

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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I thank the referee for his/her constructive remarks, which I found very useful in preparing a revised version of this paper. In the following, I present a response to both major and minor comments, which refers to the revised version posted as supplementary material on the interactive discussion website.

Response to major comments

1. This comment suggests to provide more physical insights into the links between $D(APE)$ and $W_{r,turbulent}$, in order to clarify why these two quantities become

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equal in the context of the L-Boussinesq model, which constitutes the main framework in which these two quantities have been discussed. To that end, I have emphasised the fact that both quantities depend on molecular diffusion, as well as on the gradient of the reference vertical position $z_r = z_r(x)$ of the isopycnal surfaces. I also show that $W_{r,turbulent}$ appears as a leading order term in a series expansion in P of $D(APE)$ around $T = T_r$, the correction terms vanishing in the limit of a linear equation of state. Figure 1 now illustrates the overall energy cycle for freely decaying turbulence, and compares the “traditional” interpretation of Winters et al. (1995) versus the new one proposed by Tailleux (JFM, 2009).

2. This comment argues against promoting one particular definition of mixing efficiency (that based on $D(APE)$) against that based on $W_{r,turbulent}$ on the basis that mixing efficiency is often determined by measuring net changes in GPE_r over a turbulent mixing event. My opinion, however, is that the possibility of measuring the mixing efficiency from measuring the net changes in GPE_r , accounting for the factor ξ , is quite distinct from the issue of how the concept of mixing efficiency should be defined. In fact, providing a clear physical basis for the concept of mixing efficiency seems to be important for ensuring a sound physical basis for future empirical investigations. I find it important, therefore, to clarify the physical arguments in favour of each definition, and to take a stand on which definition I consider to be physically more sound, making sure in the process to separate the issue of defining mixing efficiency from that of measuring the latter from measuring the net changes in GPE_r , whose validity is unaffected by the arguments developed in the paper. I hope that the rephrasing of the different issues is more satisfactory.
3. This comment concerns the excessive dependence upon Tailleux (2009) and Fononoff’s papers. In order to address this comment, I significantly expanded the material in order to be more self-contained. I hope that the result is more satisfactory.

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4. I have now added a comment that makes it clear that the energy budgets considered apply to a closed domain, and should not be regarded as local balances. I agree that the sign of $d/dz[\alpha P/(\rho C_p)]$ appears to change sign throughout the domain in some instances, which is a complicating factor. I have added material that clarifies this issue, by looking at the link between the parameter ξ and two different measures of the vertical variations of Υ . I believe that in the future, I should re-examine the problem by trying to look at cases for which $d\Upsilon/dz$ does not vary too much over the domain considered, in order to get better correlations between ξ and $d\Upsilon/dz$. I hope to do that in the near future. The quantity Υ_r is the value that $\Upsilon(x)$ would have if displaced adiabatically to its reference position.
5. I have addressed this comment by adding the Table 1, as well as Figure 3.
6. I have addressed this question by computing averages for the ratio $W_{r,turbulent}/D(APE)$, and by labelling the different stratifications.

Responses to minor comments

1. In principle, one could also speak of turbulent mixing as applying to the viscous mixing of momentum, which I hope to address in a near future. The terminology turbulent diffusive mixing is used as a preparation to such a future use.
2. Point taken
3. There seems to be no choice but for the downward transport of heat to balance high-latitude cooling in a steady state. I have tried to clarify this point by introducing the advective/diffusive balance considered by Munk (1966) and Munk and Wunsch (1998).
4. I agree. This was rephrased.

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5. Done.
6. Done.
7. Changed to prompting much debate.
8. Done.
9. In the class I thank the referees for their constructive remarks.

0.0.1 Response to major comments

- (a) This comment suggests to provide more physical insights into the links between $D(APE)$ and $W_{r,turbulent}$, in order to clarify why these two quantities become equal in the context of the L-Boussinesq model, which constitutes the main framework in which these two quantities have been discussed. To that end, I have emphasised the fact that both quantities depend on molecular diffusion, as well as on the gradient of the reference vertical position $z_r = z_r(x)$ of the isopycnal surfaces. I also show that $W_{r,turbulent}$ appears as a leading order term in a series expansion in P of $D(APE)$ around $T = T_r$, the correction terms vanishing in the limit of a linear equation of state. Figure 1 now illustrates the overall energy cycle for freely decaying turbulence, and compares the “traditional” interpretation of Winters et al. (1995) versus the new one proposed by Tailleux (JFM, 2009).
- (b) This comment argues against promoting one particular definition of mixing efficiency (that based on $D(APE)$) against that based on $W_{r,turbulent}$ on the basis that mixing efficiency is often determined by measuring net changes in GPE_r over a turbulent mixing event. My opinion, however, is that the possibility of measuring the mixing efficiency from measuring the net changes in GPE_r , accounting for the factor ξ , is quite distinct from the issue of how

