For Anonymous referee #1

The reviewer deserves our thanks for the comments, remarks, and suggestions on our manuscript and appreciate the time they dedicated to this review. Below (highlighted in blue and magenta) is an itemized response to the different issues raised in the review.

This is a good paper with solid analysis. It builds on a good framework of previous studies, and presents interesting new results.

Some of the discussion is a bit over-simplified. The pattern of energy transfer terms (Fig 5.6,) are complex and it is hard to know what to focus on. Features and 'results' highlighted not always as clear as the text suggests (e.g., most of what is emphasized in the discussion of Fig 6 can also be seen in Fig 5 - see comments on L473-475).

R: We thank the reviewer for this comment and the associated comments on L473 and L475, that helped us to be more precise in our analysis. The energy transfer terms are presented for each of the three selected cases with the specific intent of illustrating how the direction and magnitude of energy transfers are modulated by the presence and position of mesoscale eddies. We agree that the pattern of these terms is complex, and we have carefully verified and updated the results for local terms Cmn and Hmn, as well as the associated comments (in lines 451-456; 460-465; 476; 526-536; 549; 575-578; 585-594; 606 of the revised manuscript). We have revised the discussion of figures 5, 6 and 7 to more clearly guide the reader toward the key features and differences across cases, in order to avoid any ambiguity about what is being highlighted.

In addition to the changing mesoscale field, it is worth noting that the IT also changes quite a bit between the 2nd and 3rd cases - the forcing is about 50% larger! I don't think one can really ignore this and state that the IT fields are the same (L332). What are the impacts of a stronger IT (if the eddy field stayed the same)? I presume that this is part of the signal seen in Fig 7 (vs Fig 6).

R: We take note of and sincerely thank the reviewer for this important remark. We agree that the internal tide (IT) forcing is not strictly identical between the 2nd and 3rd cases, and we acknowledge that the statement at L332 was too strong. We have revised this sentence accordingly.
However, we wish to respectfully qualify the estimate of an approximately 50% difference in forcing. Examining the SSH amplitudes in Figure 3 between the two cases (~1.1 m for CEC and ~1.3 m for CEE), the difference is closer to ~18%, which is more modest than suggested.
We agree this difference should not be ignored. A stronger IT forcing, with the mesoscale field held constant, would be expected to increase IT energy and potentially enhance vertical mixing and energy fluxes. However, given the relatively small amplitude difference, this effect is expected to be secondary. We argue that the dominant signal observed in Fig. 7 compared to Fig. 6 remains primarily attributable to the contrasting mesoscale conditions between the cases, rather than to the modest change in tidal forcing.
To clarify this point, we have added the following sentence in lines 351-353 of the revised manuscript:
"

Although the tidal forcing is not strictly identical across the three cases, the differences in tidal amplitude remain small (less than 18%) and are therefore considered secondary compared to the large contrasts in mesoscale conditions between the cases.
"

The analysis is well done, and the results very interesting, although arguably pretty complicated. It could be good to build composites (for all cases when a CE is on top of the seamount, for example, or to the north of it, or...), or try to quantify the dominant terms in a control region over multiple realizations, or ... Overall, the manuscript leaves many questions open but still is a solid piece of work that might inspire other studies. As such, I recommend publication, with authors reading and thinking about my comments.

R: We humbly thank the reviewer for sharing this interesting suggestion.

We acknowledge that building composites or quantifying dominant terms over multiple realizations would be a valuable approach. However, it is one year and half more work to assess and quantify all these statistical events. The present study acts as a first step toward this quantification. It is specifically designed as a process-oriented investigation addressing three targeted questions: (1) whether the IT propagates freely, deviates, or becomes trapped by mesoscale features; (2) whether these outcomes depend on the IT's vertical mode, or on the location of the eddy encounters along with the associated background conditions (currents and stratification); and (3) what the synergistic roles of topography and cyclonic eddies are in governing modal energy transfers. In this framework, the three selected cases are chosen as qualitative configurations to dissect these interactions through vertical mode projection and intermodal energy transfer analysis. A systematic statistical analysis — such as composite maps or frequency-of-occurrence diagnostics — is scientifically very relevant and will be done in a second study.
We have added a sentence explicitly acknowledging this as a natural and promising direction for future work (lines 771-775 of the revised manuscript), as follows:

"
Limiting our analysis to three case studies reflects the primarily qualitative nature of our approach. A natural next step would be to extend it toward more quantitative results by conducting composite analyses over a larger set of eddy–IT interaction cases. Grouping configurations by eddy position relative to the seamount, for instance, would allow the IT response to mesoscale variability to be characterized in a statistically robust way.
"

Figure 1 - Cyan contour should not be used for Ceará Rise seamount. In the rest of the paper, cyan is used to mark eddies. It would be better to mark the seamount by a different color (thick black?), and repeat it in later figures (Fig 2,4) where the seamount is hard to locate.

