Our responses to Reviewer 2:

Note: The reviewer's original comments are in black, and our responses are in blue.

The authors conducted an extensive field campaign to survey turbulence in the South China Sea and reported a spatial pattern of turbulent intensity from the observed data. They also compared the observed data against two theoretical models. The results are well known. I found no new information. I appreciate that the amount of work involved in the data collection, but as far as the science concerns the present manuscript reads like a data report. I found no new scientific finding and no new facts other than the survey was conducted in the South China Sea. Unless Ocean Science accept a manuscript aimed at a data report, I would not recommend this manuscript for an official scientific paper. For their revision purpose I will comment on this manuscript as followed:

Responses:

We thank the reviewer for the comment. In this paper, the turbulent mixing in the South China Sea (SCS) is analyzed from two aspects, not just a simple data report. Firstly we explore the mixing features and mixing regimes in the SCS in great detail. Secondly we assess two parameterizations with the microstructure data.

Many microstructure measurements have been conducted in the SCS. There is no doubt that these measurements have greatly aided our knowledge of turbulent mixing in the SCS. However, the microstructure measurements are localized and scattered with most of them focusing on the northern SCS. The mixing features and mixing regimes in different regions of the SCS are still not fully understood. With the microstructure data in 2010, we present the spatial distribution of turbulent mixing in the upper ocean of the SCS and explore the mixing features and mixing regimes in different regions of the SCS. In the revised text, we strengthened the discussion on energy sources for the turbulent mixing (lines 246-275 and 422-431). Our observation indicated that strong turbulent mixing mainly occurred in west of the Luzon Strait where there are strong shear and weak stratification, and internal waves made a dominant contribution to the elevated turbulent mixing in west of the Luzon Strait.

Another work in this paper is the assessment of two parameterizations (GH and MG models). Though many microstructure measurements have been conducted in the SCS, none of the two models has been assessed against the dissipation in the SCS. It remains unknown which parameterization can successfully reproduce the dissipation in the SCS and why. In manuscript, we assess the two parameterizations with the dissipation data of the SCS, which would provide useful tools for ocean researchers. In fact, the microstructure measurements in the ocean are much fewer and more difficult than fine-structure measurements (i.e., CTD and ADCP measurements) in the ocean, especially in the deep sea. Thus to understand the spatial distribution and seasonal variation of the turbulent mixing in the ocean, researchers often turn to the parameterizations (Wu et al. 2011; Jing and Wu 2010). The assessment of parameterizations can also provide reference for modelers. Models often need to calibrate a background mixing level to correctly predict the physical phenomenon (Rippeth 2005). The requirement of calibration reduces the success of models on large scales since differing forcing mechanisms and mixing processes require specific methods and levels of tuning. Before the physical phenomenon can be modeled realistically, the distribution of mixing must be established and the major mixing processes parameterized. Parameterizations can provide a reference of the background mixing for the modelers.

According to reviewers' comments, we have strengthened the content of motivation (lines 52-74) and added discussion on the effect of surface winds and internal waves on turbulent mixing and shear (lines 246-275) in the revised text.

1. Material and methods should be explained in more detail. Also those mooring data outside the observation period should be removed from the text. The Luzon Strait is not well known to the audience. Indicate where LS is.

Responses:

We thank the reviewer for this suggestion. Mooring data were used to show the feature of wave field in the SCS. We think that it should be kept in the text. We have strengthened the description of the material and methods in the revised text (lines 76-123). The location of Luzon Strait has been indicated in Fig .1.

2. P.4 line83: "caused by instrument vibrations" If so, you can verify the vibration with accelerometer data. Those are mostly electronic noise.

Responses:

We apologize for this confusion due to our inaccurate statement in the original text. They are vibration noise caused by the strumming of the suspension wires in the flow (Wolk et al. 2002). We have clarified in the revised text, see lines 108-109.

3. You have to focus the science. What is deriving high turbulence in Region 1. Most likely internal tides are playing a major role in generation of turbulence.

