Dear Editor Katsumata,

We would like to sincerely thank you for the opportunity to revise our manuscript. We appreciate the constructive feedback provided by both you and the reviewers. In this revised version, we have carefully addressed all comments raised during the review process, providing a point-by-point response to each comment. We hope that the revisions satisfactorily address all concerns and that the manuscript is now suitable for publication.

We thank you again for your time and consideration.

Answers to Editor Comments

[Editor Comment #1] As the authors have not yet replied to 14 specific comments from Reviewer #1, I believe the review process is not yet complete. It is imperative that the authors respond to ALL comments from the reviewer.

We here provide answers to the 14 specific comments. Please, note that we actually did not provide some of these answers by mistake, and we really apologize for that!

[start 14 comments]

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

Fig. 1 is now showing all the necessary toponyms [this was already done in the revised manuscript egusphere-2025-4762-manuscript-version3.pdf submitted on 15 February 2026];

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

We now include the circulation pattern on the map [see new Fig. 1].

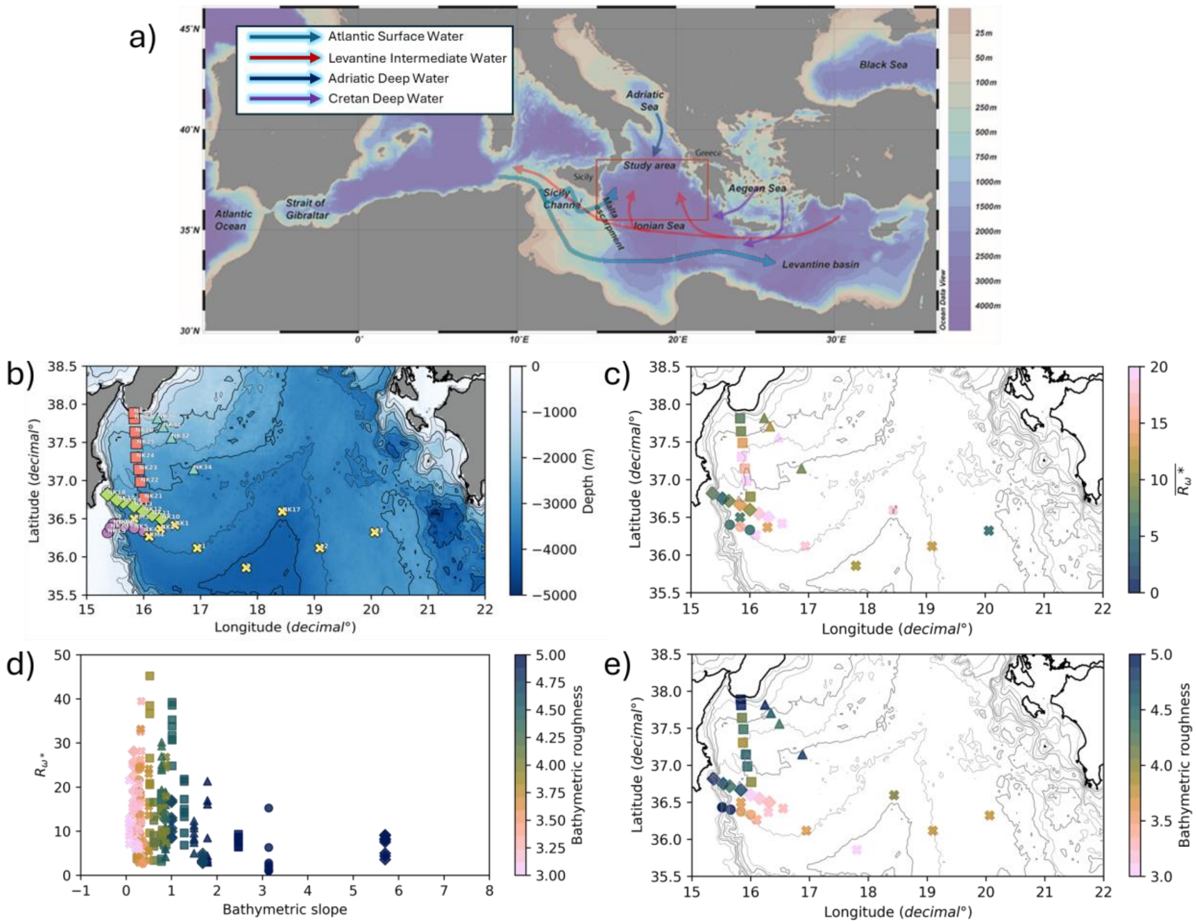

Figure 1. ***a)*** *The Mediterranean Sea bathymetry and the main regional features that are discussed within the text; the red box indicates the study area; the panel also shows the main subsurface circulation pattern of the eastern and central basins.* ***b)*** *The Ionian Sea bathymetry and the main regional features that are discussed within the text; LADCP-CTD casts are grouped in five main hydrological transects: four shelf-to-plain transects, i.e., T1 (▲), T2 (■), T3 (♦), T4(●), and one abyssal transect T5(✗).* ***c)*** *Shear-to-strain ratio R_{ω^*} at the bottom of the LADCP-CTD profiles, averaged from bins of 80m in the layer 360-920m distant from the bottom.* ***d)*** *Bottom roughness (log10 of var(z)) at the cast sites.* ***e)*** *R_{ω^*} as a function of bathymetric slope (x-axis), and roughness (in color).*

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

Thanks for this question that give us the opportunity to underline a specific characteristic of the Mediterranean circulation. There are several works that actually show that tidal flows in the study area (and, in general, in the Mediterranean Sea) are much weaker than the mean flow: "Compared to the adjacent Atlantic Ocean, the tidal signal in the Mediterranean Sea is relatively weak" (Sannino et al., 2015; https://doi.org/10.1016/j.pocean.2015.03.002). From van Haren, 2026 (arXiv:2601.03771): "Mediterranean tides are "limited" even at depth; Barotropic tidal signals are very small (order ~0.1 m surface elevation); Deep signals are detectable but

very weak and embedded in internal-wave noise". Based on this literature, we added the following parts in new version of the manuscript:

"Barotropic tides are relatively weak in the Mediterranean Sea and deep signals are embedded in internal-wave noise (Sannino et al., 2015; van Haren, 2026)." (lines 140-141)

"Deep current meter measurements collected in the westernmost part in the period 2007–2008 showed a quite energetic and impulsive abyssal thermohaline 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); these measurements confirmed that tides are "limited" even at depth (i.e., order of ~ 1 cm/s)." (lines 181-186)

