



EGUsphere, referee comment RC1
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Comment on egusphere-2022-439

Anonymous Referee #1

Referee comment on "Spatial-temporal characteristics of the oceanic bottom mixed layer in the South China Sea" by Wenhui Liu et al., EGU sphere,
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GENERAL COMMENTS

This manuscript examines spatio-temporal variations of the BML thickness in the SCS by using accumulated full-depth CTD and two mooring data. It is pointed out that thick/unsteady BML tends to be observed in the highly energetic northern continental slope area while thin/steady BML in the moderately energetic deep-sea area. Such BML variations are attributed to the strength of tidal dissipation purely on the basis of the statistical analysis without being discussed in the light of the most relevant bottom Ekman layer dynamics, which is not acceptable to me. Intuitively, observed thick BML in the highly turbulent area is not surprising, given that the classical Ekman layer theory (albeit in a simplified condition) yields the Ekman layer thickness in terms of the turbulent eddy viscosity. However, it remains to be seen whether existing bottom Ekman layer theories can quantitatively capture observed variations of the BML thickness in the SCS, which, in my opinion, should be rather examined in this study. There are several complications inherent to the bottom Ekman layer dynamics, such as effects of bottom slope and non-constant eddy viscosity (e.g., Garrett et al. 1993; Muller and Garrett 2004), which may affect the observed results. In addition, observed temporal BML variations seems interesting, whose physical interpretation should be explored much more.

Overall, I cannot support the publication of this manuscript because of the complete lack of discussion of the most relevant bottom Ekman layer dynamics.

*Reference

Garrett, C., P. MacCready, and P. Rhines, 1993: Boundary mixing and arrested Ekman layers: Rotating, stratified flow near a sloping boundary. *Ann. Rev. Fluid Mech.*, 25, 291-323.

Muller, P., and C. Garrett, 2004: Near-boundary processes and their parameterization. *Oceanography*, 17, 107–116.

SPECIFIC COMMENTS

Section 1, Introduction: are there any previous papers discussing BML variations in the SCS or other regions?

L85: "less than" should be replaced with "more than"?

L99: "where" should be replaced with "in the latter of which"?

L106-108: why high frequency signals are removed? I think BML is modulated with tidal periods.

Section 2.3, The other dataset: the contents of the dissipation dataset should be described in more detail. What is the depth range of the two-dimensional dissipation map used in this study? What are "four dissipative processes" (L114-115)? What are "five process contributions" (L116)?

L124-125: I wonder if the relative variance method has systematic bias against the length of each CTD full-depth profile. Or bottommost partial profile data with some fixed length were used to avoid such bias?

L132: "higher" should be replaced with "highest" among the three H_{BML} estimates?

L168-170: Why interesting? I wonder if the fitted curve equation (2) can be well approximated to $R_H/D \sim 100/D$, just indicating the BML thickness doesn't vary so much with the total depth.

L187-L193: there are no descriptions of temporal variations of BML, especially unsteady features of thick MBL at M1, which should be pointed out here.

L201-203: It is not clear to me how the maximum gradient of the first EOF mode is related to the quasi-homogeneous structure. I cannot see the clear merits of conducting the EOF analysis here.

Section 4, The potential formation mechanisms of the BML in the SCS: as described in my general comments, there are no discussions concerning the most relevant bottom Ekman layer dynamics.

L254: "Huang et al. (2019), which" should be replaced with "Huang et al. (2019) that"?

Figures 9c and 11: the best fit curve is empirical and doesn't necessarily have the wide applicability. Moreover, it should be noted that internal wave breaking occurs in the stratified part of the ocean above the BML, not in the BML. The obtained relationship between the BML thickness and internal tide dissipation should be regarded as secondary. Tidal flow itself and its frictional damping should play a primary role in the BML. Therefore, I think the authors' discussion is inappropriate.