Responses:

We thank the reviewer for this good question. There are a large amount of internal waves (tides) in the SCS, which has been reported in many literatures, such as Niwa and Hibiya (2004), Zhao et al. (2004), Klymak et al. (2006), Jan et al. (2007). Most of internal waves originate in the Luzon Strait and propagate northwestwards through the deep water zone near Luzon Strait to the continental shelf (Alford et al. 2015). Mooring data (Lien et al. 2014) indicate that internal waves would induce strong shear. A comparison of the spatial distributions of turbulent mixing, winds, and internal waves suggests that the elevated turbulent mixing and shear in west of the Luzon Strait (region 1) does not result from the effect of surface winds. The internal waves are expected to make a dominant contribution to elevated turbulent mixing and shear in west of Luzon Strait. We have strengthened the discussion on this in the revised text, see lines 246-275 and 422-431.

4. Discussion should be separated from Summary. The summary should summarize both results and a punch line of the discussion.

Responses:

We thank the reviewer for this good suggestion. We have separated the discussion from summary in the revised text.

Correction

We are very sorry for that there is a mistake in the magnitudes of dissipation spectra in Fig.2. We have corrected them in the revised text.

Our responses to Reviewer 1:
Note: The reviewer's original comments are in black, and our responses are in blue.

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 parameterizations by Gregg (1989) and to the parameterization 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.

Responses:

Thank you for your good advice. There are a large amount of internal waves (tides) in the South China Sea, which has been reported in many literatures, such as Niwa and Hibiya (2004), Zhao et al. (2004), Klymak et al. (2006), Jan et al. (2007). Most of internal waves originate in Luzon Strait and propagate northwestwards through the deep water zone near Luzon Strait to the continental shelf (Alford et al. 2015). Mooring data (Lien et al. 2014) indicate that these internal waves would induce strong shear.

A comparison of the spatial distributions of turbulent mixing and internal waves indicated that the internal waves are expected to make a dominant contribution to elevate the turbulent mixing in west of the Luzon Strait. For reasons why strong shear and elevated dissipation occurred in west of the Luzon Strait, we have strengthened the discussion in the revised text, please see more detail in lines 246-275 and 422-431.

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.

Responses:

We thank the reviewer for this good suggestion. There are two motivations for this study: firstly exploring the mixing features and mixing regimes in different regions of the SCS and secondly assessing two parameterizations with the microstructure data.

Many microstructure measurements have been conducted in the SCS. There is no doubt that these measurements have greatly aided our knowledge of turbulent mixing in the SCS. However, the microstructure measurements are localized and scattered with most of them focusing on the northern SCS. The mixing features and mixing regimes in different regions of the SCS are still not fully understood. With the microstructure data in 2010, we present the spatial distribution of turbulent mixing in the upper ocean of the SCS and explore the mixing features and mixing regimes in different regions of the SCS in great detail. In the revised text, we strengthened the discussion on energy sources for the turbulent mixing (lines 246-275 and 422-431). Our observation indicated that strong turbulent mixing mainly occurred in west of the Luzon Strait where there are strong shear and weak stratification, and internal waves made a dominant contribution to the elevated turbulent mixing in west of the Luzon Strait.

Another motivation for this paper is the assessment of two parameterizations (GH and MG models). Though many microstructure measurements have been conducted in the SCS, none of the two models has been assessed against the dissipation in the SCS. It remains unknown which parameterization can successfully reproduce the dissipation in the SCS and why. In manuscript, we assess the two parameterizations with the dissipation data of the SCS, which would provide useful tools for ocean researchers. In fact, the microstructure measurements in the ocean are much fewer and more difficult than fine-structure measurements (i.e., CTD and ADCP measurements) in the ocean, especially in the deep sea. Thus to understand the spatial distribution and seasonal variation of the turbulent mixing in the ocean, researchers often turn to the parameterizations (Wu et al. 2011; Jing and Wu 2010). The assessment of parameterizations in the SCS would provide reference for researchers on the selection of parameterization to study the turbulent mixing in the SCS. The assessment of parameterizations can also provide reference for modelers. Sea models have success in reproducing the water column structure in seasonally stratified shelf seas (Holt and Umlauf 2008; Simpson and Bowers 1981; Simpson and Hunter 1974). However, models need to calibrate a background mixing level for the correctly prediction (Rippeth 2005). The requirement of calibration reduces the success of models on shelf-wide scales. Before the water column structure in shelf seas can be modeled realistically, the distribution of mixing must be established and the major mixing processes identified and parameterized. Parameterizations would provide a reference of the turbulence mixing for the modelers.

