2nd Review on “Spatial distribution of turbulent mixing in the upper ocean of the South China Sea” (os-2016-80) by X.D. Shang, C.R. Liang and G.Y Chen.

General

The authors have improved the manuscript based on the comments made by two reviewers. However, I still hold the same opinion that I found no new scientific finding and no new facts other than the survey was conducted in the South China Sea.

In fact, the authors mention that the objectives of this study are the following two points:

1) We explore the mixing features and mixing regimes in the SCS.
2) None of two models (GH and MG) has been assessed against the dissipation in the SCS.

Cleary this work is focusing on a local area, not a general scientific question. Even the work is a local study, at least a scientific question should be identified. The important features the authors found were:

1) Mixing is strong in the northern part of the SCS. The mixing is induced by internal tides.
2) MG fits the observed mixing intensity well.

As they state in Summary, there are numerous previous works regarding mixing in the SCS. This is also mentioned in Introduction. Do not repeat the same context. This part should be move to Introduction so that what have been identified and what has not been studied in the SCS. Then states, the motivation of this study focusing on the following two points:

1) Where is the strong mixing taking place in the SCS? And what derives the mixing?
2) In order to estimate the mixing without microstructure measurements, we explore the two models. And investigate which one works better and why it works better.

At a moment, Introduction ends with sort of Summary. Do not mention the conclusion in Introduction. Once the authors resort the context and slim down the whole paper without stating the same context, the manuscript should become acceptable. Next I will state specific comments.

Specific

p.14,L43-44: “almost two orders” No, at most one order!

p.15, L73: “the improved ocean model” You are not improving the model. Do not state your wish list.

p.20, L167, 171: S^2 and N^2 should be in italic. Elsewhere as well.

p.21, Fig.5 caption: “buoyancy frequency” -> “buoyancy frequency squared”

p.22,L222: z_b and z_t should be defined where they are first appeared (L159 and 160).

p.22,L224: Before Fig.6 appears, show the probability density function (pdf) of epsilon for each region estimated from non-parametric pdf estimator, such as histogram. The pdf should suggest you whether the source of mixing is a single forcing (single mode) or multiple forcing (multiple modes).

It is important to investigate the pdf when you discuss statistics, even the mean. If you look at the correlation between tidal elevation and mean dissipation rate, you might find some positive correlation. Later part, you pointed out that the source of mixing is due to internal waves, most likely internal tides. Internal tides have to generated from some forcing, usually barotropic tides. Both K1 and M2 components can propagate at this latitude, so may not be easy to identify which one is dominating the most. But maybe previous studies, such as Zhao et al. (2004) mentioned the properties of the internal tides. Also some numerical works have been done in this area, that should be useful information to state which one is dominating.

p.24,L266: “Internal wave” should be “Internal tide”.

p.24, L274: “These observations indicate” No, you are not demonstrating if it is due to internal wave (tide). Say “suggested” instead of “indicate”.

p.25,L288-289: “does not result from the effect of spring-neap tides” Are you sure they are uncorrelated? If so, what generates Internal waves (tides). Usually internal tide (baroclinic mode) is generated from surface tide (barotropic mode).

p.26, Fig.7: In addition to Fig.7, I would like to see a direct comparison of observed epsilon vs model epsilon and show R^2 (not correlation coefficient). And test the linear regression equation. You might claim that Fig 5 does it.

But this not sufficient. You have to test it. You must also discuss the error band based on the model. You promised to deliver a useful tool to modelers that can assess the mixing intensity without microstructure data. So you need to state the accuracy of the model.

p.29,Fig. 9: These diagrams are all based on averaged velocity between 60 and 270 m. Are you looking at only barotropic components? How could you be make sure you can identify both barotropic and baroclinic component separately? Since mixing (turbulence) is most likely generated from internal tides (K1 and M2), it is important to show two components separately.

p.29-30: The first paragraph should be in Discussion. This is not a summary of your work. You must clarify what have been reported from the previous studies and show what this study identified. A part of this story may be in Introduction as a motivation of this study.