R: We welcome and appreciate the reviewer's suggestion. We agree that using cyan for the seamount contour is inconsistent with its use for eddies in fig. 4. However, since black is already used for velocity and energy flux arrows, white and red/blue for vorticity and CE/AE fields, yellow-red-black for EKE amplitude, and magenta for the Mid-Atlantic Ridge, we have used a thick green contour to mark the seamount in all figures.

L332 and Figure 3 - Magnitude of the internal tide for the 3rd case is much larger.

R: We thank the reviewer for this comment. This point is addressed in our response to the previous comment regarding the selection of the three cases.

Figure 4 - it might be worth stating that while the conditions for the 3rd case seem very similar to the second case, the energetics (discussed later) are very different. Looking at Fig 4, I was wondering why there was a 3rd case.

R: We thank the reviewer for this comment. We have added a comparison of the background conditions (currents and stratification) along the IT propagation paths from sites A and D across the three cases (NE, CEC, and CEE). This comparison shows that the key distinction between the eddy cases lies not only in where the IT beam encounters the eddy (eddy core vs. eddy edge), but also in the associated background conditions. This is illustrated in lines 368-383 of the revised manuscript. In our study, the three cases are qualitatively distinguished by the presence or absence of a cyclonic eddy (CE), and, when a CE is present, by the geometry of the IT–CE intersection. We have also added a quantitative definition of the CE "core" and CE "edge" in terms of normalized distance from the eddy centroid, in lines 384-389 of the revised manuscript..

L338-343: Information about the seamount is scattered in the first two items. Combine in a new sentence before listing the 3 cases (L335).

R: We extend our sincere thanks to the reviewer for this suggestion. We agree that the information about the seamount was scattered across the case descriptions. We have reorganized the text by adding an introductory sentence before the list of cases that gathers all the topographic information (Ceará Rise/seamount location, dimensions, and distance to the Mid-Atlantic Ridge) in one place. This reorganization is mentioned in the lines 355-358 of the revised manuscript as follows:

"

The three selected cases are located in a region shaped by two major topographic features: the Ceará Rise seamount (~500 km from sites A and D; between 4°N–6°N, 45°W–42.5°W), with an amplitude (hmax) of ~1000 m and a width (wmax) of ~100 km, and the Mid-Atlantic Ridge (~1100 km from sites A and D).

"

L473: "In the CEC case, the net effect of Hmn (Figs. 6g-i) is primarily governed by its symmetric part (Figs. 6m-o)." This is a bit of an overstatement. Like in the previous case (Fig. 5), I would argue that it is 'clear' that H^A_{1-3} dominates H_{1-3} and H^S_{2-3} dominates H_{2-3}, but it's hard to immediately decide if A or S dominates for H_{1-2}. In this case, its's really a combination of both. The symmetric component plays a bigger role than it did in the previous case.

R: We thank the reviewer for this comment that will help us to be more precise and clearer. We have carefully verified and updated the results for local terms C_{mn} and H_{mn}, as well as the associated comments for figures 6, 7 and 8 (in line 451-456; 460-465; 476; 526-536; 549; 575-578; 585-594; 606 of the revised manuscript). We have revised L473 to make this distinction more explicit and to avoid any overstatement regarding the overall dominance of the symmetric component.

L475. "Specifically, between the shelf break and the southern edge of the CE, Mode-2 IT loses energy to the Mode-3 background flow (Fig. 6o, blue patches)." That statement is also true for the previous case (Fig 5o). Maybe it is stronger here, but it's really hard to see with this (saturated) colorbar.