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.

We now included a small paragraph that states (see lines 200-205): "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. In particular, our new analysis aims at providing a spatial distribution of shear-to-strain ratio rather than a temporal analysis carried out over a specific region (i.e., the deep area of the Ionian Sea) as in Artale et al. (2018)."

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.

Thanks for this suggestion, we changed the words accordingly (see lines 270-273): "When $R\omega > 3$, shear variance dominates, low-frequency internal waves contribute more strongly, and $\hbar_1$ decreases (thus reducing $\varepsilon_{i\omega}$), while $\hbar_2$ increases (thus increasing $\varepsilon_{i\omega}$). When $R\omega < 3$, strain variance dominates, high frequency internal waves contribute more to the ratio: $\hbar_1$ increases (thus increasing $\varepsilon_{i\omega}$), $\hbar$2 decreases (thus reducing $\varepsilon_{i\omega}$)."

L208-210: I cannot understand this argument. Please explain more.
We rephrased this part to avoid ambiguity (see lines 273-275): "This makes the shear-

based parameterizations more effective when $R\omega > 3$, and conversely the strain-based parameterizations are more effective when $R\omega < 3$ (Chinn et al. 2016)."

L249: 0.22, not 0.22N2

Thanks! This was actually corrected from version2 to version3.

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?

The 640 m refers to the length of the sliding window used to compute strain and shear spectra. These windows are shifted every 10 m, producing a set of overlapping local variance estimates. The 80 m does not correspond to the spectral segment length, but to a vertical binning applied to these estimates. This binning is used to reduce the variance of individual spectral estimates and to provide a more stable vertical structure, while retaining a vertical resolution smaller than the 640 m window length. The choice of 80 m represents a compromise: it is large enough to average several overlapping estimates (given the 10 m step), but small enough to preserve meaningful vertical gradients over the bottom ~1 km where variability is expected. We replaced the text as follows:

"Final variances are obtained by averaging these overlapping estimates within finite-sized vertical bins (here 80 m), which are independent from the 640 m spectral window length and chosen as a compromise between statistical stability and vertical resolution." (lines 326–328)

"To investigate the influence of bottom topography on internal wave shear-to-strain ratio from Equation (6), the variance estimates derived from the 640 m sliding windows are subsequently bin-averaged every 80 m from bottom to surface, in order to reduce the variance of individual estimates while preserving vertical structure." (lines 475–478)

L268-275: "from 640 to 107 m" It would be easier to understand if you rewrite them in rad/m
Thanks, we now added "from 640 to 107 m (i.e., from 0.01 to 0.06 rad/m)" (line 339) and "from 128 to 10 m (i.e., from 0.05 to 0.63 rad/m; the GM upper limit)" (line 345)

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

Thanks. We now modified accordingly. See lines 412-420.

