

# ***Interactive comment on* “The Influence of Turbulent Mixing on the Subsurface Chlorophyll Maximum Layer in the Northern South China Sea” by Chenjing Shang et al.**

**Ilker Fer (Editor)**

ilker.fer@uib.no

Received and published: 29 July 2020

Dear Chang-Rong Liang,

Thank you for your submission to the Ocean Science Discussion. While the observations you report can be of interest, I find the analysis short of qualifying a scientific article. The manuscript is highly descriptive, and no detailed analysis or convincing discussion is presented. The material would be more appropriate for a (very good) report or a data description paper. Following my comment and the comments from two reviewers, please post an authors' response that outlines how you intend to improve the study (e.g., which additional analysis and revisions you intend to do). At this stage

I do not encourage you to prepare a full revised version for submission to Ocean Science, but to summarize how you intend to proceed. Please allow me to decide based on your response.

The observations from two sections (one section with 6 stations and a second section with 9 stations) are presented as distance-depth distributions, like a data report: one figure for hydrography, one figure for the turbulence measurements and one figure for the water samples (Chl-a, nutrients and phosphate concentration). The next figure combines the diffusivity with concentration gradient to obtain the turbulent fluxes. The final figure implies curl-driven upwelling can be important, but it is introduced in passing and not a convincing analysis is presented. Overall, one section is turbulent and the other is not turbulent. Narrative repeatedly compares the two sections in various parameters.

Some minor comments: The observation period is given as approximately 1 month from late April to late May; however, which days the sections were collected are not stated. If the sections are separated by several weeks, the role of temporal variability could be discussed.

Descriptive occurrence of internal wave packets from satellite images (Zhao et al 2004) are from a different year. More convincing case could be made presenting actual observations from the cruise period.

Please make sure to use same units, for example nitrate fluxes are given in  $\text{mmol N m}^{-2} \text{s}^{-1}$  in line 43 and  $\text{micromole m}^{-2} \text{s}^{-1}$  in line 46.

Throughout, is it correct to refer to CTD as hydrological data?

What acoustic frequency is the ADCP?

Li 110, fine scale shear variance is introduced right after the microstructure shear variance. This can be confusing for the reader.

Please use standard notation for potential temperature and potential density anomaly.

[Printer-friendly version](#)[Discussion paper](#)

Please do not use psu for salinity unit. Indicated that it is practical salinity, given on practical salinity scale (no units).

Li 129-130: salinity layers are identified in brackets in density ranges. This is confusing. At least clarify that the values are density.

Ri at 16-m vertical scale does not resolve the turbulence processes. It can be removed all together. Citing that previous literature reported Ri from 2m to 16 m vertical scale does not help (li 115).

In Fig 8, are the two separate parts around section A and B. I assume these are 3-day averages for days corresponding to the section time periods (i.e. you created a mosaic from two images). If so, please clarify in the caption and in the text.

---

Interactive comment on Ocean Sci. Discuss., <https://doi.org/10.5194/os-2020-26>, 2020.

Printer-friendly version

Discussion paper