We have added the related content in the revised text as the reviewer suggested. Please see lines 52-74 in the revised text.

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).

Responses:

We understand the reviewer's concern. There is no doubt that the resolution of shear might affect the values of Richardson number. Unfortunately, we have only 16 m shear data, which prevents us from discussing the influence of shear resolution on the Richardson number. In our text, Richardson number was estimated following MacKinnon and Gregg (2005), see their Fig. 5. In the previous literatures, different shear resolutions were used to calculate the Richardson number, which range from 2 m to 16 m (MacKinnon and Gregg 2003; 2005; van der Lee and Umlauf 2011; Xie et al. 2013; Yang et al. 2014). High resolution of shear (2-4 m) was often used on the shelf area to catch the small scale internal waves. Low resolution of shear (8-16 m) was often used in deep water due to the large depth of the water column. Our observations mainly located in deep water, so the ADCP vertical resolution was set in 16 m to cover more depth. Although the resolution of 16 m might miss some overturning in our observation, it does not affect the comparison of Richardson number in different regions too much. We have strengthened the discussion on this in the revised text, see lines 177-183.

Specific comments

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

Responses:

Thanks for reminding. We have corrected the mistake, see line 26 in the revised text.

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

Responses:

We have changed "large numbers of … tides" into "numerous … tides", see line 40 in the revised text.

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.

Responses:

We apologize for this confusion due to our inaccurate statement in the original text. Parameterization is important mainly due to its reference to modelers. Shelf sea models have success in reproducing the water column structure in seasonally stratified shelf seas (Holt and Umlauf 2008; Simpson and Bowers 1981; Simpson and Hunter 1974). However, models need to calibrate a background mixing level for the correctly prediction (Rippeth 2005). The requirement of calibration reduces the success of models on shelf-wide scales. Before the water column structure in shelf seas can be modeled realistically, the distribution of mixing must be established and the major mixing processes identified and parameterized. Parameterizations would provide a reference of the turbulence mixing for the modelers. Of course, we hope that these parameterizations can be applied to model in the near future.

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

Responses:

We have strengthened this in the revised text, see lines 52-74.

Line 58: what does “LT” stand for?

Responses:

We apologize for this confusing abbreviation. “LT” stands for “local time’’. We have changed “LT” into “local time’’ in the revised text, see lines 76-77.

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?

Responses:

The noise level of the TurboMAP profiler is $\varepsilon \sim 10^{-10}$ W/kg (Matsuno and Wolk 2005; Wolk et al. 2002). TurboMAP profiler resolves dissipation rates as low as 5×10^{-10} W/kg. Lower values of dissipation rates can be inferred by comparing the measured spectra against the assumed universal form. We have added this in the revised text, see line 117.

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.

Responses:

Richardson number was estimated following MacKinnon and Gregg (2005), see their Fig. 5. Firstly calculate 16-m shear and 2-m buoyancy frequency, then interpolate the 2-m buoyancy frequency to the 16-m shear grids, and then calculate the Richardson number with the shear and buoyancy frequency. In the previous literatures, different shear resolutions were used to in the calculation of the Richardson number, which range from 2 m to 16 m (MacKinnon and Gregg 2003; 2005; van der Lee and Umlauf 2011; Xie et al. 2013; Yang et al. 2014). High resolution of shear was often chosen (2-4 m) on the shelf area to resolve the small scale internal waves. Low resolution of shear was often chosen (8-16 m) in deep water due to the large depth of the water column. Our observations mainly located in deep water, so the ADCP vertical resolution was set in 16 m to cover more depth.