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.

We agree with the reviewer. During reflection, the intrinsic frequency and propagation angle of internal waves are conserved by the dispersion relation, while the wavenumber can change through scattering processes. Our original formulation was imprecise. What we intend to describe is not a change in propagation angle, but a redistribution of energy across vertical wavenumbers depending on the slope criticality. We have clarified this point in the revised text. We rephrased the text as (500-504): "This angle controls the reflection regime: near-critical conditions (Δ-slope ≈ 0°) are associated with a redistribution of energy toward higher vertical wavenumbers, leading to enhanced vertical shear, while more supercritical conditions favor upward energy propagation and relatively stronger vertical displacements (strain) (Musgrave et al., 2022)."

L453: is this spectral increase statistically significant?
Yes, we specifically addressed this point by performing a dedicated statistical assessment of the spectral differences. We implemented a bootstrap resampling approach combined with permutation tests to evaluate whether the observed spectral increases are robust or could arise from sampling variability. In addition, we applied a False Discovery Rate (FDR) correction to account for multiple testing across wavenumbers. These analyses show that a significant fraction of the spectral differences (including the increase mentioned here) remains statistically robust. For clarity and transparency, we now provide the full significance analysis, including confidence intervals and p-values across spectral bands in Supplementary Material.

Figure R1 (see below) compares representative shear and strain spectra between adjacent Δ-slope classes and evaluates whether the observed spectral differences remain robust under resampling (see also Supplementary Material for full analysis and explanation). The right panels show the logarithmic spectral differences together with bootstrap confidence intervals and FDR-corrected significant wavenumbers, highlighting with green points spectral components where the differences are statistically distinguishable from sampling variability. The figure demonstrates that the observed changes are not randomly distributed across scales but correspond to coherent and statistically robust reorganizations of the internal wave spectral structure across Δ-slope regimes.

To address this point, we add the following sentence in the main text (see lines 523-526): "These increases in both shear and strain energy levels are statistically robust, as

confirmed by a dedicated bootstrap and permutation-based significance analysis (see Supplementary Material), showing coherent variability across configurations and no systematic dependence on bandwidth selection”

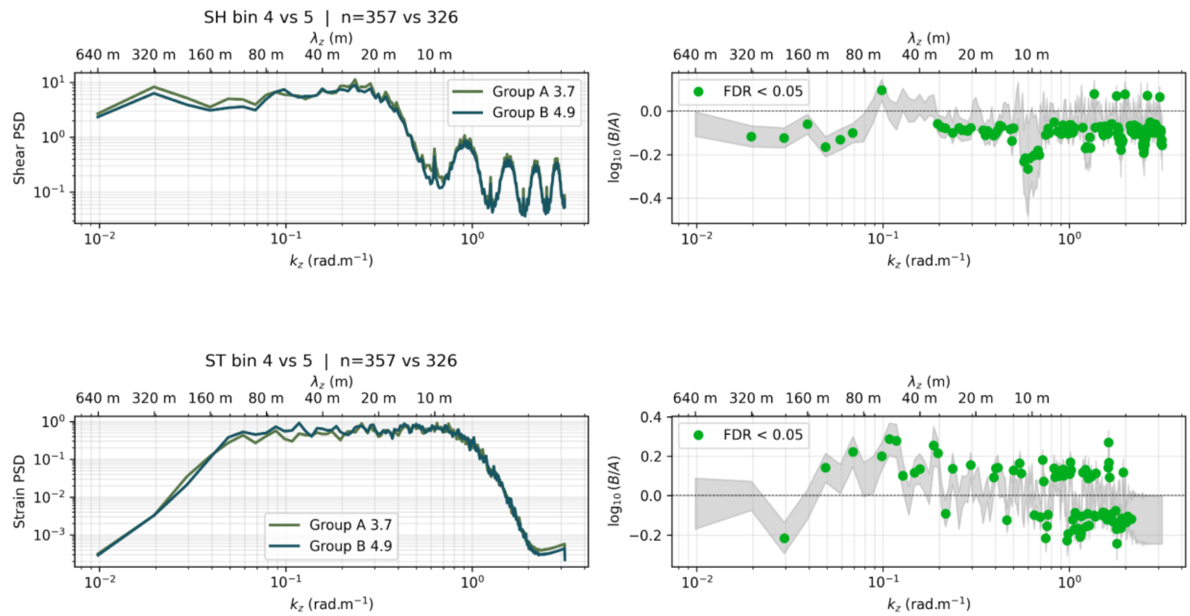

Fig. R1 (extracted from Fig. S1)*. Representative statistical comparisons between adjacent Δ-slope bins (bin 4 vs 5) for shear spectra (top panels) and strain spectra (bottom panels). Left panels show the mean spectra for the two groups, while right panels show the logarithmic spectral difference together with bootstrap confidence intervals (grey shading) and FDR-corrected significant wavenumbers (green markers). The observed spectral differences are statistically robust and unlikely to arise from sampling variability alone. The strain spectra display a more coherent spectral organization than the shear spectra across the considered Δ-slope classes. For more details see Supplementary Material.*

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.

