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Interactive comment on "Turbulence observations in the Gulf of Trieste under moderate wind forcing and different water column stratification" *by* F. M. Falcieri et al.

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The authors would like to thanks Dr Peters review for its helpful and supportive comments and suggestions. An item-by-item reply to Dr Peters comments follows.

RC: The authors present an interesting and relevant set of observations. Their paper can eventually make a fine contribution to the literature. I have a generally favorable view of the analysis. However, while the analysis of circulation patterns and water masses are thorough and well done, I see significant problems with the analysis of the turbulent dissipation rates. In essence, although with modifications, the authors claim that "TKE dissipation rates follow closely the similarity scaling for wind stress

generated turbulence and for the one due to bottom shear stress near the sea floor (Fig. 8)." However, in a substantial number of profiles, epsilon deviates by an order of magnitude or more from similarity scaling. Specifically, epsilon appears systematically elevated over similarity values toward the surface (upper 7-12 m). Some of these instances are not addressed by the authors. The analysis is carried out solely in terms of boundary forcing with modifications due to stable stratification.

AC: We thank the reviewer for the comments, in the revised paper we will follow his suggestions, integrating the text with a more in depth analysis of the similarity scaling. We can anticipate that the differences in the surface layer can be most likely related to three major sources: 1. turbulence generated from waves. During the campaign a WASS (Wave Acquisition Stereo System) was installed on the vessel deck to collect wave data. Throughout the yoyo acquisitions the average significant wave heights were in the order of 1 m, although without much wave breaking. This would suggest that the effect of wave generated turbulence should be confined in the first few meters 2. ship oscillations. In order to collect yoyo cast the R/V had to maintain a minimum cruise in the proximity of the cast position. This resulted in stronger ship pitch and hence a more significant production of noise in the turbulence surface measurements. 3. Langmuir circulation: in the revised manuscript this aspect will be further investigated and assessed.

RC: The text is also somewhat hard to follow.

AC: We apologize for this; the text will be revised in order to make it more clear and accessible.

RC: In my view the analysis of the turbulent mixing is not very statisfactory and insufficient. I recommend a wider perspective that should include the effects of surface waves, Langmuir cells and internal wave-driven forcing. With respect to the latter, the work of MacKinnon and Gregg needs to be considered.

AC: as stated above those suggestions will be integrated in text.

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RC: Abstract, line 9: "ensembles" - not "enables". The need for ensembles is not universal. Just consider the single-cast deep ocean microstructure profiles.

AC: We tend to agree that there is not universal need for ensembles profiles, but in the case of shallow waters, and in absence of time and technical constrains, we think that it is appropriate to perform multiple casts in order to obtain more significant observations. The text was modified to more clearly state this concept.

RC: Abstract, near Line 15: Attempts at reducing vertical smearing in averages have been done many times before, typically using potential density (or Thorpe-sorted pot. density). This works well in data with vertical displacements due to internal waves. It does not work so well in the presence of lateral water mass variations. I leave it up to the authors to comment on my note or to ignore it.

AC: Thanks for the suggestion. We will integrate the text providing a wider context in section 2.2

RC: Introduction, first paragraph: While I agree with the last sentence, the remainder of the opening of the paper is pretty much off the mark. Turbulence observations have become rather common. And from the 1980s microstructure measurements have been accompanied by the necessary ancillary measurements.

AC: The paragraph will be modified accordingly.

RC: Page 1734, line 27: What is the "NIB-MBS" profiler?

AC: They are the two profilers used during operations, one owned by CNR-ISMAR and one by the Marine Station of the Slovenian National Institute of Biology (NIB-MBP). Although this was already mentioned in the text, the differentiation will be better explained in the revised version.

RC: Fig. 3: The red line i (d) is hard to see.

AC: the figure will be modified accordingly

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RC: Fig. 6: As there are only small vertical variations in T, S and turbidity, a set of "waterfall plots" might show more detail of the data. – The contour interval in epsilon obviously is 1 in log(epsilon/(W/kg), notin epsilon itself (ditto in later figures).

AC: We will superimpose to the current plots the waterfall plots so to keep the same palettes of figure 9. The axis label for epsilon contours will be modified.

RC: Fig. 8: The measured dissipation rates of Yoyo Y01 are described as "in agreement with the local forcing". Yet epsilon is systematically and strongly elevated above the wind-related similarity scaling in the upper 7-9 m. This needs an explanation or modification of the text. The elevated epsilon near the surface is even more pronounced in later Yoyos. See text above for other parts of the data set.

AC: see above

RC: Fig. 8: Why not plot the surface buoyancy flux as vertical lines?

AC: We are not sure to fully understand this comment. Actually, the surface buoyancy flux is already plotted as a vertical magenta dashed line

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