R: We recognize and appreciate the reviewer's observation.
We agree that this signal was also visible in Fig. 5o for the NE case. We have carefully verified and updated the results for local terms C_{mn} and H_{mn}, as well as the associated comments (in line 451-456; 460-465; 476; 526-536; 549; 575-578; 585-594; 606 of the revised manuscript). We have revised L475 to explicitly acknowledge any similarity and difference between the cases.
Regarding the saturated colorbar, the colorbar range and the spatial smoothing were deliberately chosen as a compromise to highlight specific features in panels of figures without losing meaningful information. We have added a sentence in the figure caption to explicitly note that the colorbar is saturated to aid the visualization of energy transfer. This is mentioned in lines 488-489 of the revised manuscript as follow:

"

It should be noted that the colorbar range is saturated in panels to enhance the visibility of energy transfer features..

"

For revised terms *Cmn* and *Hmn*, we have integrated the budget over a clearly defined interaction box in all three cases and presented it below in Figure in figure RC1.1-3. We did not find it necessary to include them in the manuscript, as the existing results already support our main conclusions:
In the NE case, the term Cmn is dominated overall by a forward energy cascade between IT modes, both upstream of (Figs. RC1.1c, e) and directly over (Figs. RC1.1b, d) the seamount. For the term Hmn, its symmetric part dominates overall in these same regions (Figs. RC1.1f–i). This indicates an interplay between Cmn and Hmn in the NE case.
In the CEC case, the term Cmn is dominated by an inverse energy cascade between IT modes near the CE core and seamount (Figs. RC1.2c, d). For the term Hmn, its symmetric part likewise dominates overall in these regions (Figs. RC1.2f–i), indicating a similar interplay between Cmn and Hmn as in the NE case.
In the CEE case, both forward and inverse energy transfers coexist for the term Cmn near the CE edge and seamount (Figs. RC1.3b–e). For the term Hmn, its symmetric part again dominates overall in these regions (Figs. RC1.3f–i), indicating a similar interplay between Cmn and Hmn as in the CEC case.

