

Reply to reviews of “Spatial-temporal characteristics of the oceanic bottom mixed layer in the South China Sea”

Reply to reviewer #2:

We would like to thank you for your careful reading, helpful comments, and constructive suggestions, which have significantly improved the manuscript.

Major comments

- 1) Although the relative variance method is applied to potential temperature, salinity, and potential density measured with the CTD, the EOF analysis is applied only to potential temperature measured at the mooring sites. I understand that there are no salinity measurements at Station M1, but at least the salinity and potential density data at Station M2 should be analyzed to make sure that there exists no significant difference among the H_{BML} estimated from potential temperature, salinity, and potential density.

Response: Thanks for your suggestion. Considering the EOF analysis maybe does not seem that significant to the main conclusions. Thus, we have deleted the EOF analysis contents in the revised manuscript. The explanations are also given in Comments 2 and 3.

- 2) The depths of sensors listed in Table 1 are much coarser than those of the circles in Figure 7. The authors should describe how to calculate the EOF modes more in detail.

Response: In the original manuscript, we use the linear interpolation method to interpolate the raw mooring data (seven sensors in each mooring) into 10 m intervals, and then conduct the Empirical Orthogonal Function (EOF) analysis. We consider the linear interpolation method may induce some spurious information, we have deleted the EOF analysis in the revised manuscript.

- 3) Figure 7a shows that stratification around 115 m is reduced (positive) when stratification in the BML (below 100 m) is enhanced (negative), and vice versa. By contrast, Figure 7b shows that stratification around 50 m is reduced (positive) when stratification in the BML is reduced (positive) as well. I feel the latter is physically reasonable, but the former is not. The authors

should explain that the former result is not inconsistent with the quasi-homogeneous structure shown in Figure 6.

Response: In the original manuscript, the inflection points around 115 m (Figure 7a) and 50 m (Figure 7b) can interpret as the maximum gradient. Considering the coarser resolution of mooring observations in vertical direction. We agree that the vertical structure of the first EOF modes may not be real and think that it could possibly result from the vertical interpolation. We have thus deleted the the EOF analysis in the revised manuscript.

4) The total energy dissipation is not suitable for examining the influence of internal tide dissipation on H_{BML} (Figures 9c and 10) because the low-mode dissipation attributed to wave-wave interaction occurs in the stratified water column “over” the BML. Instead, the sum of the “low_modes_shoaling”, “low_modes_critical_slopes”, “low_modes_scattering”, and “high_modes_local” dissipation, all of which are included in the dataset provided by de Lavergne et al. (2019), should be used. The influence of external (barotropic) tide dissipation, which is not considered in the present study at all, should also be examined. In fact, Zu et al. (2008, Deep-Sea Res. Part I) demonstrated that the barotropic tidal energy is dissipated around the Luzon and Taiwan straits.

Response: According to the Reviewer’ s suggestion, the wave-wave interaction dissipation has been excluded from the total energy dissipation, and we have recalculated the relations and modified the figures and corresponding parts in the revised manuscript.

It is a good suggestion to examine the influence of barotropic tidal energy on the distribution of the H_{BML} , which needs the energy dissipation near the near-bottom regions in the SCS can be well simulated. The internal tide energy sinks datasets used in this study include the barotropic and baroclinic tidal energy, but the depth-integrated internal wave energy dissipation doesn’t contain much information in the vertical direction. Further studies are needed to address this issue using a more complete and high-resolution ocean model. We will leave this for our further study.

5) Eq. (3) is valid only when the vertical eddy diffusivity, A_z , is constant. In the present case that A_z varies in the vertical direction, the right-hand side of Eq. (3) must be $d(A_v(dT/dz))/dz$. The relevant parts of the manuscript should be revised as well.

Response: We are very grateful for your comments. Based on the mooring observations, we estimated three terms of $\partial T/\partial t$, $\partial T/\partial z$, and $\partial^2 T/\partial z^2$ in Eq. (3), then the unknown values of w and A_z can be estimated from a set of linear equations using the least-square fitting method. So the calculated w and A_z are depth-averaged values. Considering the Reviewer's concern, we have added the explanations that the vertical eddy diffusivity, A_z , is constant at the vertical observation range of two moorings.

Minor comments

6) Line 85: "less than 50 m" should read "more than 50 m".

Response: Thank you for pointing out this error in the manuscript, we have corrected.

7) Lines 191-192, Figure 6: This is not a fair comparison because the contour interval of Figure 6a(6b) is ten times larger than that of Figure 6c(6d).

Response: The ranges of potential temperature at M1 and M2 were about 2.2~2.4°C and 2.06~2.08°C, respectively. The variations of the potential temperature difference at M1 was 10 times greater than M2. For a fair comparison, we choose a normalized value, 25% variations of the potential temperature gradients, as the index for comparison. This value is $-4 \times 10^{-4} \text{ }^\circ\text{C m}^{-1}$ at M1 and $-0.25 \times 10^{-4} \text{ }^\circ\text{C m}^{-1}$ at M2.

8) Lines 106-108, Figure 8: The power spectra shown in Figure 8 cannot be calculated from the filtered data where the inertial, diurnal, and semidiurnal signals are removed. To avoid misunderstanding, the authors should mention that the power spectra are calculated from the raw data.

Response: According to the Reviewer's suggestion. We have added the contents to mention that the power spectra are calculated from calculated from the hourly mean data.

9) Lines 212-214, Figure 8: For the benefit of a reader, the heights of the data used for spectral analysis should be denoted.

Response: We have added the depth information in the figure.

10) Figure 2: The title of the y-axis is missing.

Response: Thank you so much for your careful check. We have added the title of the y-axis in Figure 2 in the revised manuscript.

11) Figure 6: For the benefit of a reader, the depths of sensors should be superposed.

Response: We have marked the depths of sensors with “☆” in Figure 6.