully less confusing: the diffusive flux of γ is just (obviously) $-\mathbf{K}\nabla\gamma$.

P6-L20 I think it should be $V(y0, t)$.
no sorry we don't think so.$V(y, t)$ is the volume of ALL water parcels of reference density y0 satisfying $ymin < y0 < y$

P6-L20-25, this was a bit hard to understand. A simple schematic would help.
ok

In Fig 3., why is the black line smaller than all the others, at great depths?
The black line is γ^T, it thus suggests that γ^T is closer to the neutral vector than all other variables under consideration here at great reference depth. Note that it is not "great depths" but great reference depth which is not exactly the same (sorry if you already know that).

P11-L35 - Maybe not Neutral density, if you remove the 5%.
the 5% calculation is just made to show that the discrepancy are very localized in space, we are not suggesting to use the neutral density whitout 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.**

10^{-4} refers to the widely cited Munk and Wunsch figure, which is widely regarded as a canonical value. We agree that, as it is, it is not very clear. We have therefore added the MW reference. First of all, one of the most important point is that the effective diapycnal(or diaγ) mixing depends on the γ variable used so that $10^{-4}\ m^2/s$ could be considered as "large" for a density variable that is approximately neutral but small for a variable that is far from being neutral such as σ_0. Secondly, our figures are computed from global-mean calculation and are thus not made to be compared with local values. Concerning the variables with local gradient close to the neutral vector, the high values that we found are due to very localized discrepancies (localized on less than 5% of each surface) between the neutral vector and the γ gradient this is why it is not in contradiction with the fact that global values give much smaller values than 10^{-4} at least for γ^n. Plus: inverse and observationally based method calculate local values of mixing in the neutral direction i.e. dianeutral mixing (it seems to be the case at least for Groeskamp et al 2017 and waterhouse 2014), we calculate the effective diapycnal mixing through a $\gamma(S,T)$ due to isoneutral mixing which is not the same .

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?

Thanks for suggesting the reference to McDougall and Jackett (2005), which will be done in the revised manuscript. Regarding the consequences for neutrality, we don't know yet. However, our study points to a clear path to elucidate the issue. Indeed, our study makes a prediction for the effective diapycnal diffusivity of a few density variables assuming that neutral rotated diffusion is correct. Because the density variables considered are mathematically well-defined, it should be possible, at least in principle, to make a prediction for their effective diapycnal diffusivity directly from first principles

(from the analysis of the non-averaged Navier-Stokes equations) by linking it to microstructure measurements. Provided that this is possible, we would have an objective and independent way to predict the effective diapycnal diffusivity, thus allowing us to test whether neutral rotated diffusion is the right way to mix heat and salt in numerical ocean models or not. This is an avenue of research that we are currently exploring and that we hope to report on in the near future, but which we are stiff far from having resolved yet.

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.

As explained in our paper, the concept of effective diapycnal diffusivity is at the heart of most inverse methods as well as of some recent attempts at diagnosing spurious numerical mixing due to numerical advection schemes, but so far, the assumption has been that the contamination due to isoneutral mixing was small and therefore negligible. Our results are important for demonstrating that this assumption is not justified in practice, and hence that accurate ways to remove the contribution of isoneutral mixing in the above said methods will need to be figured out.