We agree with the reviewer that 16-m resolution might miss some overturning in our observation. Unfortunately, we have only 16-m shear data, which prevents us from discussing the influence of shear resolution on the Richardson number. In spite of this, it does not affect the comparison of Richardson number in different regions too much since the same shear resolution was used in the SCS. We have strengthened the discussion on this in the revised text, see lines 177-183.

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.

Responses:

We thank the reviewer for this good question. There are a large amount of internal waves (tides) in the South China Sea, which has been reported in many literatures, such as Niwa and Hibiya (2004), Zhao et al. (2004), Klymak et al. (2006), Jan et al. (2007). Most of internal waves originate in Luzon Strait and propagate northwestwards through the deep water zone near Luzon Strait to the continental shelf (Alford et al. 2015). Mooring data and microstructure measurements (Laurent 2008; Lien et al. 2014) indicate that internal waves would induce strong shear and produce elevated turbulence.

A comparison of the spatial distributions of turbulent mixing, winds, and internal waves indicated that elevated turbulent mixing in west of Luzon Strait (region 1) does not result from the effect of surface winds. The internal waves are expected to make a dominant contribution to elevated turbulent mixing and shear in west of the Luzon Strait. Unfortunately, we have only one profile of microstructure measurement and short time series (about one hour) of current velocity obtained by the ADCP for each station, thus it is impossible to separate the internal waves in various frequencies and explore their respective contributions to the dissipation. We have strengthened the discussion on this in the revised text, see lines 246-275.

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

Responses:

We agree with the reviewer. Small overturning might be missed in our observation due to the low shear resolution.

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.

Responses:

We apologize for this confusion. Diapycnal diffusivity from turbulent mixing in the open ocean thermocline ranges from 5×10^{-6} to 3×10^{-5} m^2/s (Gregg 1998; Polzin et al. 1995). We have clarified this in the revised text, see lines 26-27, 210, and 215-217.

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)?

Responses:

In steady state, dissipation should equal the rate of transfer from the internal waves to turbulence mixing. Column integrated dissipation $\int_{-H}^{0} \rho\varepsilon dz$ (W/m^2) represents the rate of energy dissipation per square meter, which is often used to calculate the energy of internal waves losing to dissipation. Here, in order to compare the magnitude of dissipation and diffusivity in different stations and regions, we use the averaged dissipation rate, which can remove the influence of different thermocline depths on the comparison. Averaged eddy diffusivity and dissipation rate are also used to discuss the influence of wind and internal waves on the distribution of turbulence mixing. In addition, averaged eddy diffusivity is used to compare with the values in the open ocean.

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.

Responses:

The barotropic tides extracted at location of (18°N, 114°E) were used in the discussion without considering the tidal phase. The tidal phase does not affect our discussion too much because the bias in the arrival of spring-neap tides in different locations of the SCS is small. Fig A1 shows the time series of the barotropic tidal velocity predicted from TPXO 7.1 for three locations: (21°N, 119°E), (18°N, 114°E), and (10°N, 114°E). One can see from Fig A1 that bias in the arrival of spring-neap tides between northern location (21°N, 119°E) and southern location (10°N, 114°E) was less than 3 hour. Related content has been added in the revised text, see lines 237-239.

We have added discussion on the effect of surface winds on turbulent mixing and shear in the revised text, see lines 246-265.