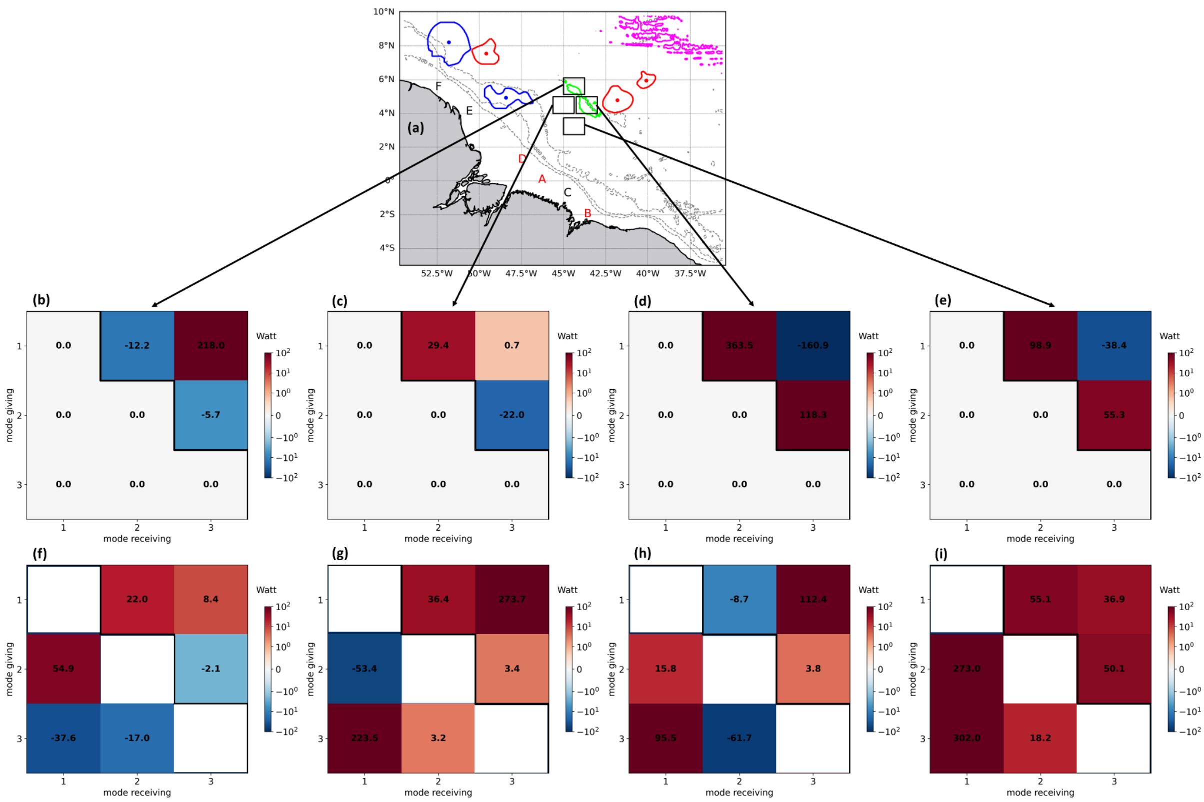

Figure RC1.1. Matrices of energy couplings integrated (in Watt) over a defined interaction box, where each matrix entry represents the coupling between a row mode and a column mode, for the NE case. A positive (negative) value indicates a net energy transfer from the row mode to the column mode (and vice versa). The upper triangle shows the anti-symmetric part of the energy couplings, while the lower triangle and diagonal show the symmetric part. Panel (a) shows the spatial extent of the interaction box, along with the detected eddy edges (blue and red contours). Panels (b–e) show the topographic scattering and stratification term (Cmn; color shading). Panels (f–i) show the horizontal shear coupling term (Hmn; color shading).

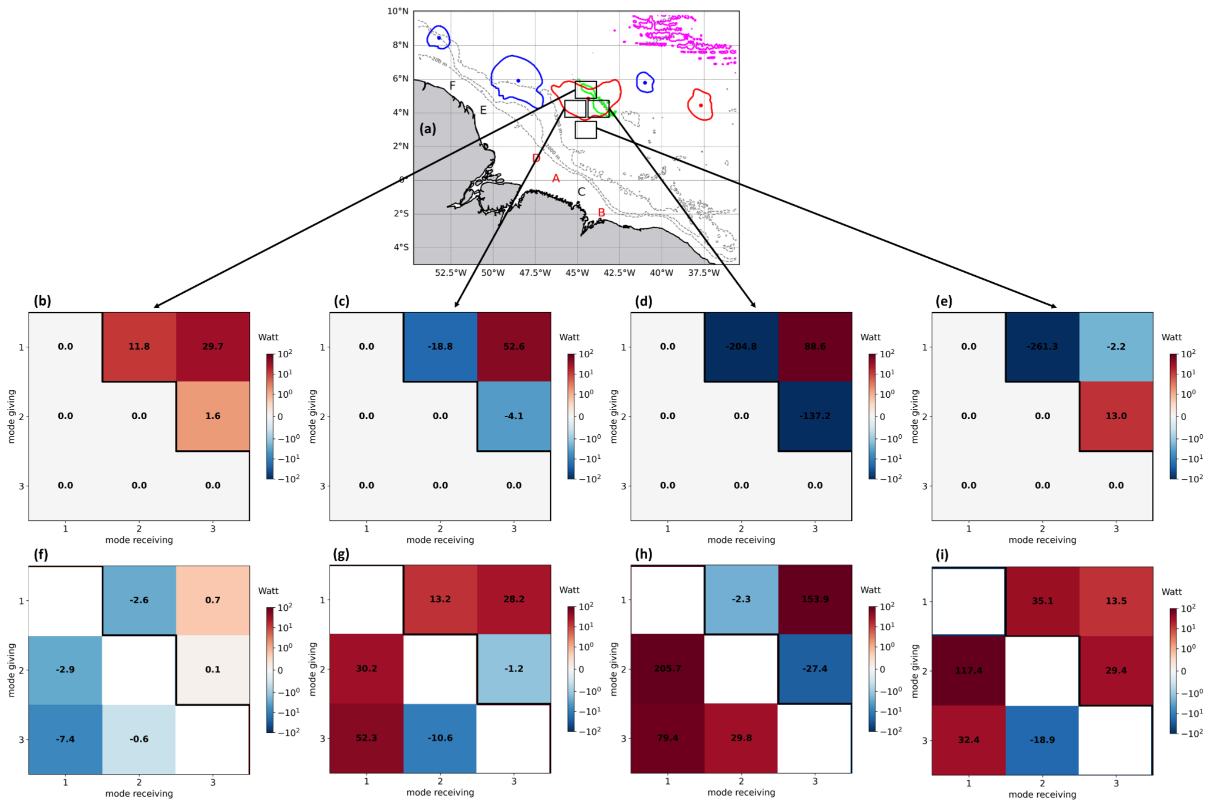

Figure RC1.2. Matrices of energy couplings integrated over a defined interaction box, where each matrix entry represents the coupling between a row mode and a column mode, for the CEC case. The format follows that of Fig. RC1.2.

