Reviewer comments

Reviewer #1:

This study analyzes the variability of near-bottom shear/strain variance ratios ($R\omega$) in the western Ionian Sea, with particular emphasis on how these ratios are influenced by local seafloor parameters. The authors find that $R\omega$ 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 $R\omega$ 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 that is similar to the global map of Nikurashin and Ferrari (2011), and comparing it with the observed $R\omega$ variability.

We thank the reviewer for the encouraging comment. We followed the reviewer's suggestion and applied Bell's linear theory to estimate the barotropic-to-internal tide conversion at each station (St. Laurent et al. 2012; See Fig. R1). For this purpose, we used the bottom velocity from the LADCP, the local buoyancy frequency at the seafloor, and an effective topographic height H and length scale L derived from the GEBCO bathymetry (with $H = \sqrt{var(z)}$ and H/L given by the local bottom slope). This yields a Bell-type conversion rate of order 10^{-5}–10^{-3} W m^{-2} over most stations. These small values are consistent with the fact that our (s/α, ku_0/ω) parameters lie in the subcritical, low-excursion regime, where linear internal tide generation is expected to be weak and lee-wave generation is not dominant. We therefore conclude that barotropic lee-wave generation cannot explain the observed variations of the shear-to-strain ratio, which are more likely related to modifications of the internal-wave spectrum by background stratification and shear.

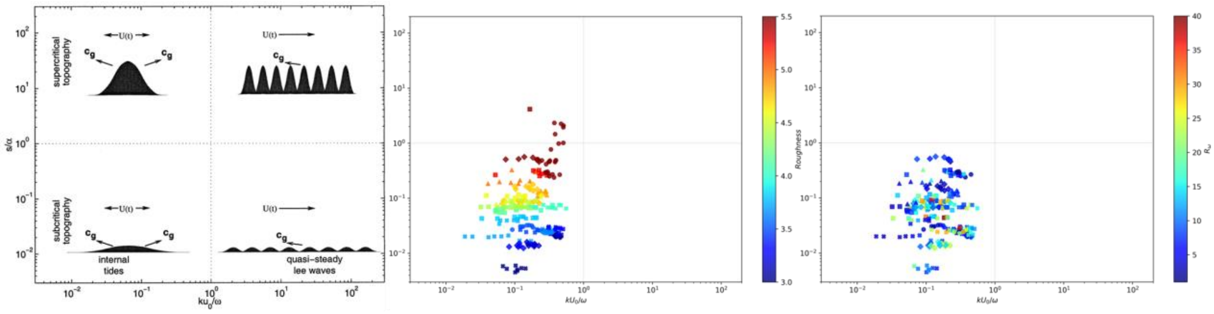


Figure R1. *Diagram showing the internal wave response to tidal flow over bathymetry for the slope parameter s/α and the tidal excursion parameter ku_0/ω (left panel from St. Laurent et al. 2012; middle and right panels from our observations, in the same way than St. Laurent et al. 2012, highlighting variability of topographic roughness and Rω). Internal tides occur when the scale of the topography k^{-1} exceeds the tidal excursion scale u_0/ω. In this regime, waves radiate energy in both the up- and downstream directions, as shown by the group velocity (c_g) vectors. Quasi- steady lee waves occur when the scale of tidal excursion exceeds the scale of the topography. Wave energy is radiated in the upstream direction only. The right panel does not show those shallow stations were Rω cannot be estimated.*

In the main part of the analysis, the authors compare $R\omega$ with the "Δ-slope", the difference between the $R\omega$-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 $R\omega$ 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 $R\omega$ 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 $R\omega$ 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 $R\omega$ and the local topographic slope and roughness.

Many thanks for this comment. Yes, of course, the 1/x behaviour is due to the analytic relationship showed by the reviewer. However, making the difference "Δ-slope" allows us to highlight and disentangle the dynamic range that is controlled by the roughness. This is evident, for instance, in our Fig. 3b (from the original version of the manuscript), where the $R\omega$ vs. Δ-slope relation tends to linearize for high values of bathymetric roughness. This confirms that high roughness reduces the $R\omega$ range (e.g., $R\omega$ in the range 0 - 10) by enhancing higher frequencies, low roughness increases the $R\omega$ range (e.g., 0 - 30) by broadening the frequency range, i.e., allowing more low-frequencies. On the other hand, the relation $\beta(R\omega)$ vs. $R\omega$ (Fig. R2) does not identify the separation; the 1/x is not what we want to highlight: we want to highlight the dynamical separation.

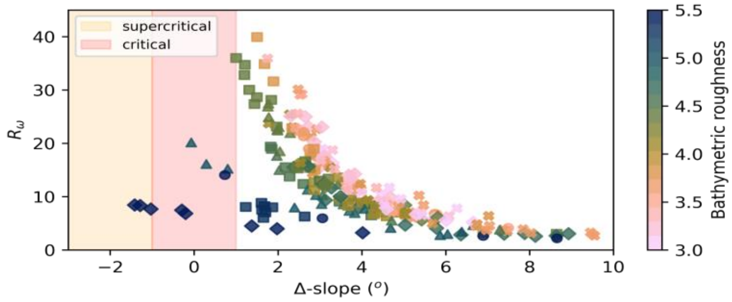


***Figure 3b (from the original version of the manuscript)**. Distributions of Rω in the layer 360-920 m distant from bottom, in function of the difference between the associated beam angle and the bathymetry slope (Δ-slope). Roughness is indicated through the z-axis (in colour). Higher values on the x-axis indicate a beam angle steeper, relative to the bathymetry slope, while lower values indicate a beam aligning with the slope. The critical range where the alignment is close to be parallel to the slope is arbitrarily marked by the shaded red area from -1 to 1 degrees, out of which a supercritical range is indicated in orange.*

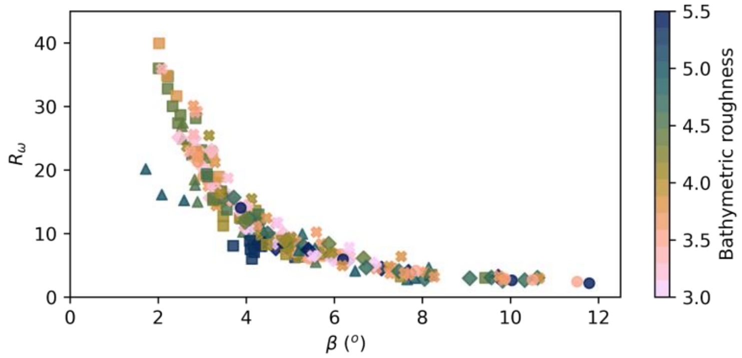


***Figure R2.** Distributions of Rω in the layer 360-920 m distant from bottom, in function of internal-wave propagation slope (β).*

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.

We followed the reviewer's suggestion. We computed the bathymetric roughness after removing a large-scale linear trend (least-squares plane fit) and after applying a low-pass Gaussian filter to define the large-scale component (Fig. R3). From a comparison between our original method and what suggested by the reviewer we observe that the station-to-station patterns and the correlations with the shear-to-strain ratio remained essentially unchanged (Figs. R3 and R4). Moreover, the ranking of "rough" versus "smooth" sites was the same as with our original definition. This indicates that our results are robust to the specific detrending/filtering method, and we therefore retain the original roughness metric in the main analysis and mention these tests as a robustness check.

Figure R3. *Setup of the bathymetric smoothing applied to the GEBCO product. The first panel shows the smoothed bathymetry together with a longitudinal section across the Ionian Basin at the mid-latitudes of the study area. The other panels display the slope and roughness computed using spatial windows of 6 points (2.7 km), 12 points (5.3 km), and 23 points (10.3 km). Roughness is estimated using three approaches: (1•) the variance of bathymetry within the window (our current method), (2•) the variance of bathymetry after removing a local smoothing within the window, and (3•) the variance of bathymetry after removing the large-scale smoothing.*

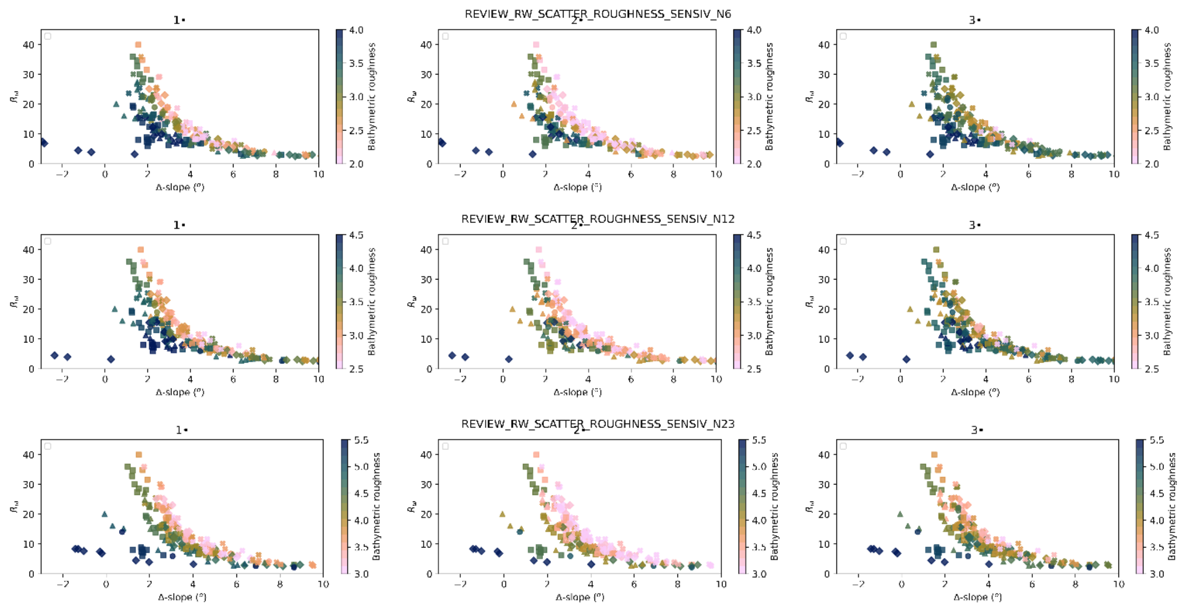


Figure R4. *Scatterplots of Rω in function of Δ-slope for the three roughness calculations and for spatial windows of 6, 12, and 23 points.*