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the concept of mixing efficiency should be defined. In fact, providing a clear physical basis for the concept of mixing efficiency seems to be important for ensuring a sound physical basis for future empirical investigations. I find it important, therefore, to clarify the physical arguments in favour of each definition, and to take a stand on which definition I consider to be physically more sound, making sure in the process to separate the issue of defining mixing efficiency from that of measuring the latter from measuring the net changes in GPE_r , whose validity is unaffected by the arguments developed in the paper. I hope that the rephrasing of the different issues is more satisfactory.

- (c) This comment concerns the excessive dependence upon Tailleux (2009) and Fofonoff's papers. In order to address this comment, I significantly expanded the material in order to be more self-contained. I hope that the result is more satisfactory.
- (d) I have now added a comment that makes it clear that the energy budgets considered apply to a closed domain, and should not be regarded as local balances. I agree that the sign of $d/dz[\alpha P/(\rho C_p)]$ appears to change sign throughout the domain in some instances, which is a complicating factor. I have added material that clarifies this issue, by looking at the link between the parameter ξ and two different measures of the vertical variations of Υ . I believe that in the future, I should re-examine the problem by trying to look at cases for which $d\Upsilon/dz$ does not vary too much over the domain considered, in order to get better correlations between ξ and $d\Upsilon/dz$. I hope to do that in the near future. The quantity Υ_r is the value that $\Upsilon(x)$ would have if displaced adiabatically to its reference position.
- (e) I have addressed this comment by adding the Table 1, as well as Figure 3.
- (f) I have addressed this question by computing averages for the ratio $W_{r,turbulent}/D(APE)$, and by labelling the different stratifications.

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0.0.2 Responses to minor comments

- (a) In principle, one could also speak of turbulent mixing as applying to the viscous mixing of momentum, which I hope to address in a near future. The terminology turbulent diffusive mixing is used as a preparation to such a future use.
- (b) Point taken
- (c) There seems to be no choice but for the downward transport of heat to balance high-latitude cooling in a steady state. I have tried to clarify this point by introducing the advective/diffusive balance considered by Munk (1966) and Munk and Wunsch (1998).
- (d) I agree. This was rephrased.
- (e) Done.
- (f) Done.
- (g) Changed to prompting much debate.
- (h) Done.
- (i) In the classical interpretation of the advective/diffusive model, which is at the heart of Munk (1966) and Munk and Wunsch (1998) approaches, the upward advection of cold water is balanced by the downward diffusion of heat. The rate of upwelling is set by the rate of deep water formation. The advective term, therefore, represents the effect of cooling by deep water formation. This is how the advective/diffusive balance is taught in oceanography courses I know about. This represents a more physical way of present things that saying it is balanced against the rate of upwelling, which says little about the physics.
- (j) Done.

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- (k) I am not sure I understand. A positive $W_{r,mixing}$ implies a conversion of exergy into GPE_r rather than the reverse.
- (l) I have inserted a note about that.
- (m) P_{max} replace by P_{min} . Done.
- (n) Rephrased.
- (o) I'd be surprised if cases existed such that $D(APE)$ underestimated $W_{r,turbulent}$, but I agree that until this is proven, I need to say that for the moment, this has been done only for water or seawater.

ical interpretation of the advective/diffusive model, which is at the heart of Munk (1966) and Munk and Wunsch (1998) approaches, the upward advection of cold water is balanced by the downward diffusion of heat. The rate of upwelling is set by the rate of deep water formation. The advective term, therefore, represents the effect of cooling by deep water formation. This is how the advective/diffusive balance is taught in oceanography courses I know about. This represents a more physical way of present things that saying it is balanced against the rate of upwelling, which says little about the physics.

- 10. Done.
- 11. I am not sure I understand. A positive $W_{r,mixing}$ implies a conversion of exergy into GPE_r rather than the reverse.
- 12. I have inserted a note about that.
- 13. P_{max} replace by P_{min} . Done.
- 14. Rephrased.

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- 15. I'd be surprised if cases existed such that $D(APE)$ underestimated $W_{r,turbulent}$, but I agree that until this is proven, I need to say that for the moment, this has been done only for water or seawater.

Please also note the Supplement to this comment.

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