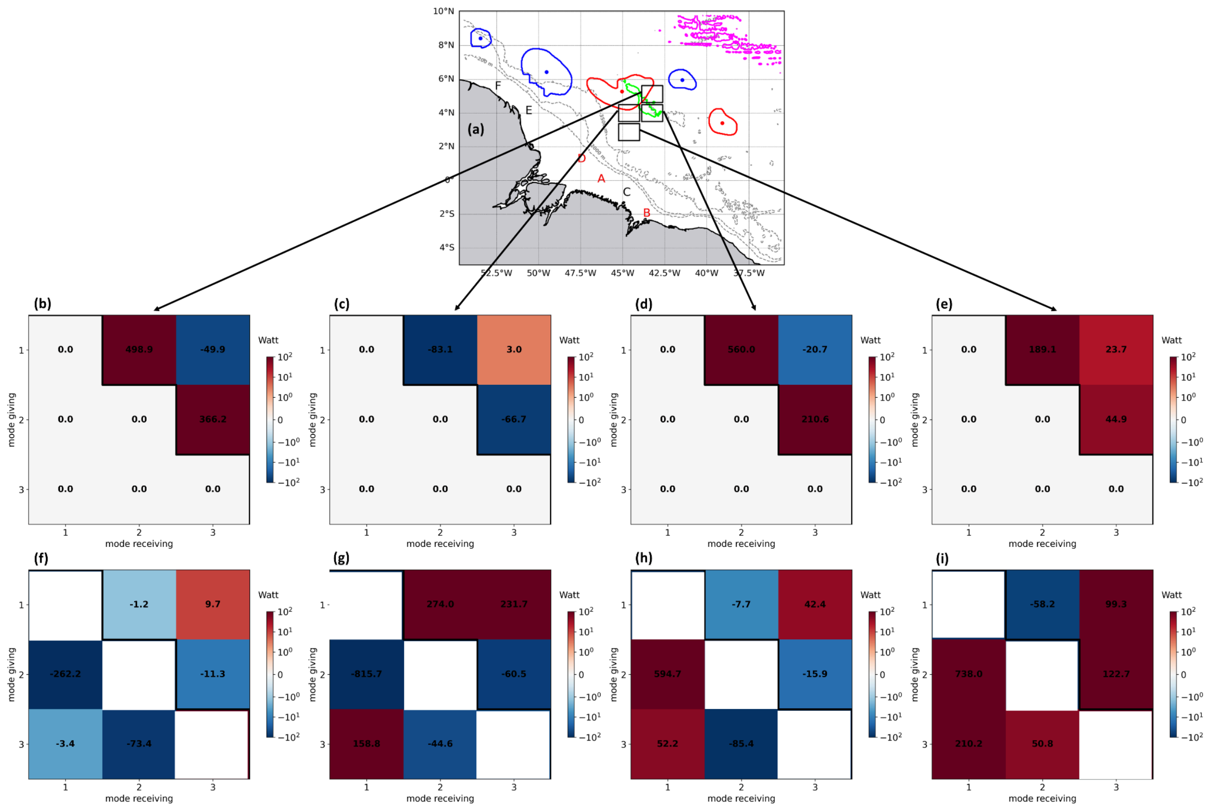

Figure RC1.3. Matrices of energy couplings integrated over a defined interaction box, where each matrix entry represents the coupling between a row mode and a column mode, for the CEE case. The format follows that of Fig. RC1.2.

L5530-538: One of the most striking feature of Fig 7 is the alternating bands in H_{12}, and H{23}, to a lesser extent. This is noted in L532, but left without explanation. This is due to the symmetric part (background and IT interactions). Is this due to an interference pattern (stronger IT)? You see it both before and after the seamount...

R: The reviewer's comment, remark, and suggestion are duly noted and appreciated.
We agree that the alternating bands in H_{1-2}, and to a lesser extent H_{2-3}, dominated by the symmetric component, are among the most striking features of Fig. 7. We also agree with the reviewer's suggestion that these bands could result from an interference pattern, as they appear both upstream and downstream of the seamount. We have added a sentence at L532 to explicitly acknowledge this and propose it as a plausible physical mechanism. This is mentioned in lines 589-592 of the revised manuscript.

L602: The impact of anticyclonic eddies is not shown in this paper.

