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Ocean Science Discussions

Interactive comment on "Understanding mixing efficiency in the oceans: do the nonlinearities of the equation of state for seawater matter?" *by* R. Tailleux

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In this paper, the author examines the effect that a nonlinear equation of state has on the energetics associated with turbulent mixing. The nonlinearity can alter the energy transfers (relative to those found traditionally for a Boussinesq fluid and a linear equation of state) associated with mixing and subsequently used to define a mixing efficiency. This result is demonstrated for seawater under a variety of temperature, salinity and pressure conditions. These ideas will be new to many in the oceanographic community and I would like to see this material published. However, I believe that a number of revisions are required for correctness and to improve the readability and impact of the paper.

Major comments:

1. This paper requires a clear physical description of how $D(APE)$ and $W_{r,mixing}$ are linked. Both affect the potential energy budget and the claim that these two quantities represent "two fundamentally distinct types of energy conversion" (p. 384, l. 5-6; and similar statements elsewhere) is stronger than can be made on the basis of the material in this manuscript. The fact remains that $D(APE) = W_{r,mixing}$ for a Boussinseq fluid with a linear equation of state. This equality is evidence that there is a connection between dissipated APE and conversion to GPE_r (the connection is still present, although less direct, when dealing with a non-Boussinesq fluid and nonlinear equation of state). Appealing to serendipity as the reason for this equality in the paragraphs on p. 376/7 surely conceals key elements of physics. I believe the clue to that physics is apparent in equations 17 and 18, namely that both $D(APE)$ and $W_{r,mixing}$ are dependent upon the same fundamental physical process, i.e. molecular diffusion. A diagram of the energy conversions would probably be a useful addition to address this issue.

2. I consider the main point in this paper is that mixing does not necessarily raise (and can in fact lower) the gravitational potential energy of the background state at the same rate at which available potential energy is removed by diffusion. Consequently, this factor needs to be borne in mind when efficiencies for turbulent mixing processes are calculated or applied. I do not believe this paper should proceed to argue for or favor one definition of "mixing efficiency" (based on $D(APE)$) over another (based on $W_{r,mixing}$). The reality is that both definitions (equations 15 and 16) have their place. In fact, it is often the change in gravitational potential energy of the background state (based on $W_{r,mixing}$) that we are most interested in finding, but which this paper shows to be most affected by nonlinearity and, consequently, are hardest to calculate. Accordingly, the notion that a mixing efficiency based on $D(APE)$ is in some sense either "correct" or the only quantity of interest should be changed throughout the manuscript.

3. This paper currently does not "stand-alone" very well. In particular, the material relies heavily on the author's in-press paper (Tailleux 2008) and references to Fofonoff

(1998, 2001). The author should develop some ideas more fully in the text to increase the impact of this manuscript: a) more discussion of the physical meaning of the parameter in equation 4 (see also comment 4); b) the use of a reference state (p. 381, l. 14 - it is unclear if the $\alpha_r P_r/\rho_r C_{pr}$ term in equation 17 is evaluated at a given position or if it is evaluated for the parcel at that given position, but which is in general at another position in the reference state).

4. The discussion of the energetics in this paper usually requires consideration of the whole (closed) fluid volume, not a local balance. This is not made clear in equations 2 and 3 for instance. What happens to the GPE evolution when the parameter in equation 4 varies (or even changes sign) within the volume (presumably this is the case for some of the instances considered in Figure 1)? The statement on p. 383, l. 1-3 is confusing because $W_{r,mixing}$ is a volume integrated quantity, but the parameter $\alpha P/\rho C_p$ is evaluated on a pointwise basis? The "dead" and "exergy" components of internal energy discussed on p. 377 also require global knowledge of the fluid volume.

