Review of

Spatial distribution of turbulent mixing in the upper ocean of the South China Sea

by

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submitted to

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General comments

This is an interesting manuscript presenting a new data set observed in the South China Sea (SCS) by means of micro-structure shear (MSS) profilers. In four different geographical regions of the SCS, a total of 82 MSS profiles were obtained, covering the first 500m of the water column. Analyses mainly in terms of the dissipation rate and the diapycnal diffusivity are performed and discussed in comparison to the buoyancy and shear frequencies squared and the gradient Richardson number. Comparison of the results for the dissipation rate to the dissipation rate parameterisations by Gregg (1989) and to the parameterisation by MacKinnon and Gregg (2005) is performed. In most situations, the latter model better represents the data. To explain this, internal wave spectra are derived from observations of 5 moorings in the SCS. Considerable deviations from the Garrett-Munk spectra, on which the Gregg (1989) model is based, explain the weakness of this model.

Having said this, the manuscript is generally publishable in Ocean Science. However, I have some concerns which need to be considered by the authors before acceptance can be recommended. Major revisions are required.

One concern is the lack of physical interpretation of the results. The authors should explain why certain areas show large or small shear and stratification, respectively. The role of high-amplitude internal waves entering from the Luzon Strait and their effect on mixing in the SCS needs to be discussed.

The study needs a better motivation. Which is the major knowledge gap to be filled? This should come out as a result from the introduction. In the moment it reads a bit like a report to present new data for the first time.

The shear estimated from 16m-bins is very coarsely resolved. Therefore, the gradient Richardson number calculated on that shear might be substantially underestimated. This needs to be discussed in more depth (not only in section 3.3).

Specific comments

Line 24: wrong unit (should be m2/s)

Lines 37/38: "large numbers of … tides": better expression needed

Lines 53/54: not clear why these parameterisations are important for ocean models. Please explain, how those could be used, since I am not aware of an ocean model using these parameterisations. See also line 310, where something similar is postulated.

Lines 54-56: here, a better motivation is needed.

Line 58: what does "LT" stand for?

Line 81: What is the detection limit for the TurboMAP profiler. You measure here very low dissipation rates of 10^{-10} W/kg. Are they still above the limit?

Line 141: here the gradient Richardson number is defined. How is it calculated? Already here, and not as late as in section 3.3, you should discuss the consequences of a very low resolution shear estimate. Do also refer to the literature, how others cope with such low resolution of the shear when calculating Ri. I assume that at many other locations in your observations Ri<1/4 should occur (otherwise the disspation rate would be lower), but you do not resolved it.

Line 151: So, why is there strong shear and weak stratification in region 1 and vice versa in the other regions. Is it external and/or internal tides which are different across the SCS? Is it different wind regimes? In general, we need more physical oceanography here.

Line 168: This overturn should have gone together with locally increased shear which is not resolved in the observations.

Line 176. There is some confusion about the background value for eddy diffusivity in the ocean. In line 25, it is 10^{-5}, in line 170, it is 5×10^{-6}, and here it is of the order of 10^{-6}. These are considerably different values. Please clarify.

Line 181: What is the physical meaning of depth and time averaged eddy diffusivity? Eddy diffusivity is a ratio (between flux and gradient), and the average of a ratio does not much sense to me. What is the additional information it gives in addition to the averaged dissipation rate (which makes sense)?

Line 196: Does also the tidal phase at which the observations where taken matter? If not, why not? What about the wind forcing? Does it vary, does it matter? See Burchard & Rippeth (2009), where wind-induced shear across the thermocline matters.

Burchard, H., and T.P. Rippeth, 2009: Generation of bulk shear spikes in shallow stratified tidal seas, *J. Phys. Oceanogr.*, 39, 969-985.

Fig. 6b: wrong unit for eddy diffusivity.

Equation after line 222: Something is wrong with the dimensions here. When f is 1/s, then 1.8 X 10^{-6} should be m2/s2. Replace 1.8 X 10^{-6} with a variable name and explain amount and unit in the text. Also, some of the brackets seem to denote an argument for a function $\cosh^{-1}$ and some denote a factor. Please clarify.

Line 222: Express cph also in Si units (1/s). Sometime cph and sometimes cpd is used, which I find confusing.

The two parametersations GH and MG should be explained for their physical reasoning. They are for different environments, deep ocean (GH) and shelf sea (MG), as I understand.

Lines 234/235: Are these data also for the thermocline region, or is it over the entire water column except for boundary layers?

Line 237: Here, a method for calculating Ri is explained. Is it different than before? Give this explanation at the first occurrence of Ri.

Fig. 7: Add locations (regions) to the plot.

Line 276: What is "fared"?

Line 298: These different techniques and seasons should be discussed with respect to their effect on observed dissipation rates.

Line 314: I had to look up the word eikonal. And it would be good, if the authors could briefly explain the eikonal model.

Line 327: typo "flied".

Line 330: Add "tidal" in front of "frequencies".

Line 332:What are the D3, D4 and D5 frequencies?

Line 335/336: Sentence is a repetition of what has been written further up.

Line 340/341: How can a model for bulk averages be used for constructing profiles (such as in fig. 5)?

Line 345: word missing after "observed".