R: We acknowledge that the impact of anticyclonic eddies is not explicitly analyzed in the present study. The brief observation of AE-induced deflection at site E (Fig. 4b) was included as a qualitative illustration supporting the polarity-dependent refraction discussed in the literature, and not as a systematic result of our study. Recent observations from the SWOT satellite on the interactions between internal waves and mesoscale eddies in my study region supported our finding, documenting analogous refraction at a CE core and diffraction at a western AE edge (Goret et al., 2026).
We have revised L602 to make this distinction more explicit and to avoid any ambiguity regarding the scope of our analysis. This is mentioned in lines 666-676 of the revised manuscript:

"

IT beam deviation is sensitive to eddy properties. The direction of deviation depends strongly on eddy polarity, as shown by previous studies (e.g., Huang et al., 2018; Guo et al., 2023; Dunphy et al., 2017; Wang and Legg, 2023; Li et al., 2024; Goret et al., 2026). While the present study focuses exclusively on CEs, a qualitative illustration of AE-induced deflection can be glimpsed in the energy flux path emanating from the less energetic generation site E (Fig. 4b: deviation of the energy flux due to an AE core centered at 5.9°N and 48.5°W). While earlier work noted that AE cores speed up Mode-1 propagation and induce clockwise (southward) refraction, whereas CE cores slow it down and induce counterclockwise (northward) refraction, our findings link specific interaction geometries to distinct intermodal energy pathways in a realistic framework. The impact of AEs on intermodal energy pathways remains an important open question. Based on previous studies (e.g., Dunphy and Lamb, 2014; Goret et al., 2026), we can assume that AEs exhibit a symmetric response; however, precise quantification is left for future investigation.

"

For Anonymous referee #2

The reviewer deserves our thanks for the comments, remarks, and suggestions on our manuscript and appreciate the time they dedicated to this review. Below (highlighted in blue and magenta) is an itemized response to the different issues raised in the review.

This manuscript addresses an important and timely question: how the internal tide-eddy interactions and internal tide-topography interactions change the vertical modes of internal tides in the Amazon shelf and off shelf region. The work tackles this question with high-resolution simulations and case-based analysis. The three selected cases provide helpful and clear comparison of internal tide propagation features under three different eddy conditions. I think the manuscript has clear potential for publication.

That said, the manuscript would benefit from more clarifications and more careful physical interpretations. My specific points are listed below:

R: We have carefully read and considered your comments and believe that our revised manuscript takes them into account. Below you will find the detailed responses to the various points raised during the review.

1. Title: The key focus of the study is on how the eddy-internal tide interactions affect the internal tide characteristics. The phrase 'eddy-permitting' is not a central takeaway. I suggest revising it to ': Evidence from 3-km NEMO-AMAZON36 Simulations.'

R: We thank the reviewer for the interest in the title of our manuscript and the suggestion you provided. You are correct, the phrase 'eddy-permitting' is not a central takeaway. We have therefore revised the title in lines 1-2 of the revised manuscript as follows:

"

Internal Tides–Cyclone Eddy Interaction and Intermodal Energy Pathways: Evidence from 3-km NEMO-AMAZON36 Simulations

"

2. For the difference among with-eddy and no-eddy cases, how do you determine that the eddy-encounter geometry is the dominant factor and rule out the contributions by different stratification, NECC intensity? I suggest showing a simple comparison of key background conditions, such as N2, mean current field. A simple 2D ray tracing analysis could also be considered as helpful additional support, but I do not view it as essential.

R: We thank the reviewer for this relevant remark. We have made and added a comparison of the background conditions (e.g., N^2 horizontal gradient and mean current field) along the IT paths from sites A and D across the three cases (NE, CEE, CEC). We have concluded that the key distinction between the eddy cases lies not only in where the IT beam encounters the eddy (eddy core vs. eddy edge), but also in the associated background conditions (currents and stratification). This comparison is illustrated in figures RC2.1 below and in lines 368-389 of the revised manuscript, accompanied with the figures of ∇N^2 and background current.

Regarding 2D ray tracing analysis, we recognize that it could provide additional support and we have dedicated it to future work.

