Comments on the manuscript submitted to Ocean Science

Title: “Modelling eddy diffusivity in the ocean with scalar-velocity cospectra”
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・**Overall quality of the preprint**

This paper seeks to understand the parameter dependence of eddy diffusivity obtained from direct numerical simulations (DNS) of stratified turbulence and observations of ice–ocean boundary layer flows, based on the behavior of the scalar flux spectrum, which represents the correlation between scalar fluctuations and vertical velocity.

Figure 4 appears to present the principal result of this study. In the regime of large horizontal Froude number (Fr_h), the eddy diffusivity follows an Fr_h^{-2} scaling and agrees well with both the DNS and observational data. In contrast, for Fr_h<1, considerable variability is observed in the behavior of the eddy diffusivity obtained from observations and DNS. Broadly speaking, the data can be divided into two groups: one in which the eddy diffusivity approaches a constant value as Fr_h decreases, and another in which it gradually increases.

A notable strength of this study is that it explains the dependence of eddy diffusivity on the horizontal Froude number in the Fr_h<1 regime by relating it to the scaling behavior of the scalar flux spectrum at scales larger than the Ozmidov length. The author also discusses the possibility that differences in the behavior of the scalar flux spectrum at large scales may arise from differences in stratification strength and turbulence-forcing mechanisms.

Developing a comprehensive understanding of the scalar flux spectrum in stratified turbulence remains an important research challenge. In particular, it is still unclear whether the -7/3 power law holds in the inertial-convective range and what scaling laws govern scales larger than the Ozmidov length, where the effects of stratification become pronounced.

In this paper, following a detailed review and assessment of previous theoretical, DNS, and observational studies, the author attempts to model the spectrum using a formulation designed to maintain consistency with existing theories and data. The manuscript is very carefully written throughout, and the discussion is well developed with appropriate citations of important studies related to spectral modeling. The author also clearly describes the shortcomings and limitations of the proposed spectral model, which is commendable.

Based on the considerations above, I believe that this manuscript is worthy of publication in Ocean Science. I provide several comments below, which I ask the author to consider when revising the manuscript.

・**Scientific questions/issues**

１． The horizontal Froude number is an important dimensionless parameter in this study. In strongly stratified turbulence, the characteristics of the flow differ between the horizontal and vertical directions; therefore, it is natural to use the horizontal characteristic length scale, l_h, as the representative length scale. However, how exactly is l_h defined in this study? The author should provide its explicit definition in the manuscript.

２． At the end of Section 2.2, the author notes that a spectrum obtained by summing over spherical shells does not necessarily imply that the underlying field is isotropic. In the presence of strong anisotropy, it is the reviewer's understanding that this shell-summed spectrum corresponds to the isotropic component obtained by applying an SO(3) decomposition to the three-dimensional spectrum, E_{w¥theta}(k_x, k_y, k_z). For further details, please refer to the discussion on p. 118 of Gotoh and Yeung (2012). Although that reference considers the decomposition of structure functions, an analogous argument can be made for spectra.

３． Section 3.3 states that the friction velocity at the ice–ocean interface is used as the characteristic velocity scale when evaluating ¥Gamma for the IOBL. For consistency with the definition of ¥Gamma in the other cases, it would seem more natural to use a bulk velocity scale. Please explain why the friction velocity was adopted for the IOBL evaluation.

４． In constructing the scalar flux spectrum model, the author smooths the transitions between the different scaling ranges. Although the motivation for this approach is understandable, smoothing is not an essential aspect of spectral modeling and does not appear to be particularly important in the present study. Indeed, Figure 2(c) shows that smoothing slightly changes the magnitude of the model spectrum; however, given the scatter and uncertainty in the DNS and observational data used for comparison, its effect appears to be limited. Therefore, I do not consider a discussion of the sensitivity to the smoothing parameter to be necessary.

On the other hand, it would be more useful to other researchers if the model spectrum could be expressed as an analytical function. For example, models have been proposed for the turbulent kinetic energy spectrum in which the entire wavenumber range, from the integral scale to the dissipation scale, is represented by analytical functions (see, for example, Section 6.5.3 of S. B. Pope, "Turbulent Flows", Cambridge University Press). Providing an analytical expression for the model spectrum would have the advantage of allowing other researchers to use the model more easily.

I do not consider it essential to include such an analytical expression in the present manuscript. Nevertheless, I encourage the author to consider this possibility as a useful direction for future development of the model.

・**Technical corrections**

5． Figure 4 presents a large number of DNS and observational results in a single plot. However, because many of the data points overlap, it is difficult to distinguish the individual datasets. The readability of the figure should be improved, for example, by separating the data into panels according to appropriate categories, such as DNS versus observations or different flow types, and/or by reconsidering the combinations of symbols and colors.

6． Is ¥bar{K}_{¥theta}, which appears immediately before Eq. (14), a typographical error for ¥bar{K}_{¥rho}? Please check.

7． The legends for the eddy-diffusivity model curves shown in Figures 4(b) and 5 include abbreviations that appear to identify the reference datasets, together with parameter values (e.g., MBL16, s_b=1/3). Does this mean that the parameters required to construct the model spectrum, including s_b, were determined by reference to the corresponding DNS or observational dataset? If so, explicitly stating this in the main text or the figure captions would help readers understand the modeling procedure.

8． Section 4.4, third paragraph: Is "Fig. 6C" a typographical error for "Fig. 6B"? Please check.