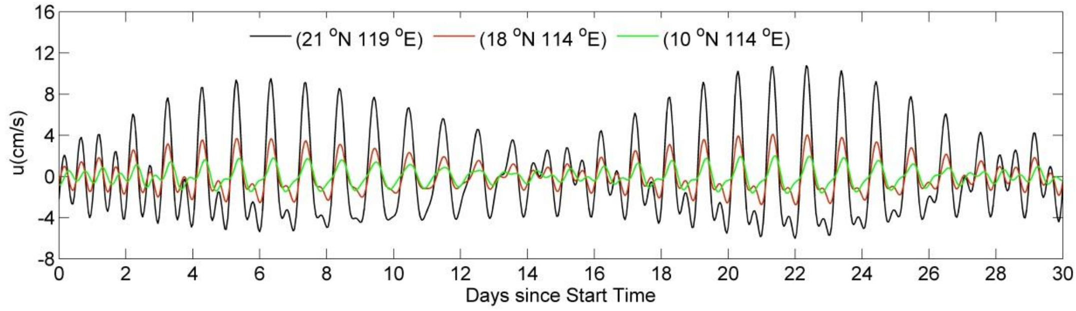

Fig. A1. Time series of the barotropic tidal velocity predicted from TPXO 7.1 for three locations: (21°N, 119°E), (18°N, 114°E), and (10°N, 114°E).

Fig. 6b: wrong unit for eddy diffusivity. Equation after line 222: Something is wrong with the dimensions here. When f is 1/s, then 1.8×10^{-6} should be m^2/s^2. Replace 1.8×10^{-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.

Responses:

Thanks for reminding. We have corrected the unit in Fig. 6. $\cosh^{-1}$ denotes inverse hyperbolic cosine function not a factor. The unit for 1.8×10^{-6} is m^2/s^2. Actually a reference Coriolis frequency f_0 has been included in 1.8×10^{-6}. We have revised the manuscript according to the reviewers.

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

Responses:

We apologize for this confusion. 1 cph=1.7×10^{-3} s^{-1} and 1 cpd=1 day^{-1}= 1.1574×10^{-5} s^{-1}. To avoid confusion, we have changed the unit "cph" into "s^{-1}" and added "(1 cpd=1 day^{-1})" in the revised text. Some of physical reasoning of the two parameterizations has been summarized in lines 358-367. For more information about the two parameterizations, one can refer to (MacKinnon and Gregg, 2003a). We are sorry for that we can't describe these two parameterizations better than them.

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

Responses:

It is 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.

Responses:

The difference between the two is that 2-m buoyancy frequency was used at the first occurrence of Ri and 16-m buoyancy frequency was used at latter. We have clarified this in the revised text, see lines 178 and 309.

Fig. 7: Add locations (regions) to the plot.
Responses:
Thanks for reminding. Locations (regions) have been added to the plot.

Line 276: What is "fared"?
Responses:
"fared" means ''show''

Line 298: These different techniques and seasons should be discussed with respect to their effect on observed dissipation rates.
Responses:
We thank the reviewer for this good suggestion. We have strengthened the discussion on this in the revised text, see lines 408-413.

Line 314: I had to look up the word eikonal. And it would be good, if the authors could briefly explain the eikonal model.
Responses:
Henyey et al. (1986) construct the analytic model by equating ε to the net flux of energy passing out of the internal wave spectrum at large wave number, which they take as $2k_3^c$, corresponding to a 5-m vertical wavelength. Because the energy flows toward both lower and higher wave numbers,

$$\varepsilon_{HWF} = \frac{1-r}{1+r}\int_f^N \Phi_{flux}(2k_3^c, \omega)\, d\omega, \qquad (1)$$

where $\Phi_{flux}(k_3, \omega)$ is the energy flux spectrum and r is the ratio of the flux passing $2k_3^c$ toward lower wave numbers to the flux going toward higher wave numbers. The flux spectrum is formulated using ray-tracing equations to describe waves having high wave numbers propagating through a field in which most of the energy resides at low wave numbers. In keeping with a ray-tracing approach, $\Phi_{flux}(k_3, \omega)$ can be expressed as the product of the GM model energy spectrum $\Phi_E(k_3, \omega)$and the rate at which the vertical wave number changes as the packet passes through the spectrum,