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

To address the reviewer's concern about the blueing of strain at wavenumbers above ~0.1 cpm, we note that 0.1 cpm corresponds to a vertical wavelength of 10 m. This is exactly the upper wavenumber limit we use for finescale integration: ***our strain variance is integrated only up to 0.1 cpm****. Therefore, the part of the spectrum where noise might increase is explicitly excluded from the calculation of Rω.*
The absence of a spectral roll-off simply reflects that we do not use finite differencing (FD) to calculate the strain: instead of employing N^2, the vertical derivative is obtained by multiplying the density by the vertical wavenumber in spectral space, which avoids the variance loss inherent to FD. This method is a variant from Kunze et al. (2006), and follows Ferron et al. (2014), and it is fully described in our Methods section. Moreover, as described in the methods too, in Ferron et al. (2014) a 160m- high-pass filter is explicitly applied on density to remove large-scale density variations, that can lead to spectral shape differing from Kunze et al. (2006) for scale larger than 160m. The overall level of our strain spectra is also closer to GM, which is consistent with the fact that our observed internal-wave field is close to GM in amplitude. ***These considerations will be added in the revised version of the manuscript.***
We acknowledge that the spectra in Fig. 1 of our original manuscript can be partly misleading because they include all water-column segments, including the shallow ones, which are more likely to contain smaller-scale variance from mesoscale contamination or double-diffusive layers. These segments are not used in our analysis: we focus exclusively on the deepest ~1000 m, and this "non-inclusion" is already visible in the spectra binned by roughness or Δ-slope that encompass only the deepest ~1000 m. ***To avoid any ambiguity, we will revise Fig. 1 (see new Fig. R5, which now shows only these analysis-relevant bins).***

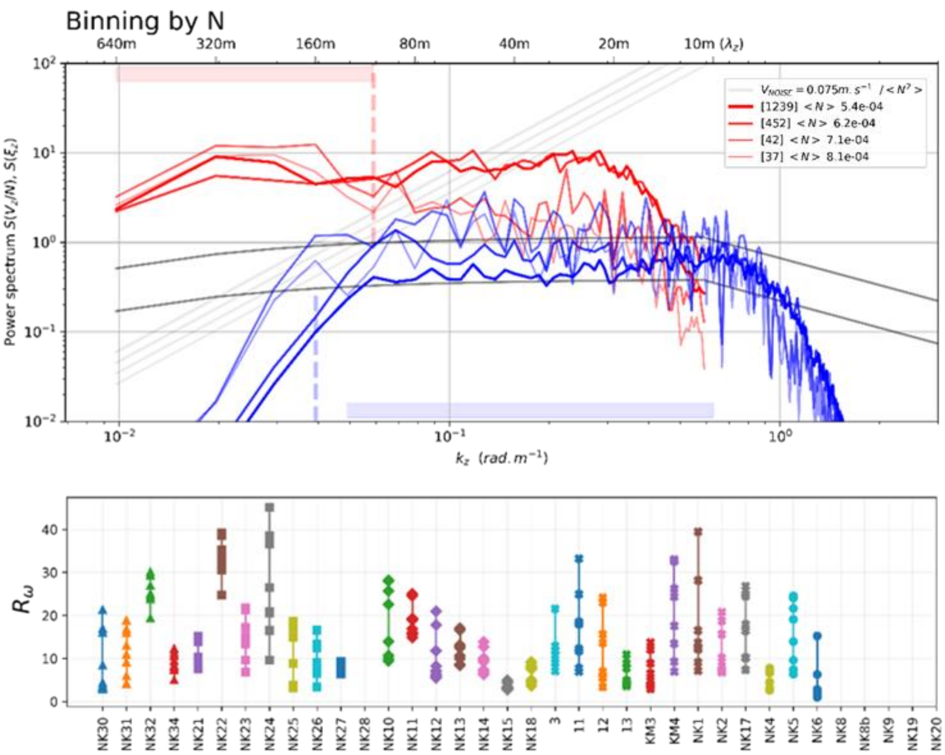


Figure R5. *Shear–strain spectra averaged within bins of stratification (upper panel) and computed over the bottom 1000 m of the water column (i.e. excluding surface layers out of the scope of our study). Strain spectra were low-pass filtered at high wavenumbers (cutoff at 10 m) to remove potential noise contamination. Rω distributions are slightly shifted up as a result of the variance filtering, but their overall shape and structure remain essentially unchanged with respect to the original Fig. 1. The associated estimates of Rω are shown by station and by vertical bins of 80 m, within the same bottom 1000 m layer (lower panel).*

Concerning double-diffusion, we provide a verification showing that its contribution is marginal. The Turner angle (Tu) and the density ratio ($R\rho$) are used to define the local stability of an inviscid water column to double-diffusive

$R_\rho = \frac{\alpha\left(\frac{\partial T}{\partial z}\right)}{\beta\left(\frac{\partial S}{\partial z}\right)}$; $Tu = tan^{-1}\left(\frac{R_\rho+1}{R_\rho-1}\right)$;

where:

- *-45°<Tu<45°: statically stable*
- *-90°<Tu<-45°: unstable with diffusive convection*
- *45°<Tu<90°: unstable to salt fingering*
- *Tu<-90°, Tu>90°: gravitationally unstable*

Turner angles (Tu) were computed for all segments, and only a small fraction of the layers is falling into Salt Fingering (SF; 13.6%) or Diffusive Convection (DC; 1.8%) where regimes exhibit active values (|Tu| above 75º) within the depths where Rω is evaluated. Thus, at the same time than these few potential layers do not present particularly significant structures in density, we consider that double-diffusive layers are only minimally present in our analysis. The corresponding plots are available at the link below:
https://drive.google.com/drive/folders/1B_TrGE7PKbZx29nEWeAL285LqKRSoGHk?usp=sharing

Reviewer #2:

The authors present an analysis of CTD/LADCP measurements from 2007 to investigate internal wave properties in the deep waters of the Ionian Sea. The major focus of the study are the ratios of integrated shear and strain variances, which, for linear waves, allow to infer wave frequencies, and their relationship to bottom depth, bottom slope, and bottom roughness. The major result of the study is that shear-to-strain ratio R_omega varies with bottom slope and roughness. Bottom slope and roughness appear to be closely related in this region.

The presented research is an interesting piece of analysis on the internal wave climate in the deep Ionian Sea using existing CTD/LADCP observations. This type of analysis has been carried out before, but I believe it is of interest in the regional context (if the authors can show that these results are new for the Ionian Sea).

Based on my review below I suggest a major revision of the paper.

We thank the reviewer for the encouraging comment. We provide below our revision notes (blue ink), which should address all major and minor points that were raised.

Major Points

The study lacks details on the regional context of both the regional circulation and the regional internal wave climate. Are there other studies on the internal wave climate of the Ionian Sea? There are papers showing how the low stratification of the deep Ionian Sea can support vertical inertial oscillations (e.g. van Haren & Gostiaux, 2007, GRL). Possibly related, why are shear variance spectra so much larger than GM spectra while strain spectra are much closer to GM?

Along with the general description of the Mediterranean Sea general circulation and the Ionian Sea dynamics (see the Sub-section 1.1) we now present a review on internal wave climate in the Ionian Sea (Sub-section 1.2):

Internal waves constitute a fundamental component of the Mediterranean Sea dynamics and play a key role in vertical energy transfer and in the long-term redistribution of heat and tracers. The semi-enclosed geometry of the Mediterranean, its mid-latitude setting, and the presence of steep and irregular bathymetry strongly shape the internal wave field, distinguishing it from that of the open ocean (Millot and Taupier-Letage, 2005; Schroeder et al., 2016).

In particular, the Eastern Mediterranean is characterized by weak deep stratification, quasi-homogeneous abyssal layers, and pronounced topographic gradients, conditions that make this sub-basin highly sensitive to internal wave processes and their variability (van Haren and Gostiaux, 2011). Despite the very low buoyancy frequencies typical of its deep layers, long-term high-resolution temperature measurements revealed intense internal wave activity in the deep Ionian Sea: vertical displacements reached several hundreds of meters, with significant vertical velocities occurring at near-inertial frequencies (van Haren and Gostiaux, 2011). These observations further showed that energetic internal waves advected through abyssal waters can generate strong vertical shear and episodic overturning, indicating that internal wave–induced mixing remains effective even under extremely weak stratification. Weak stratification does not inhibit internal wave dynamics; rather, it modifies their geometry and frequency content, allowing motions with substantial vertical components to develop in the deep basin (van Haren and Millot, 2004; van Haren and Millot, 2005).