Lines 469-480 in version2 have been removed in version3 as a consequence of Reviewer #2's main point #4 which resulted in a complete overhaul of results related to diapycnal mixing (see egusphere-2025-4762-author_response-version2.pdf uploaded on 15 Feb 2026).

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

We rephrase as follows (see lines 565-569): "Our observations suggest that, in rougher conditions, internal wave energy is redistributed toward smaller vertical scales (higher vertical wavenumbers), leading to an enhancement of strain variance relative to shear. The resulting decrease in $R\omega$ indicates a wave field that departs from shear-dominated

regimes and becomes increasingly strain-dominated, consistent with enhanced spectral distortion over rough topography (Chinn et al., 2016)".

Please note that the intrinsic frequency and propagation angle of individual waves remain unchanged. Instead, rough topography may redistribute energy toward higher vertical wavenumbers, leading to a more strain-dominated variance partition within the wave field. In a GM-like energetic partition, such conditions may resemble those associated with higher-frequency-dominated wave fields, without implying an actual increase in wave frequency.

[end of the 14 comments]

[Editor comment #2] Please also be advised that the reviewer explicitly states that the responses (egusphere-2025-4762-authors_response-version2.pdf) did not adequately address most of the major comments. I would like to emphasize the seriousness of the last major point. In the responses, the caption to Fig.R5 states that "Strain spectra were lowpass filtered at high wavenumbers (cutoff at 10 m) to remove potential noise contamination." Fig.R5 appears very similar to Fig.3a in the revised manuscript (version 3), but I could not find any indication of the use of a low-pass filter. Should the filter have been applied surreptitiously, it is difficult to believe other figures -- we have an ethical issue here.

We thank the Editor for raising this important point. We agree that the current presentation was ambiguous and required clarification. We would like to emphasize that no undisclosed or "surreptitious" filtering was applied to the analysis presented in the manuscript.

A low-pass filter at 10 m was initially introduced only as an additional sensitivity test in the rebuttal, specifically to address the reviewer's concern regarding possible high-wavenumber noise contamination in the strain spectra. This filtering was explicitly indicated in the caption of Fig. R5 ("Strain spectra were low-pass filtered at high wavenumbers (cutoff at 10 m)...") of the Author's Response "egusphere-2025-4762-author_response-version2.pdf" (uploaded on 15 Feb 2026).

However, we acknowledge that this information was not sufficiently reiterated in the main text of the response, which may have led to confusion regarding its scope and role in the analysis. In the revised manuscript (version 3), the filtered spectra were retained in Fig. 3, while the corresponding methodological description was not updated consistently. This created an inconsistency between the processing applied to the displayed spectra and the associated documentation, which we acknowledge and apologize for.

Importantly, the filtering was not part of the core R_{ω^*} calculation itself. Wavelengths shorter than ~10 m are already excluded from the R_{ω^*} computation through the spectral integration limits (0.1 cpm cutoff). The purpose of the low-pass filter was therefore only to demonstrate that potential high-wavenumber noise contamination does not significantly modify the overall spectral structure or the resulting R_{ω^*} distributions, aside from a slight amplitude adjustment at the highest resolved wavenumbers.

A compact comparison between filtered and unfiltered spectra in [0-960] m from bottom is included below (Fig. R2 of this rebuttal) to explicitly demonstrate that the differences are confined to wavelengths shorter than ~10 m, i.e. outside the integration range used for the R_{ω^*} calculation itself.

