Review of MS#egusphere-2025-4762 entitled “Bottom topography effects on abyssal diapycnal mixing in the Eastern Mediterranean Sea” by Florian Kokoszka et al.

This study analyzes the variability of near-bottom shear/strain variance ratios (Rω) in the western Ionian Sea, with particular emphasis on how these ratios are influenced by local seafloor parameters. The authors find that Rω is generally low near the shelf break and the Malta Escarpment, in contrast to overall high values observed in the abyssal plain. They demonstrate that lower Rω correlates with increased topographic slope and roughness. This is reasonable, given that high-frequency internal lee waves are expected to be generated by the boundary current as it impinges on rough seafloor topography (e.g., St. Laurent et al. 2012; Waterman et al. 2013; Sheen et al. 2013). However, I was unable to find a clear interpretation of these observations in the manuscript. This study would be more strengthened if the authors could include an analysis of lee-wave generation, producing a lee-wave generation map in the Ionian Sea similar to the global map of Nikurashin and Ferrari (2011), and comparing it with the observed Rω variability.

In the main part of the analysis, the authors compare Rω with the “Δ-slope”, the difference between the Rω-derived internal-wave propagation slope (β) and the local topographic slope, which is conceptually interesting. However, I strongly suspect that the well-defined curves of Rω vs Δ-slope in Figs 3, 5, 6 arise primarily from the analytic relationship

$$\beta = \tan^{-1}\left(\frac{f}{N}\sqrt{\frac{2}{R_\omega - 1}}\right),$$

with only minimal influence from the actual topographic slope. Moreover, the above analytical relationship of β itself is valid only for internal waves with a “single frequency”, and its applicability at low Rω has been questioned by Ijichi and Hibiya (2015). Therefore, the discussion in L429-443 regarding whether the topography is near-critical (Δ-slope ~ 0) or super-critical (Δ-slope < 0) is, in my view, questionable, and I do not think it meaningfully aids the interpretation of the observed Rω variability. Overall, it is unclear to me what additional insight these Δ-slope plots provide beyond what is already shown in Fig. 2, that is, the relationship between Rω and the local topographic slope and roughness.

I also suspect that the authors’ method for calculating the local topographic slope and roughness (L393–400) may not adequately distinguish “small-scale roughness” from the underlying “large-scale slope,” particularly in regions of abrupt topography such as the shelf break and the Malta Escarpment. One possible improvement would be to estimate the large-scale slope using a least-squares fit, detrend the bathymetry using this large-scale slope, and then compute the roughness from the detrended data.

Lastly, the strain spectra presented in Figs. 1 and 4 become increasingly blue at wavenumbers above ~0.1 cpm. This is unusual for strain spectra derived from standard CTD data (see Kunze et al. 2006) and raises two possibilities: either there are instrument/noise issues in the CTD data used in this study, or the blue spectra reflect real non-internal-wave dynamics (e.g., double-diffusive processes or other high-wavenumber phenomena). The manuscript should address this explicitly. If this is instrumental, the authors must quantify the noise contribution before calculating strain variances. If this is physical, the authors should discuss whether the standard internal-wave finescale parameterizations can be used in this area despite the clear spectral departure.

Because of these major concerns, I am unable to recommend this manuscript for publication in its current form. My additional specific comments and suggestions for future improvement are listed below.

L86: I cannot find the Strait of Gibraltar in Fig. 1.

Fig. 1: It would be helpful to overlay the circulation pattern on the map.

L113-124: are tidal flows much weaker than the mean flow?

L125-134: It would be helpful to explicitly state what is lacking in Artale et al. (2018) in order to better motivate the present study. Clarifying the gaps or limitations in that work will strengthen the rationale for your research.

L206, 208: I suggest reconsidering the use of “overestimate” and “underestimate” in the text. In this context, terms like “increase” and “reduce” may be more accurate and appropriate.

L208-210: I cannot understand this argument. Please explain more.

L249: 0.22, not $0.22N^2$

L258-261, L403-407: I’m confused with the statements. Could you clarify what “80 m” refers to in this context? Bin size should be larger than 640m?

L268-275: “from 640 to 107 m” It would be easier to understand if you rewrite them in rad/m

L314-373: I think the descriptions are very redundant. For example, equations 7 and 8 are identical. Please rewrite.

L435-438: I think the description is not accurate. The internal-wave frequency or propagating angle doesn’t change during the reflection, but wavenumber changes.

L453: is this spectral increase statistically significant?

L480: I cannot discern a clear coherent pattern in the figure. The authors should clarify which aspects they consider coherent and provide an explanation to support this interpretation.

L513: “suppression of the strain in rougher area” is correct?