Evidence of energetic internal wave variability has also been reported in other Mediterranean sub-basins, such as the Western Mediterranean, where studies in the Tyrrhenian Sea documented active internal wave fields associated with mesoscale circulation and boundary processes (van Haren, 2004; Buffett et al., 2017). Although these studies focus on *different hydrographic and dynamical settings, they support the broader view that internal waves remain dynamically relevant throughout the Mediterranean Sea, including deep environments.*

In addition to broadband internal wave activity, recent studies have highlighted the importance of internal tides as a persistent and organized contribution to the Eastern and central Mediterranean internal wave climate. Although barotropic tides are relatively weak in the Mediterranean Sea, their interaction with steep bathymetric features, particularly in straits and along major topographic gradients, can generate energetic baroclinic internal tides (Artale et al., 1989; Oddo et al., 2023). Observational and modeling studies in the central Mediterranean have shown that diurnal internal tides, often with Kelvin-like characteristics, are generated at the transition between shallow regions of the Sicily Channel and the deep Ionian basin, propagate away from their generation sites, and contribute significantly to the internal wave energy budget while inducing strong vertical shear (Oddo et al., 2023). Direct observations in the deep Ionian Sea further indicate that baroclinic internal tides penetrate into abyssal layers, modulating temperature and velocity fields even under weak stratification (Giambenedetti et al., 2023).

This body of evidence defines a Mediterranean internal wave climate that is both spatially heterogeneous and dynamically rich, shaped by the interplay between stratification, bathymetry, and tidal forcing. Within this framework, the Ionian Sea emerges as a key region where internal wave activity intersects with large-scale circulation variability. The hydrographic structure of Atlantic Water, Levantine Intermediate Water, and deeper water masses controls internal wave propagation paths and vertical structure (Budillon et al., 2010), while variability in this structure directly affects the efficiency and vertical reach of internal wave–induced mixing, particularly along continental slopes and over irregular bathymetry. On longer timescales, decadal changes in water mass properties further modulate the background conditions for internal wave dynamics and internal redistribution of momentum and tracers (Gačić et al., 2010).

When interacting with rough topography, internal waves and internal tides have been linked to the redistribution of heat and tracers in abyssal layers, contributing to the observed variability of heat content and influencing long-term basin-scale circulation (Artale et al., 2018). This mechanism becomes particularly relevant during periods of reduced deep-water formation, when internal mixing plays an increasingly important role in controlling abyssal ventilation. Internal tides, in particular, represent a continuous source of energy that sustains mixing in deep layers, complementing episodic convective events and reinforcing the coupling between the deep Ionian interior and boundary regions (Giambenedetti et al., 2023; Oddo et al., 2023). Overall, this internal wave climate provides the dynamical background against which internal wave–driven mixing processes operate and contribute to the thermohaline structure and long-term evolution of the Ionian Sea and the broader Eastern Mediterranean basins.

The study does not address the impact of temporal variability of the internal wave field on the results. Instead, results are framed as an investigation into spatial variability of the shear-to-strain ratio. However, other studies have shown that "Temporal variability in R_omega is observed at daily, weekly, and monthly scales." (Chinn et al., 2016, p. 3299)

Unfortunately, our analysis on shear-to-strain ratio is based on CTD-LADCP casts that were carried out during a single oceanographic cruise in 2007, and results are framed as an investigation into spatial variability. Therefore, we cannot do any analysis on the temporal variability of the spatial patterns we observe. However, we can provide some discussions in the light of the interannual analysis (1977-2011) presented by Artale et al. (2018). These authors highlighted the presence of a "new" bottom Ionian water, from the 2003, resulting from the continuous entrainment of the warmer, upper waters, a process that would cause a loss of kinetic energy and gain of potential energy in the deep layer (Marotzke & Scott, 1999). To explore topographic-induced diapycnal mixing these authors calculated a time series of dissipation rates from the "CTD strain-based" parameterization (Kinze et al., 2006). The resulting Hovmöller diagram (Fig. R1) shows an increase of bottom-induced kinetic turbulent diffusion rate from the onset of the 2003. Based on this analysis we can argue that the spatial pattern of shear-to-strain ratio we observe is likely depicting the *particular barotropization of the bottom layer found in Artale et al. (2018), characterized by a homogeneous bottom layer between 3000 and 4000 m depth and weak horizontal gradients of potential temperature over the Ionian basin (Fig. R2). These considerations are now included in the revised version of the manuscript (see Discussions and conclusions).*

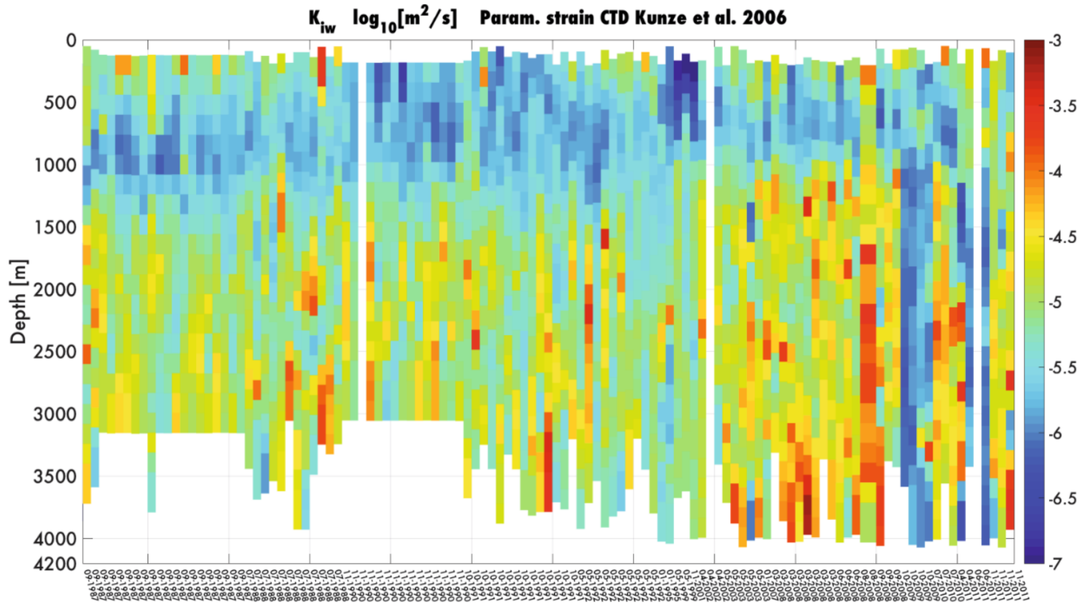


Fig. R1. *Hovmöller diagram (from 1977 to 2011) of the kinetic turbulent diffusion rate (K_{IW}). All details in Artale et al. (2018).*

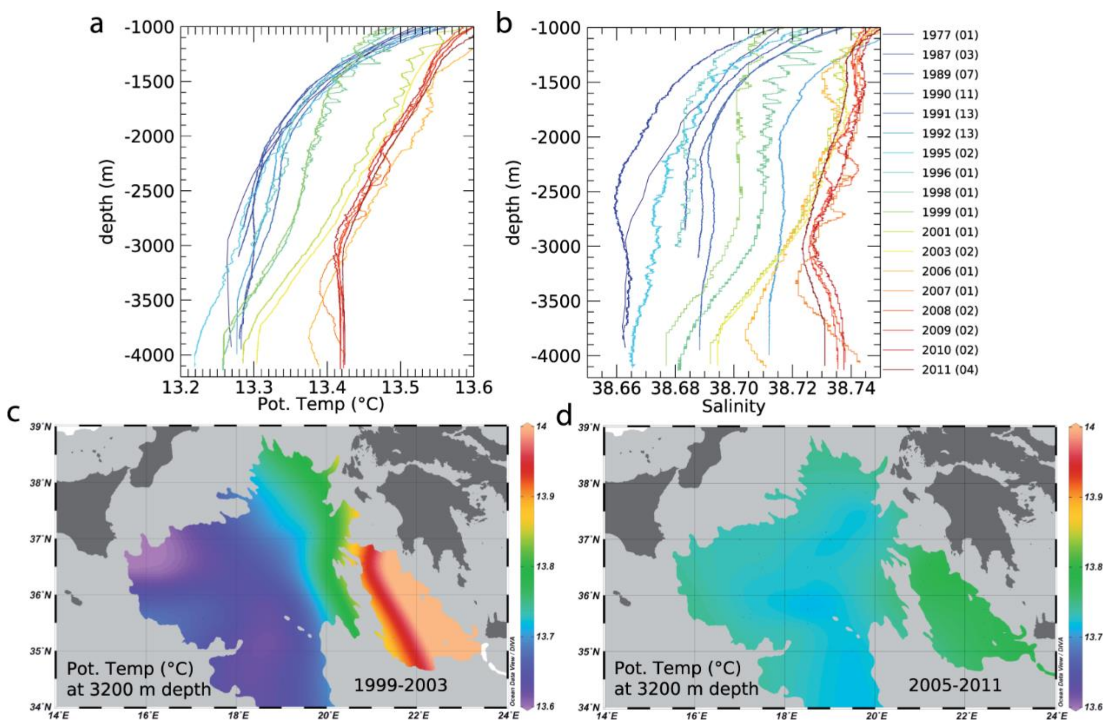


Fig. R2. *(a) Temperature profiles. (b) Salinity profiles; colors indicate the year of data collection, as also reported in Fig. 4a while in parenthesis we report the number of stations that were averaged in the same year. (c) Horizontal temperature map at 3200 m depth, as obtained from CTD measurements collected from 1999 to 2003. (d) Horizontal temperature map at 3200 m depth, as obtained from CTD measurements collected from 2005 to 2011. [From Artale et al., 2028]*

The study lacks a comparison with other papers investigating shear-to-strain ratios outside of Kunze et al. (2006). Is Kunze et al the only paper that looked into shear-to-strain ratios? If not, do results presented here align with other studies?