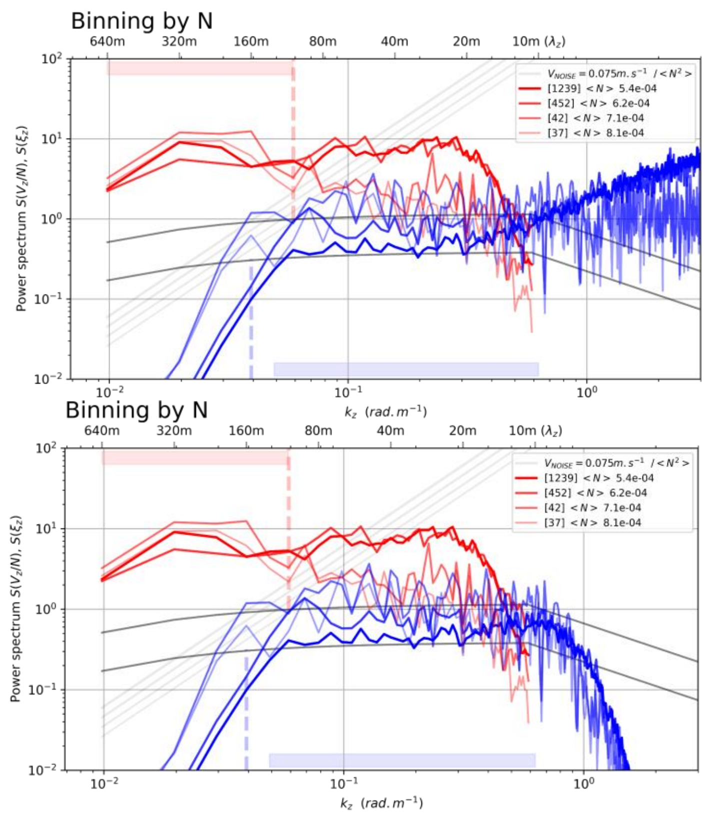

Fig. R2. *Comparison between unfiltered (top) and low-pass filtered (bottom) [0 - 960m] from bottom spectra. The filtering only affects the highest vertical wavenumbers corresponding to wavelengths shorter than ~10 m, while the spectral ranges used for the R_{ω^*} computation remain unchanged. This demonstrates that the filtering does not alter the structure of the spectra within the integration bands used in the analysis.*

We now rephrased caption of Figure 3 as follow: "Although wavelengths shorter than 10m are already excluded, an additional low-pass filtering is applied to prevent the potential inclusion of noise leaking". A similar sentence is added in the main text (see line 528-530).

This clarification ensures full transparency in the data processing and resolves the ambiguity identified by the Editor.

[Editor comment #3] In the revised version, please ensure that the authors' responses address the specific scientific issue raised by the reviewer. For instance, in response to the second comment regarding β and the 'single frequency' assumption, the authors initiated a discussion on the merits of the Δ-slope plot. I do not believe that it responds to the reviewer's request for justification of the use of single frequency. Merits do not overwrite physics.

We thank the Editor for this clarification and agree that our previous response did not directly address the physical implications of the "single-frequency" assumption. We clarify that the shear-to-strain ratio used here is constructed from variance integrated over finite, and in our reference case distinct, wavenumber bands for shear and strain. It therefore represents a broadband statistical descriptor of the internal wave field rather than a single-frequency quantity (See lines 353-356; 436-443).

Moreover, to address the robustness of this approach, we now provide in the Supplementary Material a dedicated sensitivity analysis using alternative bandwidth configurations and statistical comparisons between configurations. These results support that the diagnostic reflects a coherent property of the wave field rather than a specific spectral choice.

Accordingly, we no longer interpret this quantity in terms of true wave frequency, and a more detailed response is provided below in Response to "Single-frequency assumption", i.e., Editor comment #6.

[Editor comment #4] It is no longer necessary to reply to Reviewer #2. I would like to express my gratitude to the authors for their rebuttal letter, which has been instrumental in clarifying the various perspectives we hold. I would like to raise the following points in response. In the next revision, please give due consideration to the following comments. The line numbers refer to those on egusphere-2025-4762-manuscript-version3.pdf. C16 and RL refer to Chinn et al. (2016) and the rebuttal letter, respectively.
Many thanks. See our answers below.

[Editor comment #5] The parameter "Rw".
As the fourth paragraph of RL shows, the parameter "Rw" differs from R_ω (LaTeX notation). The former "Rw" is a new quantity introduced in this manuscript and its physical meaning needs to be clearly explained. The latter R_ω has a clear physical meaning (shear-to-strain ratio of internal wave as superposition of linear waves (Eq.(10) of C16) or the frequency ratio (Eq.(6) of C16) if interpreted as a single wave (explicitly noted before Eq.(6) on C16)).
I even find it confusing to use the symbol Rw for a distinct quantity from R_ω and would recommend using a different symbol.
It is evident that "Rw" is employed as a "statistical" parameter here ("designed as statistical exploration", last paragraph RL), however its physical significance remains ambiguous. As illustrated in Eq.(6), the ratio is calculated as
'shear relative to GM from waves with wavelengths between 107 and 640 m'
divided by
'strain relative to GM from waves with wavelengths between 10 and 128 m'.
Despite gauging shear and strain against GM, the authors state that they do not intend to characterise departures from GM itself (4th paragraph RL).