5. It is not possible to see much detail in Figure 1b for large values on the vertical axis - would it be clearer to plot the vertical gradient of $\alpha P/\rho C_p$? (Incidentally this must have a better name than the "thermodynamic efficiency-like quantity" used in the caption). The text discussing Figure 1 on p. 381 should be expanded. For instance, what was T_{max}, and what range of temperatures were considered (my rough estimate gives ΔT of order 1-10 degC)? This would be useful to assess whether or not the 27 cases considered represent strong stratifications for oceanic conditions. How much does the parameter $\alpha P/\rho C_p$ vary in the volume (this is hard to judge currently using the scale in Figure 1b)?

6. Figure 2 is not particularly useful, and the caption, axis labels and text in section 4 are inconsistent with the data that is plotted (I am guessing that the text is the correct description). In any case, the axes scales hide much of interest, e.g. they have insufficient resolution to see the regime where $D(APE)$ and $W_{r,mixing}$ are approximately equal. Further, APE seems to be of little physical relevance (especially when the en-

ergetics budget is focused on energy transfer rates). However, as the plots do show that APE serves to define a unique parametric representation (for a given case) of the relationships with $D(APE)$ and with $W_{r,mixing}$, I wonder if a more instructive plot would be $W_{r,mixing}$ vs $D(APE)$. Such a plot should show the region in which traditional interpretations of mixing efficiency are not useful. The author should explore a key or set of labels that allow the curves to be related to the 27 cases (or an important subset thereof) illustrated in Figure 1 - without this, statements in the discussion (such as para. 3, p. 383) are not well supported.

Minor comments:

These include a number of examples of poor style and factually incorrect statements that require addressing.

1. throughout: I find the terminology "turbulent diffusive mixing" clumsy; it does not add any extra meaning to the accepted terminology "turbulent mixing".

2. p. 373, l. 4-6: Research into stratified turbulent mixing has been undertaken for many more reasons than "...to design physically-based parameterizations, etc..."

3. p. 373, l. 13-14: How the downward transfer of heat balances high-latitude cooling is not explained clearly. Incidentally, this constitutes an application (and should not be included as fundamental reason, p. 373, l. 7) for studying stratified turbulent mixing.

4. p. 374, l. 7: Munk (1966) found a K_ρ that was consistent with measured vertical T, S and tracer profiles in the North Pacific. He did not use a constraint on meridional heat transport to find a value of K_ρ.

5. p. 374, l. 9: It would be more accurate to replace "enough stirring" with "enough energy required to maintain stirring".

6. p. 374, l. 13: Some references to observational studies would be appropriate here.

7. p. 374, l. 15: "...stirring much debate..." is an unfortunate choice of words (and I

think that Munk & Wunsch, 1998, arguably set a minimum standard for wordplay in an oceanographic paper).

8. p. 374, l. 23: "stirring energy" is better expressed as "energy to support stirring" (i.e. tke).

9. p. 375, l. 2: Munk & Wunsch (1998) estimated the rate of GPE increase due to turbulent mixing by requiring the stratification to be maintained against the rate of upwelling. They did not invoke a balance with the rate of GPE loss due to cooling.

10. p. 376, l. 3-4: Reword sentence - hydrostatic pressure variation does not imply $\alpha/(\rho C_p)$ is a constant. Reference to "a hydrostatic fluid" does not make sense.

11. p. 376, l. 24 (or elsewhere): Note that positive $W_{r,mixing}$ represents the conversion rate from GPE_r to exergy, as indicated by equation 8.

12. p. 379, l. 11: The time average has to be applied such that the averaging period is not long enough for significant viscous dissipation from the mean flow.

13. p. 381, l. 19: I think that P_{max} should be P_{min}.

14. p. 384, l. 2-5: the long sentence is unclear, in particular "... and not the same kind of conversion associated with the irreversible conversion, etc..."

15. p. 384, l. 16: The claim that $W_{r,mixing}$ always underestimates $D(APE)$ is presumably too general - it applies for seawater in this paper.

Interactive comment on Ocean Sci. Discuss., 6, 371, 2009.