We now provide some discussion regarding what we found in the light of these previous studies.

Kunze et al. (2006) is not the only study to investigate shear-to-strain ratios. Subsequent observational and theoretical works have shown that R_ω exhibits substantial spatial and temporal variability and departs from the canonical GM value depending on wave regime, forcing, and proximity to rough topography (e.g., Chinn et al., 2016; Polzin et al., 2014; Ferron et al., 2014). Our results are consistent with these studies in showing that shear–strain balance is not universal but systematically modulated by environmental conditions, here specifically seafloor roughness and slope in the Ionian Sea. This last sentence was included in the Discussions and Conclusions:

"Our results are consistent with previous studies (i.e., Chinn et al., 2016; Polzin et al., 2014; Ferron et al., 2014) in showing that shear–strain balance is not universal but systematically modulated by environmental conditions, here specifically seafloor roughness and slope in the Ionian Sea."

Much of the motivation is framed in terms of diapycnal mixing and its importance for the Mediterranean and global ocean circulation. However, the bulk of the analysis focuses on the internal wave climate in the Ionian Sea. I suggest keeping the focus on the internal wave climate without trying to branch out too much. Otherwise, more ground will have to be covered along questions of, e.g., how well the does the finescale parameterization work for estimating dissipation rate of turbulent kinetic energy in these lowly stratified regimes? Is a mixing efficiency of 0.2 applicable here? Why not remove calculation of diapycnal diffusivity (the results are not further analyzed) and maybe even turbulent dissipation and just focus on internal wave dynamics? I am not convinced that the study presents results on regional patterns or the influence of bottom slope on these turbulence parameters.

We now narrow the scope of our paper by maintaining the focus on the relation between IW climate and topographic features, without expanding these findings to the role of diapycnal mixing and its importance for the Mediterranean and global ocean circulation. This topic is just presented as an introductory part. See sub-sections 1.3, 2.2, the final paragraph of section 3 (we also remove the former Fig. 6), and the beginning of section 4

Minor Points

I suggest changing the title as the study is less about abyssal diapycnal mixing but rather on the internal wave climate. How about "Bottom topography effects on the internal wave climate in the Ionian Sea"?

Done. We changed the title using the one suggested by the reviewer

Typos and Small Comments

12f: I sugest removing "diapycnal mixing" and "turbulent kinetic energy" from the keywords and adding "finescale parameterization". *Done. We also added "seafloor topography"*

18: Spatial patterns of diapycnal mixing are not really the focus of the study (see my comment above). *We totally agree. We modified this sentence as:*

"Here we investigate spatial patterns of internal wave climate over the bottom of the Ionian sub-basin."

21f: I believe you show that bottom slope and seafloor roughness are linked to absolute values of shear-to-strain ratio, not variability. *Yes, correct. We modified the text as:*

"Our results show that increasing seafloor roughness reduces the absolute values of shear-to-strain ratio"

28: Specify what you refer to with large-scale and small-scale.

By "large-scale" we refer to low vertical wavenumber, low-mode internal waves with vertical scales O(10^2–10^3 m), while "small-scale" denotes higher vertical wavenumber internal waves with vertical scales O(10–10^1 m), closer to the turbulent dissipation range resolved by finescale parameterizations. We rephrased as:

"...suggest that roughness redistributes energy from large-scale (low-mode, low-vertical-wavenumber internal waves with vertical scales O(10^2–10^3 m)) toward small-scale (high-vertical-wavenumber internal waves with vertical scales O(10–10^1 m)), fundamentally altering the balance of internal wave energy across scales."

29ff: The concluding sentence reads very generic and is not suported by the study. I suggest being more specific in why your results matter. *Thanks for the suggestion. We made the sentence much narrower:*

"These results provide useful knowledge for ad hoc finescale parameterization based on seafloor topography."

43ff: I suggest not going into details of where upwelling and downwelling happens but rather state that where turbulent mixing happens matters. *We simplify the sentence as:*

"The resulting small-scale mixing due to breaking of IWs is, therefore, likely responsible for vertical energy transport, which contributes to ocean ventilation (Munk, 1966; Osborne and Burch, 1980; Walin, 1982; Wunsch and Ferrari, 2004; Garrett and Kunze, 2007; Nikurashin and Ferrari, 2013; Ferrari, 2014; Ferrari et al., 2016; Cavaliere et al., 2021; La Forgia et al., 2021)."

54ff: Rephrase. It is well quite established by now that rough bathymetry leads to enhanced IW generation, scattering, and breaking, and thus increased levels of turbulent dissipation. *Thanks for the suggestion. We rephrased as:*

"On the other hand, it is quite established by now that rough bathymetry leads to enhanced IW generation, scattering, and breaking, and thus increased levels of turbulent dissipation (Polzin et al., 1997; Ledwell et al., 2000; St. Laurent et al., 2012; Ferrari et al., 2016; Mashayek et al., 2017)."

61ff: This would be good to expand on as it lays the groundwork for your study! *Many thanks! We expanded the paragraph by including that:*

"Chinn et al. (2016) found that vertical structure of the shear-to-strain ratio is related to topographic interactions because sites near large topographic features"

65: Mixing processes are not studied in this paper. You are investigating the composition of the internal wave field or the internal wave climate. *Correct! We rephrased the sentence as:*

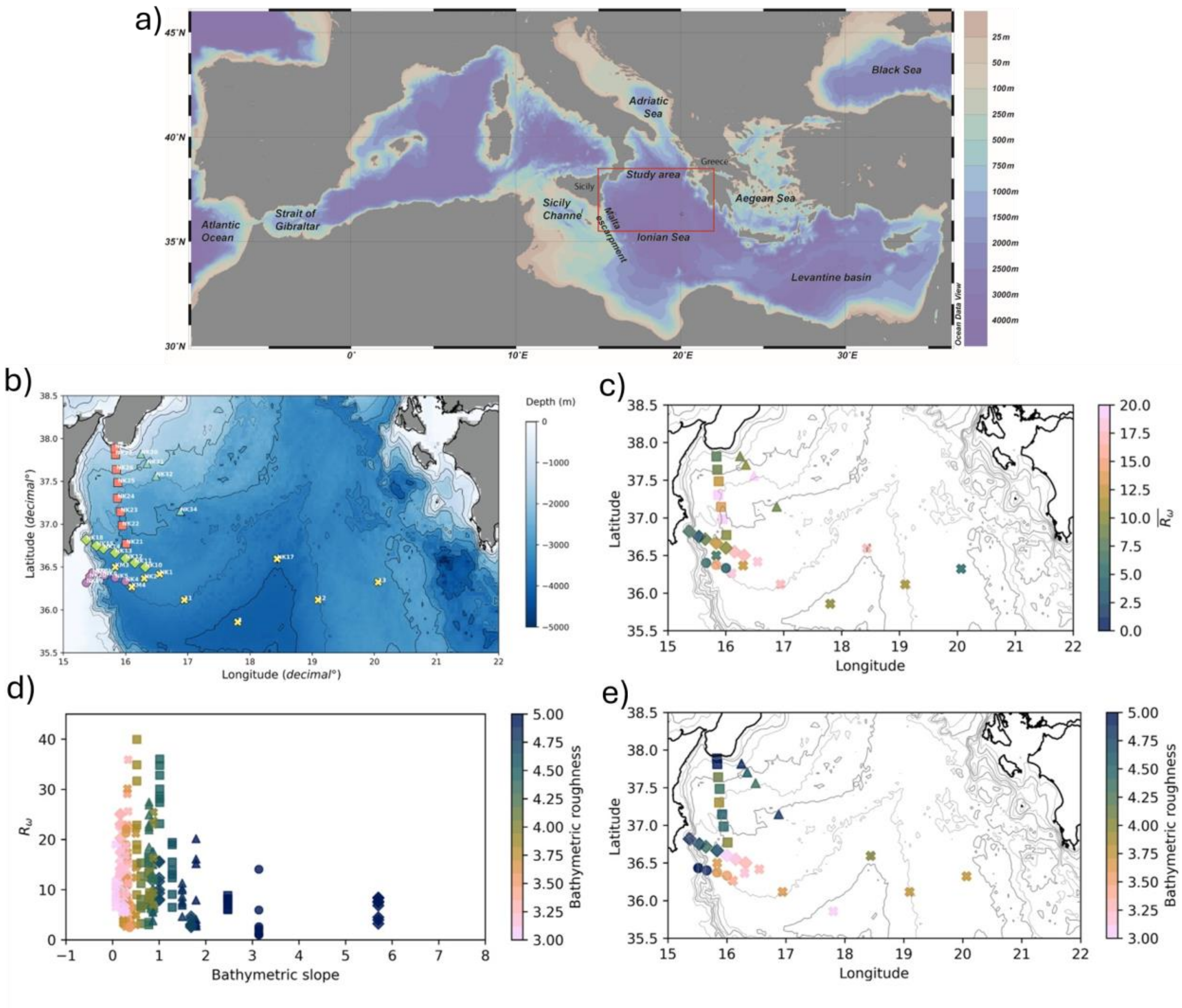
"These considerations inspired us to investigate spatial patterns of internal wave climate and how seafloor topography affects the absolute values of shear-to-strain ratio. Our analysis is concentrated on the Ionian Sea (Eastern Mediterranean basin), where synchronous Lowered Acoustic Doppler Current Profiler (LADCP) and Conductivity, Temperature, Depth (CTD) data were available"