In this sense, Fig. 3 was an interesting attempt to characterise the internal wave field ("low-pass filter" problem aside), and it has potential to be physically fruitful.
It may be possible to justify the use of "Rw" as a proxy for R_ω using a scientific argument based on these figures. However, I am unable to interpret Figs. 2 and 4.
It is essential to clarify how one can "diagnose bathymetric effects on the internal wave field from variance estimates" (4th paragraph RL).
It appears unfeasible to estimate the local energy level of the IW field and to modulate a canonical value of the dissipation rate of turbulent kinetic energy using "Rw" without first defining its physical meaning. (L.208-) The same remark applies to L.365. It is necessary to explain why this "Rw" is being used as a statistical parameter but compared to GM's canonical value of 3 (see L.463).
The quantity "Rw" is not significantly related to the finescale parameterisation of the dissipation rate. It appears that Section 2.2 is not appropriate in this particular context. As stated in RL, 'interpreting estimates of ε or K (which is not our objective)' is not the objective of this project.

We thank the Editor for this important comment. We agree that the use of the notation "Rw" was confusing and that its physical meaning was not sufficiently defined.

We now clarify that the quantity used in this study is not the classical Rω. It is constructed from variance integrated over finite and distinct wavenumber bands for shear and strain, each integration band containing contributions from multiple wave components with different frequencies and propagation geometries. Consequently, the resulting ratio cannot, in general, be associated with a unique wave frequency or interpreted within a monochromatic internal-wave framework. R_ω should be interpreted as a broadband statistical descriptor of the internal wave field, reflecting the partition between shear and strain variance across scales. It does not represent a dynamical parameter and is not used within a finescale parameterisation framework.

To justify its use, we now provide in the Supplementary Material (see also Fig. R3 of this rebuttal) a dedicated sensitivity analysis of a representative spectral shear-to-strain ratio, including multiple bandwidth configurations (also including symmetric bands) and statistical comparisons (pairwise Spearman correlations and ordering tests across all profiles). These results show coherent variability across configurations (mean Spearman ρ ≈ 0.75) and no systematic ordering between subsets, indicating that R_{ω^*} captures a robust property of the wave field rather than an artefact of the spectral choices.

In the main text we now include the following sentences:

"To avoid confusion with the classical monochromatic internal-wave framework, we introduce the notation R_{ω^*}, ω*, and β*, to denote the broadband statistical quantities derived from the shear–strain variance partition used in this study." (see lines 440-443).

"We note that our definition of R_{ω^*} is meant to give a statistical representation of the classical (single-wave frequency) R_ω, usually found in literature (e.g., Kunze et al., 2006; Polzin et al., 2014; Ijichi and Hibiya, 2015, 2017)." (see lines 551-553).