$$\Phi_{flux}(k_3, \omega) = \Phi_E(k_3, \omega)\left|\frac{dk_3(\omega)}{dt}\right| \text{ and} \qquad (2)$$

$$\Phi_E(k_3, \omega) = \frac{2b^3\beta_* N N_0 E_{GM}}{\pi(\beta_*+\beta)^2}\frac{f}{\omega(\omega^2-f^2)^{1/2}}, \qquad (3)$$

where $\beta_* = j_*\pi N/N_0$ and $\beta = bk_3$. Because the flux is evaluated at a wave number in the roll-off region of the energy spectrum,

$$\Phi_E(k_3, \omega) = \frac{k_3}{k_3^c}\Phi_E(k_3^c, \omega) \qquad k_3 > k_3^c, \qquad (4)$$

where $k_3^c = (3Ri_c j_* bE_{GM})^{-1}$, For large wave numbers, the ray-tracing equations give

$$\left|\frac{dk_3}{dt}\right| = |\boldsymbol{S}\cdot\boldsymbol{k}|, \qquad (5)$$

where $\boldsymbol{S}$ is the shear vector. If $\boldsymbol{S}$ and $\boldsymbol{k}$ are uncorrelated,

$$\left|\frac{dk_3}{dt}\right| = Nk_h\left[\frac{1}{2}\langle Ri^{-1}\rangle\right]^{1/2}. \qquad (6)$$

where $k_h = k_3[(\omega^2 - f^2)/(N^2 - \omega^2)]^{1/2}$. In evaluating $\langle Ri^{-1}\rangle$, they take account of the additional shear contributed by wave numbers between k_3^c and $2k_3^c$,

$$\langle Ri^{-1}\rangle = Ri_c^{-1}[1 + \ln(k_3/k_3^c)]. \quad (7)$$

Using (2)-(7) in (1) leaves the integral

$$\int_f^N \omega^{-1}(N^2 - \omega^2)^{-1/2}\, d\omega = N^{-1}cosh^{-1}(N/f)$$

Following Munk (1981) Ri_c^{-1}=0.5, $k_3/k_3^c = 2$, and r =0.4,

$$\varepsilon_{HWF} = 0.33f^{-1}[4\pi^{-1}j_* bE_{GM}f]^2 cosh^{-1}(N/f).$$

Line 327: typo "flied".

Responses:

Thanks for reminding. We have corrected the typo "flied".

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

Responses:

Thanks for reminding. We have added the missing word "tidal".

Line 332:What are the D_3, D_4 and D_5 frequencies?

Responses:

D_3, D_4 and D_5 are the higher tidal harmonic frequencies, i.e., D_3=D_1+D_2, D_4=D_2+D_2, and D_5= D_2+D_3, where D_1 and D_2 represent the diurnal and semidiurnal tidal frequencies, respectively. These higher tidal harmonic frequencies mainly result from nonlinear interaction between internal waves [van Haren, 2002; van Haren, 2003; Xie, 2010]. We have cited the work of van Haren (2003), van Haren et al. (2002), and Xie et al. (2010) in the revised text, see lines 377-379.

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

Responses:

Thanks for reminding. We have deleted repetition "The GH model is typically evaluated for the wave field with the GM spectral shape".

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

Responses:

With the calculated shear $S(z)$ and buoyancy frequency $N(z)$, profiles can be constructed from the equations of $\varepsilon_{MG}(z) = \varepsilon_0 \frac{N(z)}{N_0}\frac{S(z)}{S_0}$ and $\varepsilon_{GH} = 1.8\times10^{-6}\left[f cosh^{-1}\left(\frac{N_0}{f}\right)\right]\left[\frac{S(z)^4}{S_{GM}^4}\right]\left[\frac{N(z)^2}{N_0^2}\right]$.

Line 345: word missing after "observed".

Responses:

Thanks for reminding. We have added the missing word "dissipation".