68ff: Consider reducing the discussion on the Mediterranean overturning circulation and adding a review of what is known about internal waves in the region. *We now present a state-of-the-art of internal wave climate in the Ionian Sea. See the new subsection 1.2.*

69f: "one of the most interesting" – rephrase. *We shortened the paragraph and rephrased as:*

"The Mediterranean Sea is often considered as a "miniature ocean", due to its morpho-bathymetric characteristics and its thermohaline cell, where cold and/or salty waters sink during winter at specific regions and, subsequently, spread to intermediate and deep, near-bottom layers (Wüst, 1961; Bergamasco and Malanotte-Rizzoli, 2010; Millot and Taupier-Letage, 2005; Somot et al., 2006; Tsimplis et al., 2006; Schroeder et al., 2016; Artale et al., 2018)."

86: Strait of Gibraltar not shown in Fig. 1a. *We now include a general map of the Mediterranean Sea where we indicate location of the Ionian Sea and all the geographical names we mention in the text, including the Strait of Gibraltar:*



New Fig. 1. *(See caption in the main text)*

103f: Why does this matter? *This sentence is meant to introduce the following one: "These deep waters are characterized by IW-turbulence and mixing (van Haren and Gostiaux, 2011)."*

105: What "flow" is this? *See answer above.*

106f: More information in internal-wave-driven turbulence in the region is needed here. *See answer above.*

109: What is unique here? *See answer above. "Unique" is removed*

121: "impulsive" - Does this mean temporally variable? If so, on what scales? *Thanks for this comment. "impulsive" was actually referring to the sentence "Furthermore, cyclonic and anti-cyclonic mesoscale structures, with a period of 5-to-11 days...". We rephrased this part as:*

"Deep current meter measurements collected in the westernmost part in the period 2007–2008 showed a quite energetic and impulsive abyssal circulation pattern, strongly affected by the bathymetric constraints, with maximum velocities up to 15 cm/s and cyclonic and anti-cyclonic

mesoscale structures, with a period of 5-to-11 days, superimposed on the background geostrophic flow (Rubino et al., 2012; Meccia et al., 2015)."

133f: "showing the role..." - Can you be more precise and state their central result in this regard? Thanks for this comment. *We now provide more detail regarding Artale et al. (2018) findings:*

"To explore topographic-induced mixing, these authors estimated dissipation rates from the "CTD strain-based" parameterization (Kunze et al., 2006), showing the role of the sea bottom in enhancing isopycnal vertical strain. In particular, they highlighted that the turbulent kinetic energy generated by breaking internal waves is large within a few hundred meters of rough-bottom topography and decays to weaker values farther up in the water column; the bottom enhancement of turbulence reflects the generation of energetic waves, impinging over topography and breaking locally."

135: I am guessing you mean "synergetic", but why not simply state here that you expand the Artale et al analysis by including LADCP measurements to investigate shear variance in addition to strain variance? *Thanks for the comment. We rephrased as:*

"From the synergetic use of LADCP and CTD data, collected in the Ionian Sea during the KM3Net cruise in 2007, here we expand the analysis of Artale et al. (2018) to investigate shear variance in addition to strain variance, also exploring the role of topographic characteristics, such as bottom slope and roughness."

140f: See my other comments suggesting removing diapycnal diffusivity estimates. *Many thanks. We removed results and discussions on diapycnal diffusivity.*

150: "Hydrological" - The term does not include velocity measurements, see below. *We change the subtitle as: "CTD-LADCP casts"*

151: Move the reference to Borghini et al to the end of the sentence. *Done*

153: Does this refer to CTD/LADCP? The term hydrological does not include velocity measurements. *We rephrased as "CTD-LADCP station"*

155: have -> had. *Done, thank you!*

158: in -> at. *Done, thank you!*

160: have been preset -> were set. *Done, thank you!*

161: 10.00 -> 10. *Done, thank you!*

162: Move the link to the "Data and Code Availability" section and add reference to Visbeck (2002, JTECH). *Done, thank you!*

162f: Were shipboard ADCP data available to include in the processing? What about bottom tracking velocities from the downlooker? *Shipboard ADCP data were not available (gyro was not working).*

182: a recent -> the form of the. *Done, thank you!*

186: Brunt-Väisälä (buoyancy frequency is much easier!). *We changed.*

195: buoyancy-normalized. *Done, thank you!*

206ff: It would be better to point out (as Chinn et al) that the error associated with not having the right W_omega is not symmetric and therefore it is better to use shear variance for R_omega > 3 and strain variance for R_omega < 3. *We rephrased as:*

"Equations (3a,b) point out that the error associated with incorrectly assuming R_ω is not symmetric (Chinn et al. 2016). For R_ω =3 (i.e., the GM value), the correction functions are equal to 1 and do not affect ε_iw. When R_ω>3, shear variance dominates, low-frequency internal waves contribute more strongly, and h_1 decreases (underestimates ε_iw), while h_2 increases (overestimates ε_iw). When R_ω<3, strain variance dominates, high frequency internal waves contribute more to the ratio: h_1 increases (overestimates ε_iw), h_2 decreases (underestimates ε_iw). This makes the shear-based parameterizations more reliable when R_ω>3, and conversely the strain-based parameterizations are more reliable when R_ω<3 (Chinn et al. 2016)."

211: Remove "ideally". *Done, thank you!*

228: shear -> shear variance. *Done, thank you!*

248f: Do you implicitly apply a shear-to-strain ratio of 3 here?

[FLORIAN] If the reviewer refers to the ratio between the saturation threshold values themselves, so we implicitly apply a ratio of 3 given the chosen thresholds (0.66 N^2 for shear and 0.22 for strain)

259: more robust than? *We rephrased as:*

"to have a statistically robust representation of the fine-scale variability"

260: Remove "Conveniently". *Done, thank you!*

263: Shouldn't this be the Visbeck reference I mentioned above and not Polzin? *Correct. We changed the reference.*

263: It is unclear what "single bin error profile" refers to.

By "single-bin error profile" we refer to the depth-dependent velocity uncertainty provided by the LDEO LADCP inversion, which represents the estimated error associated with each individual vertical bin of the LADCP velocity profile. This error profile is routinely output by the LDEO processing (Polzin, 2002) and is used here to identify the vertical-wavenumber range over which shear spectra are not dominated by instrumental noise. We modified the text as:

"The shear integration upper limit is identified from the LADCP velocity uncertainty. The LDEO LADCP processing (velocity-inversion method; Polzin, 2002) provides, for each station, a depth-dependent single-bin velocity error profile, i.e. the estimated uncertainty associated with each vertical velocity bin, which is used to constrain the usable shear wavenumber range."

265: "distributed on the spectral domain" - what does this mean?

By "distributed on the spectral domain" we mean that the velocity noise is assumed to be white, i.e. uncorrelated in space and affecting all vertical wavelengths equally. The single-bin velocity error provided by the LADCP processing is interpreted as the variance of this white noise. When mapped *into the spectral domain, this corresponds to a flat (wavenumber-independent) velocity noise spectrum. After differentiation, this yields a shear noise spectrum that increases with wavenumber and is used to identify the wavenumber range where observed shear variance is dominated by noise rather than internal waves. We changed the text as:*

"Its associated variance V_NOISE^2 is assumed to arise from white velocity noise, uncorrelated in space and therefore uniformly distributed across vertical wavenumbers. In the spectral domain, this corresponds to a flat velocity noise spectrum which, once differentiated, produces a shear noise spectrum used to identify the noise-dominated upper wavenumber limit."