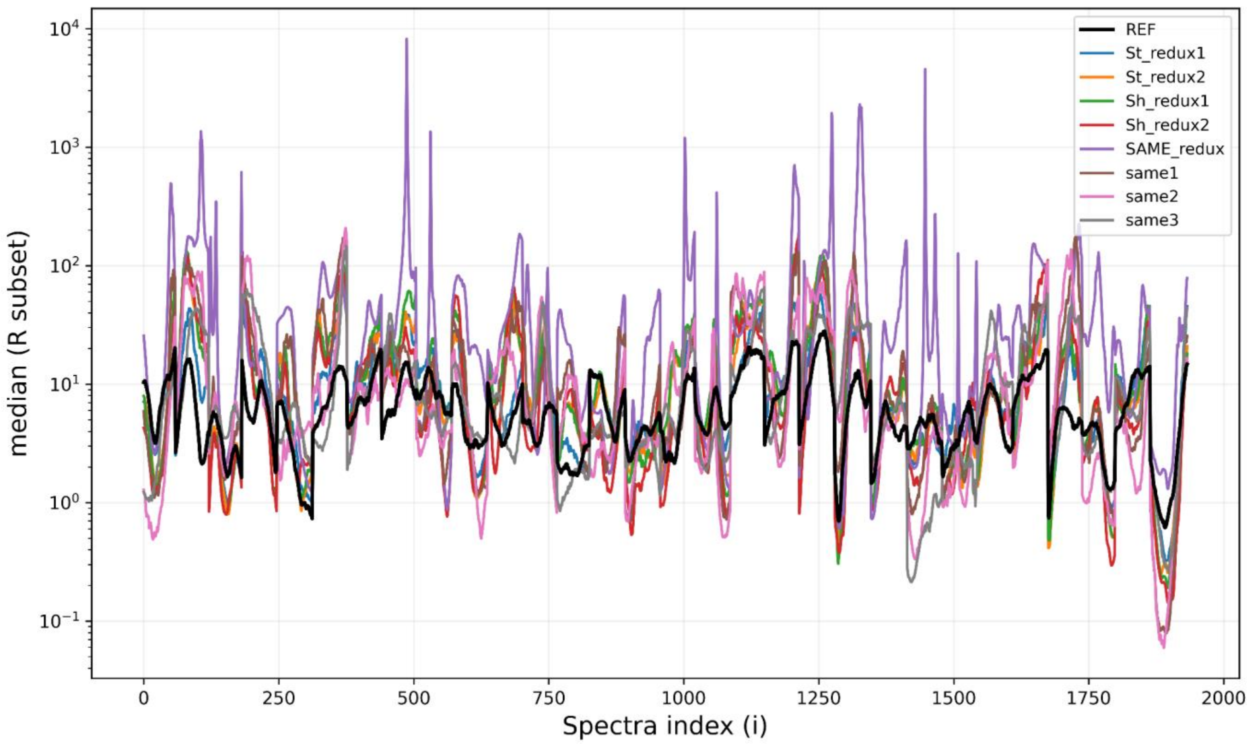

Fig. R3 (from Fig. S3 of the Supplementary Material). *Median of R ratio across spectral subsets (see Supplementary Material). Each curve represents the median value computed independently for a given spectral subset configuration (e.g., reduced or same-band integrations; Table S3, see Supplementary Material), plotted as a function of spectrum index i. The comparison highlights the robustness and variability of the shear-to-strain ratio across different choices of shear and strain integration ranges.*

	REF	St_redux1	St_redux2	Sh_redux1	Sh_redux2	SAME_redux	same1	same2	same3
REF	1.000000	0.873265	0.756037	0.766193	0.693221	0.541860	0.607333	0.470302	0.623284
St_redux1	0.873265	1.000000	0.891168	0.887453	0.831415	0.629050	0.778478	0.667198	0.799898
St_redux2	0.756037	0.891168	1.000000	0.985939	0.918206	0.686587	0.871515	0.796759	0.701547
Sh_redux1	0.766193	0.887453	0.985939	1.000000	0.927091	0.688554	0.869138	0.798202	0.703855
Sh_redux2	0.693221	0.831415	0.918206	0.927091	1.000000	0.755501	0.909747	0.786599	0.660321
SAME_redux	0.541860	0.629050	0.686587	0.688554	0.755501	1.000000	0.765231	0.521862	0.492342
same1	0.607333	0.778478	0.871515	0.869138	0.909747	0.765231	1.000000	0.864716	0.722900
same2	0.470302	0.667198	0.796759	0.798202	0.786599	0.521862	0.864716	1.000000	0.775101
same3	0.623284	0.799898	0.701547	0.703855	0.660321	0.492342	0.722900	0.775101	1.000000

Table R1 (from Table S4 of the Supplementary Material). *Pairwise Spearman rank correlations between R ratio estimates obtained from the different spectral bandwidth configurations described in Table S3 (see Supplementary Material). High correlation values indicate that the relative organization and variability of the diagnostic remain largely consistent across bandwidth choices, supporting the robustness of the observed patterns against changes in spectral integration ranges.*

We also clarify that the normalization by the Garrett–Munk (GM) spectrum is used only as a scaling reference to obtain non-dimensional quantities. We do not interpret deviations from GM, nor do we aim to estimate dissipation rates. In that sense, we agree that the original formulation could be misleading, and we will revise Section 2.2 accordingly to remove any unintended link with finescale parameterisation.

[Editor comment #6] Single-frequency assumption
This issue has also been raised by Reviewer #1 (second major point). In RL, the authors state in paragraph 2 that "Rw estimates ... capture coherent large-scale variations in the internal wave frequency content". However, in order to achieve this objective, it appears that the second equality in Eq.(11) in L.418 must be satisfied. This is precisely the single-frequency assumption. Without the single frequency assumption, the ratio of kinetic to potential energy is calculated as follows:

$$Rw = (u^2 + v^2) / (b^2 / N^2) = \{\int a^2(\omega) \cos^2 \phi(\omega) (\omega^2 + f^2) / \omega^2 \, d\omega\} / \{\int a^2(\omega) \sin^2 \phi(\omega) N^2 / \omega^2 \, d\omega\} \quad (\#)$$