269: highlighted -> highlight. *Done, thank you!*

273: Remove "then". *Done, thank you!*

282ff: Why calculate vertical diffusivity? How is N^2 calculated? Is the use of gamma=0.2 justified? *We followed the main suggestion of the Reviewer, and we removed calculation of diapycnal diffusivity and turbulent dissipation, focusing only on internal wave dynamics.*

285: Does this refer to the code used for your calculations? Move this to the data and code availability section at the end. *Done, thank you!*

292: spatial or temporal. *We rephrased as "with no significant spatial or temporal variability in spectral composition".*

293: "near mixing hotspots" - is this the only case where it deviates? *We rephrased as:*

*"However, these models sometimes fail to capture the true dynamics**, for instance,** near mixing hotspots where the internal wave spectra can be significantly distorted, showing bias toward higher or lower frequencies (Chinn et al., 2016; Polzin et al., 2014; Ferron et al., 2014)."*

313: and temporal? *Correct. We rephrased as: "emphasizing the need to resolve the spatial and temporal variability of R_ω."*

325: ration -> ratio. *Done, thank you!*

328f: This has just been stated on line 326. *Correct. We removed this sentence.*

329ff: "Even..." - "Even" makes no sense here - best to remove this statement. *Done, thank you!*

332: What does "realistic" mean here? Observed? *Yes, correct. We rephrased as "In observed internal wave spectra"*

334: Do you have a reference for this statement?

Done, thank you:

Kunze, E., Firing, E., Hummon, J. M., Chereskin, T. K., & Thurnherr, A. M. (2006). Global abyssal mixing inferred from lowered ADCP shear and CTD strain profiles. Journal of Physical Oceanography, 36(8), 1553–1576.

Musgrave, R., Nash, J. D., Alford, M. H., Kunze, E., Klymak, J. M., Legg, S., MacKinnon, J. A., & Winters, K. B. (2022). The lifecycle of topographically-generated internal waves. In Ocean Mixing (Elsevier), Chapter 6, pp. 103–166.

343ff: Can you rephrase this? What spatial variability? What does sinusoidal sense mean in this context?

The sentence has been removed to avoid an over-general statement not strictly required for the interpretation of our results.

345: Do you have a reference for this statement? *We rephrased the sentence as:*

"In observed internal wave spectra, higher-frequency components tend to be associated with enhanced isopycnal displacement relative to velocity shear, leading to strain-dominated regimes (Garrett and Munk, 1975; Kunze et al., 2006)."

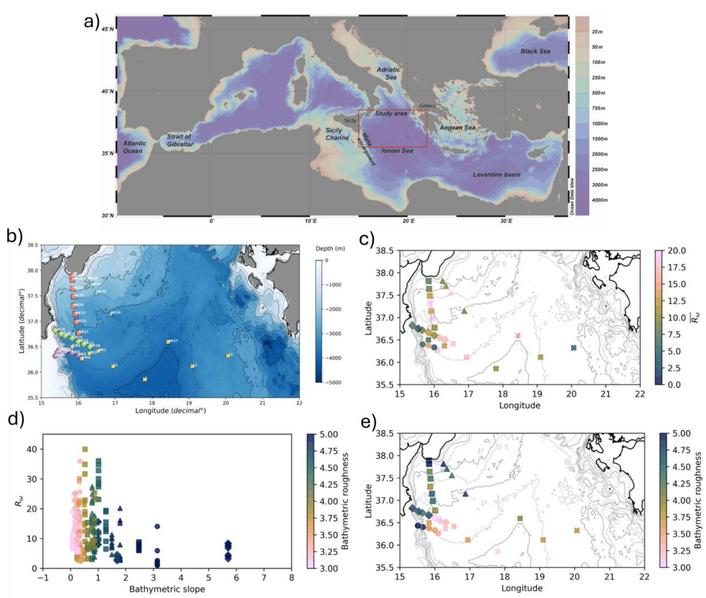
366: on -> in. *Done, thank you!*

368: bathymetrical -> bathymetric. *Done, thank you!*

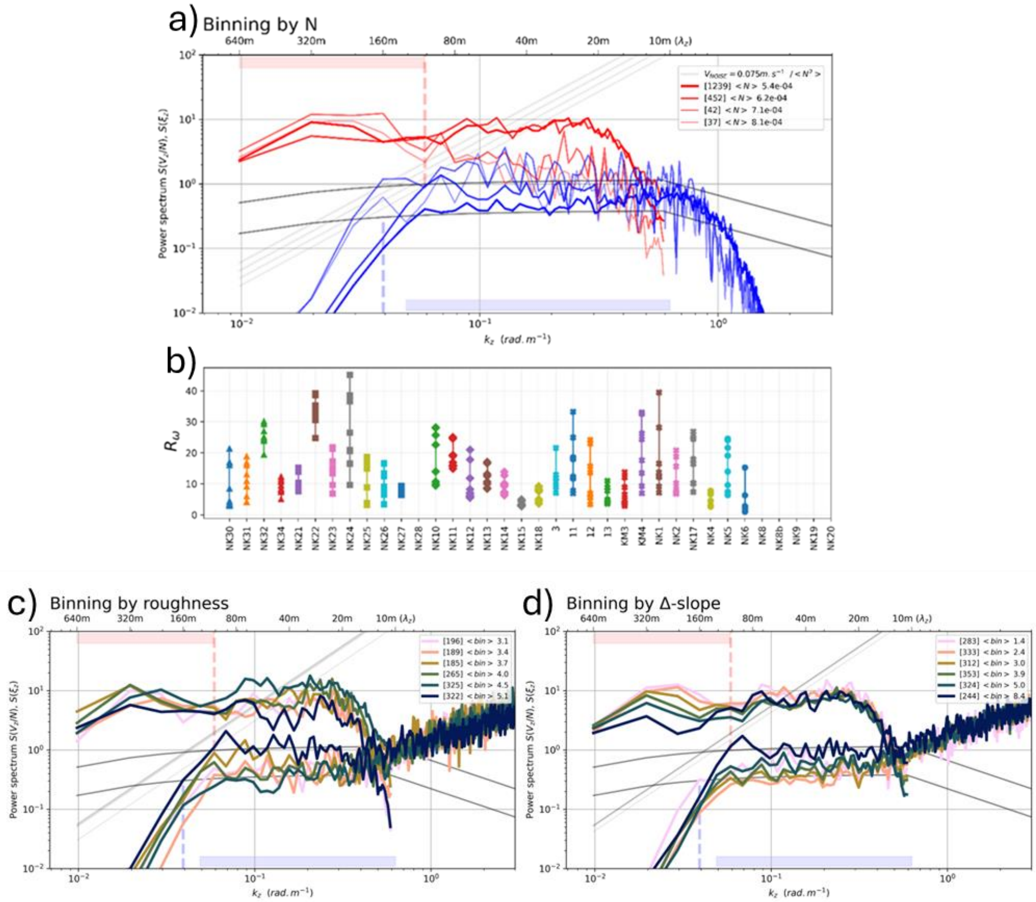
379: Does this mean internal waves on line 376 refer only to tides? *No, indeed. We rephrased the sentence (i.e., we removed "In addition to tidal processes").*

385f: Reference? If you merge this paragraph with the next one then you have your reference in Kunze et al. Also, what does large-scale refer to in this case? *Thanks! We merged the two paragraphs. By "large-scale" we refer to low vertical wavenumber, low-mode internal waves with vertical scales* $O(10^2$*–*10^3 *m); see answer for line 28.*

394: It would be good to show your results for slope and roughness with two extra panels in Fig. 1. I suggest moving current 1b to its own figure and adding slope and roughness here. *We followed this suggestion and we merged Fig. 1a and Fig. 2. The 1b is now merged with Fig. 4:*



New Fig. 1 (see main text for the caption)



New Fig. 3 (see main text for the caption).

399f: How is roughness calculated in other studies, e.g. Kunze et al 2006? Does your method diverge from these? *Please, see answer to Reviewer#2. We computed the bathymetric roughness after removing a large-scale linear trend (least-squares plane fit) and after applying a low-pass Gaussian filter to define the large-scale component (Fig. R3). From a comparison between our original method and what suggested by the reviewer we observe that the station-to-station patterns and the correlations with the shear-to-strain ratio remained essentially unchanged (Figs. R3 and R4). Moreover, the ranking of "rough" versus "smooth" sites was the same as with our original definition. This indicates that our results are robust to the specific detrending/filtering method, and we therefore retain the original roughness metric in the main analysis and mention these tests as a robustness check.*

Fig. R3. *Setup of the bathymetric smoothing applied to the GEBCO product. The first panel shows the smoothed bathymetry together with a longitudinal section across the Ionian Basin at the mid-latitudes of the study area. The other panels display the slope and roughness computed using spatial windows of 6 points (2.7 km), 12 points (5.3 km), and 23 points (10.3 km). Roughness is estimated using three approaches: (1•) the variance of bathymetry within the window (our current method), (2•) the variance of bathymetry after removing a local smoothing within the window, and (3•) the variance of bathymetry after removing the large-scale smoothing.*

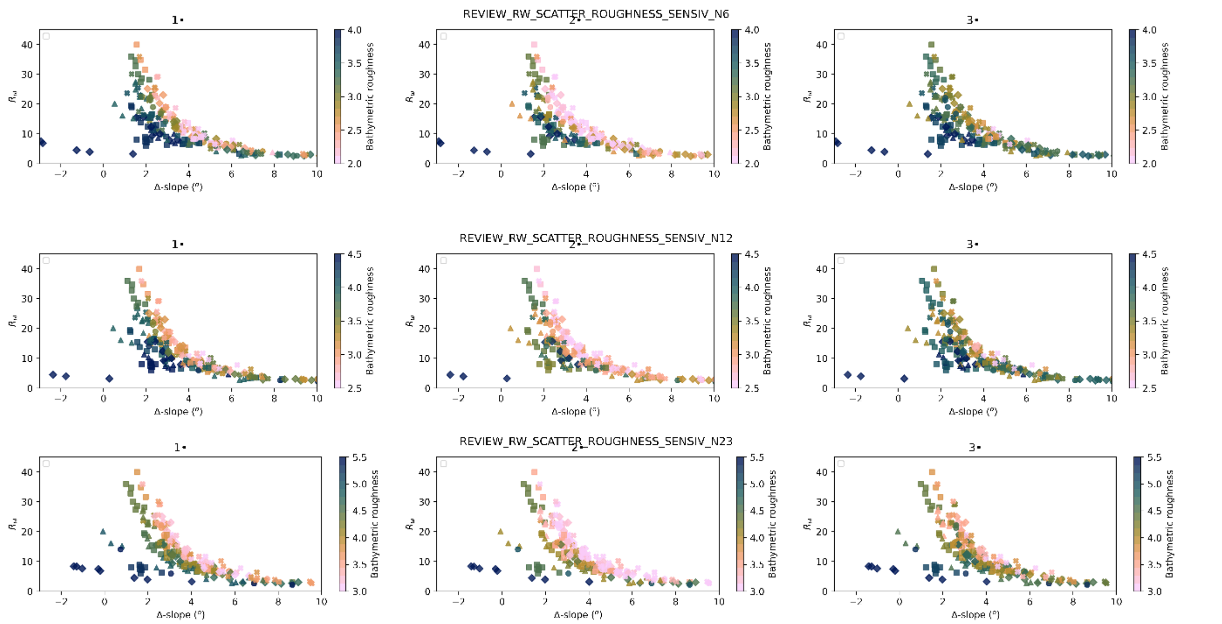


Figure R4. *Scatterplots of Rω in function of Δ-slope for the three roughness calculations and for spatial windows of 6, 12, and 23 points.*

404: to surface -> towards the surface (otherwise this implies you are investigating the full water column). *Done, many thanks!*

406: into -> in. *Done, many thanks!*

406: Remove "then". *Done, many thanks!*

407: The figure reference is not helpful here as so far you have only discussed your binning procedure. *Thanks. We removed it.*

409: 1 -> 1b. *This is now Fig. 3.*

409: Smaller -> Small. *Done, many thanks!*

411: Unclear where the Malta escarpment is. *The Malta escarpment is now indicated in Fig. 1a*

411f: I suggest moving this to the discussion section (where you already talk about this). *Indeed, this sentence (i.e., ", where mesoscale bottom meandering and vortical structures occur (Sparnocchia et a., 2011; Rubino et al., 2012)") is already stated in the discussion section. We removed from here.*

413: increase with depth? *We rephrased as:*

"High values of shear-to-strain ratio are evident in the western part of the abyssal plain..."

417: Move this to where you describe roughness calculation on lines 399f. *Done, we moved it.*

418: Remove "it results that". *Done, many thanks!*

421ff: Remove sentence "Correspondingly, ..." as this just repeats what has been stated already. *Done, we moved it.*

423: Slope and roughness appear to be almost linearly related. Are you sure your method for estimating these is good and without any errors? *Please, see answer for line 399f.*

425: Are you sure that "dispersion" is a good term to use here? *We removed "dispersion".*

429: You have highlighted this in your theory section, i.e. no analysis necessary. *We rephrased as:*

"Indeed, the increasing R_ω corresponds to decreasing frequency and flatter beam angles"

431: There are lots of vertical lines in Fig. 3a. *We rephrased as "The grey vertical line and shaded area".*

434: What does "bulk" stand for? *We removed "bulk"; α is defined in Eq. (10).*

436: In Fig. 3b and c supercritical is marked as delta slope < 0. Which one is right? *Many thanks! We corrected the text:*

Δ-slope < 0° : supercritical

The internal wave ray slope is steeper than the topographic slope. Energy cannot propagate upslope and is preferentially reflected, leading to enhanced shear, trapping, and potential breaking.

Δ-slope = 0° : critical

Ray slope matches the topographic slope. This is the resonance condition.

Δ-slope > 0° : subcritical

Ray slope is gentler than the topography. Energy can propagate upslope. "Supercritical conditions correspond to ***Δ-slope < 0°****, i.e. when the internal wave ray slope exceeds the local bathymetric slope."*

437: "Once scattered through this quantity" - what does this mean? A scatter plot against delta slope? If so, just remove this. *Yes, we rephrased the sentence as:*

"R_ω is generally related to Δ-slope (Figure 3b): higher R_ω values correspond to..."

437f: Remove "a clear pattern appears i.e.". *Done, see above.*

438: clearly -> generally. *Done, see above.*

449: "characterized by elevated roughness" - Yes, for the highest roughness (bin 5.1). However, bins 4.5 and 4.0 show relatively low strain variance which is not supportive of your statement. *We rephrased as:*

"characterized by the highest roughness (bin 5.1)".

450: "shear variance diminishes at low wavenumbers" - Not clear. Are any differences here statistically significant? *Thank you for this comment. We do not claim statistical significance in a formal sense. The statement refers to a systematic tendency observed across regions when* *comparing normalized shear spectra. To avoid ambiguity, the text has been rephrased to emphasize relative spectral redistribution rather than an absolute or statistically significant decrease in shear variance:*

"A tendency toward reduced shear variance at low wavenumbers is observed in rougher regions, accompanied by a flattening of the shear spectrum. Given the limited number of realizations, these differences are interpreted as systematic patterns rather than statistically significant changes."

454f: Yes for the highest roughness but not generally, see comment above. *We rephrased as:*

"Strain spectra exhibit a distinct pattern for the highest roughness".

459: "shear spectra in Fig. 4b show a clear dependence on delta slope" - Yes! *Thanks!*

459f: Not sure why you talk about R_omega here? *This sentence is meant to link Fig. 4 with what we observe in Fig. 3.*

461f: "Spectra [...] support what we observed" - not a surprise as R_omega is calculated from shear and strain spectra! *We rephrased as: "Spectra in Figure 4b are coherent with what we observed in Figure 3".*

462: "critical threshold" - please explain this further. *We added the following sentence to explain better:*

"...a critical threshold where high frequencies and smaller scales begin to dominate over shear: ***the range of R_ω appears to be limited when the roughness increases. However, we do not have enough data to establish if there exist an actual "threshold value" of roughness above which the spread of R_ω is limited"***

464: "Fig. 5" - why not refer to Fig. 3c showing the same? *We rephrased as "This interpretation is further confirmed by the patterns observed in both Figures 3 and 5:".*

465f: Need to define Richardson number and detail calculation. From a, b it is not immediately clear that delta slope approaching zero aligns with more critical (negative? why negative?) Richardson number events. *We now define the Richardson number in the text (R_i=N^2/[(∂u/∂z)^2+(∂v/∂z)^2]).*

Negative values come out because we use the log10 (this is now indicated in Figure caption):

print(np.log10(0.50)) => -0.3010299956639812

print(np.log10(0.30)) => -0.5228787452803376

print(np.log10(0.25)) => -0.6020599913279624

print(np.log10(0.10)) => -1.0

We agree that it is not immediately clear that delta slope approaching zero aligns with more critical Richardson number events (Fig. R5). However, this might be due to the blind color friendly chart.

The plot below actually confirms that “Ri values show their minimum values tending toward criticity with low values of delta slope (i.e., flat angles)”. We added this sentence in the text.

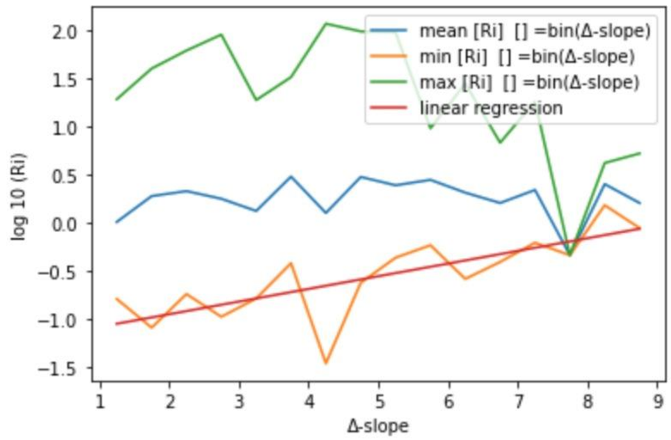


***Fig. R5.** Ri values as a function of Δ-slope*

466: This has been evident in previous figures as well! *We rephrased as: “Yet, the deviation from the GM model is evident in the shear ratio”*

468: What does vice-versa mean here? *We removed it. Thanks!*

469: What is diffusion rate of TKE? *In accordance with our answer to the major point #4, we removed results about diapycnal mixing and TKE.*

483ff: This whole paragraph is not needed here. *We removed it, also based on major point #4*

498: "Values of R_omega are necessary" - in the finescale parameterization. *Thanks, we added this.*

504ff: Please rephrase, this is incredibly hard to read and make sense of. *We rephrased as:*

“large (small) values of R_ω occur over areas with gentle (steep) topographic slopes and/or low (high) seafloor roughness. This supports the hypothesis that gentler slopes and smoother seafloor generate lower-frequency internal waves, whereas steeper and rougher topography favors higher-frequency waves, leading to a corresponding increase in the shear (strain) contribution.”