where (u, v) are the horizontal velocities, b is the buoyancy and N^2 is the buoyancy frequency, $a^2(\omega)$ is the amplitude as function of frequency ω, and ϕ is the heading angle of the wavenumber vector from the horizontal. Please note that the integration frequencies will be different for the denominator and numerator of "Rw" in this manuscript because different wavenumbers were used for the kinetic and potential energies (see Eq. (6), L.342). If the spectrum of $a^2(\omega)$ has a clear peak, one can approximate the integration around the peak. This will result in the right-hand side being approximated as $(\omega^2 + f^2) (N^2 - \omega^2) / (\omega^2 - f^2) / N^2$. Assuming $\omega^2 \ll N^2$, this is the second equality in Eq.(11) in L.418 of the manuscript. If the spectrum of a^2 does not display a clear peak, however, I am unable to see how the "coherent large-scale variations" in "Rw" can be connected to "internal wave frequency content", particularly when the integration ranges of the denominator and numerator are different in equation (#). It is not possible to take the integration out of the equation.
It is evident that the arguments presented in the following places all require the single-frequency assumption (L.422-, L.442-, L.472-, L.470-, Fig.2a, Fig.2c, L.486-, L.513-, L.518-, and so on).

We thank the Editor for this clarification and agree that our previous wording did not sufficiently distinguish between a broadband statistical diagnostic and a monochromatic wave interpretation. We believe that this point is now fully addressed. See answers to Editor comment #3 and #5.

We further clarify that the shear-to-strain ratio used in this study is not based on a monochromatic internal wave field. Instead, it is constructed from variance integrated over finite and, in our reference configuration, distinct wavenumber bands for shear and strain. As such, it represents a broadband statistical measure of the internal wave field rather than a single-frequency quantity.

In particular, the second equality in Eq. (10) is not valid without the assumption of a narrowband spectrum, which we do not impose.

R_ω (now $R_{\omega*}$) is not used to infer "internal wave frequency content". Instead, it reflects the integrated contribution of a distribution of wave components, as highlighted by the Editor, and is interpreted as a statistical measure of the structure of the internal wave field.

To support this revised interpretation, the Supplementary Material now provides a systematic sensitivity analysis of the ratio to bandwidth selection, including configurations with identical shear and strain bands, progressively reduced bands, and symmetric configurations. The resulting representative ratio R estimates (see Supplementary Material) display coherent variability across configurations (mean pairwise Spearman correlation $\rho \approx 0.75$), with no systematic ordering between subsets, indicating that the diagnostic reflects a consistent large-scale organization of the observed variance partition despite its broadband nature.

The dispersion relation is therefore used here only as a geometric mapping to define an effective slope parameter (β^*), providing an empirical interpretation of the observed variance partition rather than a dynamical inference based on monochromatic wave theory.

The manuscript is revised to clarify this broadband interpretation and avoid unintended implications of a single-frequency wave description. To avoid confusion, we now adopt a distinct notation, renaming R_ω, ω and β, as $R_{\omega*}$, ω^* and β^*, respectively, throughout the manuscript (see also answer to the previous comments).

[Editor comment #7, from the 27 March 2026 decision]. The first point the reviewer raised was the effect of lee-wave generation. In reply, the authors introduced the Bell's linear theory to come up with a conversion rate of 1E-5 to 1E-3 W/m2. The authors called this "small values" but it was not clear to what number this should be compared. Typical kinetic energy dissipation rate in the ocean is 1E-10 to 1E-8 W/m3, which becomes comparable if integrated in a 1000m-high water column, which does not appear small. Even if these lee-waves are "small", these are propagating internal waves and possibly contribute to the shear and strain to affect R_ω. It still seems necessary to demonstrate that whatever the wave field the authors are assuming is much stronger than these lee waves.

We estimated a characteristic topographic forcing frequency as $\omega = |U|\, k_{eff}$, where the effective horizontal wavenumber $k_{eff} = 2\pi\, s / H_{rms}$ is estimated with s, i.e., the bathymetrical slope, and H_{rms} is the root-mean-square height of the detrended bathymetry. The velocity |U| and stratification were taken from the observed profiles and the resulting ω was normalized by N and f, and referenced to distance above the bottom and averaged in bins of 5m.

In Figure S4 (see Supplementary Material) the normalized values of ω /N remain low, typically between 0 and ~0.2–0.25, indicating that the diagnosed frequencies are well below the buoyancy frequency. In contrast, ω/f shows a broader distribution, but most values remain *below 1, i.e. in the sub-inertial regime, with only a few localized cases approaching or slightly exceeding unity.*

Overall, these results indicate that the mean-flow/topography interaction does not fall within the dynamical regime required for lee-wave generation, and therefore lee waves are unlikely to be the dominant mechanism controlling the observed variability.