509f: I don't think you show this. *We removed this sentence.*

511: This whole paragraph is very hard to follow. *We rephrased as:*

“Our observations support the hypothesis that under rougher seafloor conditions, enhanced local shear leads to a stronger energy cascade and greater dissipation, which in turn disrupts the coherent vertical motions associated with internal waves (IWs). As a result, the strain spectra tend to be suppressed in these rougher areas. The observed imbalance — with elevated shear variance and reduced strain variance relative to trends seen in smoother conditions — reflects a wave field that is more turbulent and less coherent (Chinn et al., 2016)”

513f: I thought you show that roughness leads to low R_omega and thus high strain variance?

We thank the reviewer for pointing this out. The previous text indeed simply inverted the interpretation of our results. Our observations show that increased roughness is associated with lower Rw corresponding to enhanced strain variance relative to shear. The text has been corrected accordingly to reflect this interpretation:

"Our observations suggest that, in rougher conditions, the redistribution of internal wave energy toward higher frequencies and smaller vertical scales enhances strain variance relative to shear. The resulting decrease in Rω indicates a wave field that departs from low-frequency, shear-dominated regimes and becomes increasingly strain-dominated, consistent with enhanced spectral distortion over rough topography (Chinn et al., 2016)."

517: What does "Although" mean here? *Indeed, it was a typo. We removed it. Thanks!*

521: "shallow areas" - or rough? *Correct! We rephrased.*

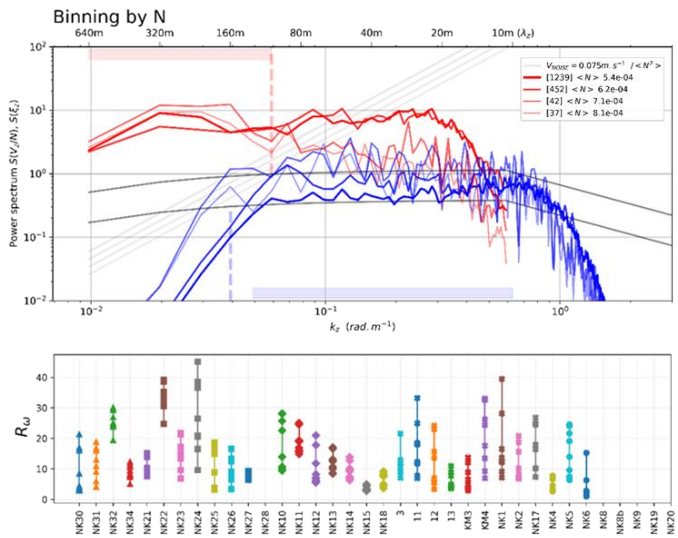
524: would -> could. *Done, thanks.*

526f: Please explain why this reference would be appropriate here - it does not appear to be on mesoscale eddies. *The reference is used to justify the term "nontraditional"*

530: Can you show this? *Our statement relies on the conservative precautions adopted in the spectral integration, with the shear integration range being explicitly limited using LADCP velocity error estimates. The observed shear spectra do not exhibit the typical signatures of noise-dominated variance. We have therefore rephrased the text to moderate the statement and clarify this point:*

"However, within the limits of our analysis, the shear spectra do not display clear indications of dominant noise contamination, given the conservative choices adopted for the integration bands."

Moreover, we acknowledge that the spectra in Fig. 1 (now in Fig. 4) of our original manuscript can be partly misleading because they include all water-column segments, including the shallow ones, which are more likely to contain smaller-scale variance from mesoscale contamination or double-diffusive layers. These segments are not used in our analysis: we focus exclusively on the deepest ~1000 m, and this "non-inclusion" is already visible in the spectra binned by roughness or Δ-slope that encompass only the deepest ~1000 m. To avoid any ambiguity, we revised the former Fig. 1b (see Fig. R4, which now shows only these analysis-relevant bins):



New Figure 1a,b. *(See caption in the main text).*

531: How do these lines connect to the previous? Should this be a new paragraph? *Indeed, it was a new paragraph but there was no indentation.*

542ff: How so? How do you know internal waves you observe have been generated by geostrophic currents? *The whole paragraph was removed, in accordance with the major point #4.*

546ff: Remove or be more specific. *The whole paragraph was removed, in accordance with the major point #4.*

553: What is "additional abyssal flow"? *We rephrased as: "may help deriving both dissipation rate of turbulent kinetic energy and diapycnal diffusivity in the finescale parameterization".*

553f: Unclear why all these references. Can you be more specific in how your results will help refine mixing parameterizations? *We rephrased as "may help deriving both dissipation rate of turbulent kinetic energy and diapycnal diffusivity in the finescale parameterization" and we removed these references.*

594: Mediterranean Sea -> Ionian Sea? *Correct! Many thanks!*

594: Please add some labels for those not familiar with the region. *Done*

596: What about the transect marked with circles? *Thanks. The transects are five. We corrected that in both caption and main text.*

596: I suggest moving 1b to be its own figure and adding a panel for bottom roughness, possibly also for bottom slope. *Done. See new Fig. 1 and Fig. 4.*

602: dotted -> dashed. *Done.*

609f: "low (high)..." - Leave this for results / discussion section. *Ok, we removed it.*

611: over the study area -> at the cast sites. *Done, thank you.*

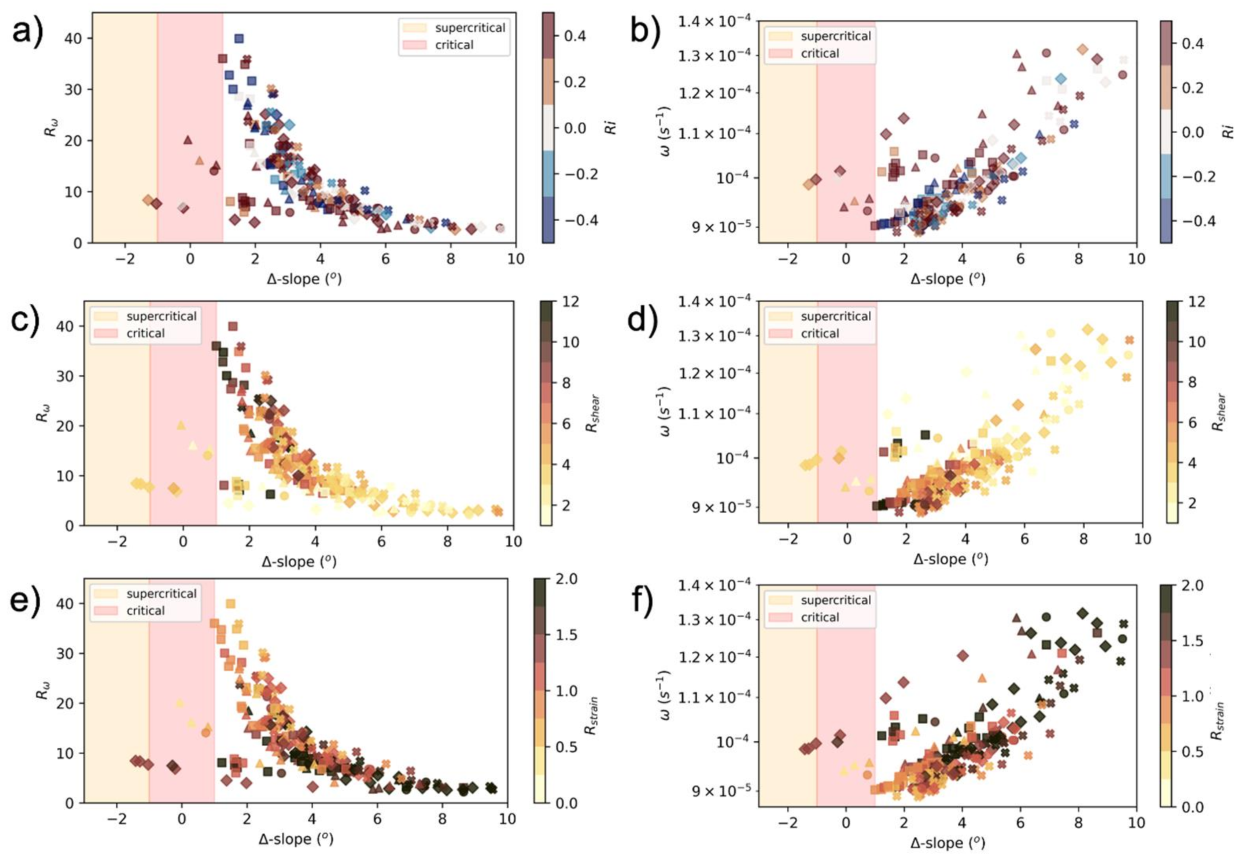
611: Remove "Scatterplot of". *Done, thank you.*

611: Here and elsewhere remove "z-axis" and call this "color". *Done.*

647: It seems you are averaging over less spectra here than in Fig. 1b. Why? What is the difference? *Figure 1b (now Figure 3a) is fully revised.*

690: Richardson number not defined. *It is now defined in the main text. See comment for line 465f.*

690f: Why is the ratio of observed to GM strain shown in log scale but shear not? *Thanks! it was just a typo in the label, it is not log10. See new Fig. 4*



New Fig. 5. (See caption in the main text).