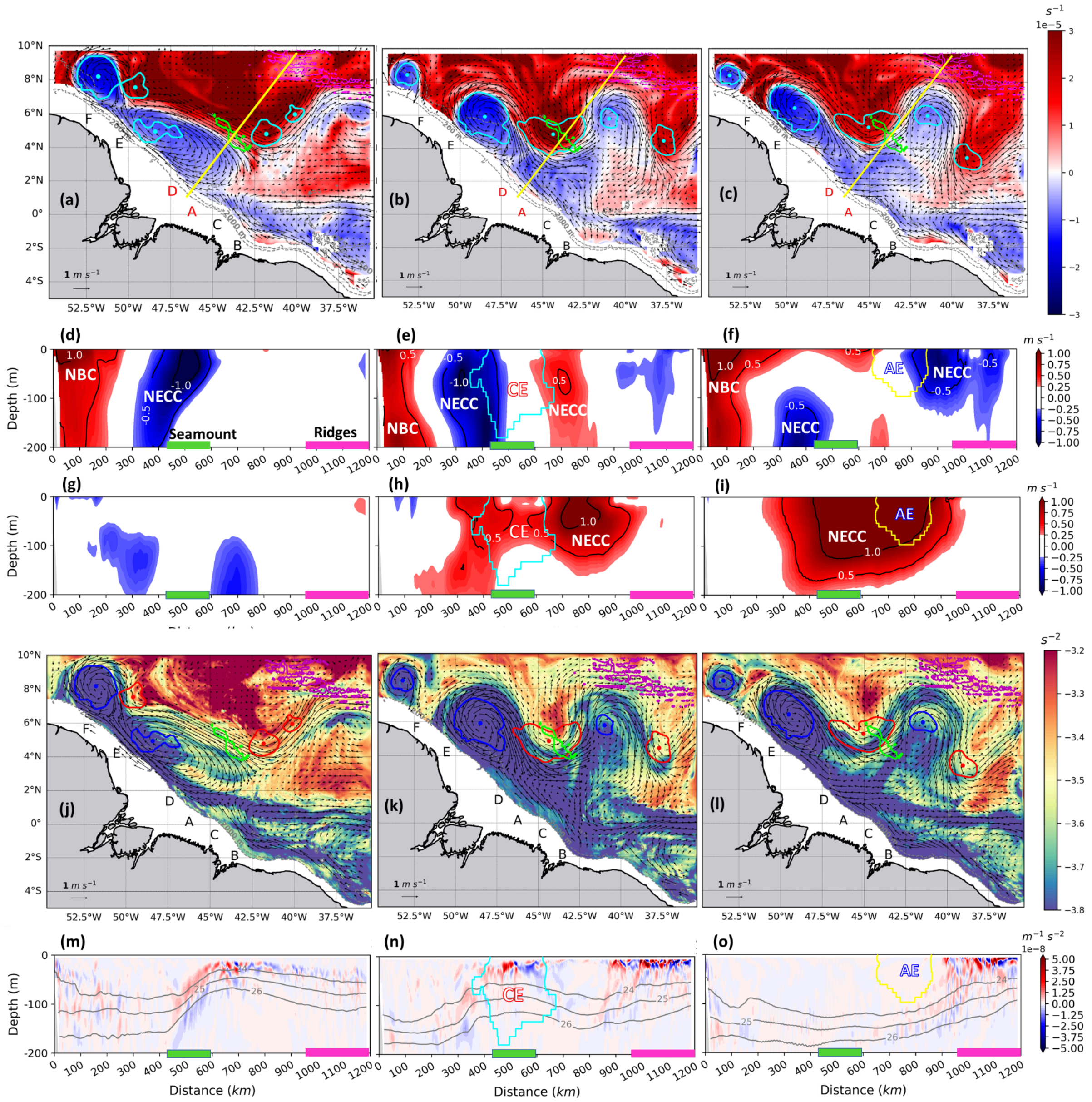

Figure RC2.1. Background conditions for the (a,d,g,j,m) NE, (b,e,h,k,n) CEC, and (c,f,i,l,o) CEE cases. Panels (a–c) show potential vorticity anomaly (PVA; color shading) averaged within the 23–25.5 kg m^{-3} isopycnal layer, with detected eddy edges (cyan, blue, and red contours), eddy centroids (colored dots), and mean background currents (black arrows) overlaid along the 24 kg m^{-3} isopycnal. The transects (yellow lines) highlight the most energetic energy flux pathways considered, originating from sites A and D. Panels (d–f) and (g–i) show the vertical structure of the cross-transect and along-transect background current velocity, respectively (color shading), in the upper 200 m, along the transects (yellow lines) defined on the internal tide paths from sites A and D. In panels (d–f), positive (negative) values indicate flow oriented approximately northeastward (southwestward). Notable topographic features are outlined by colored rectangles (seamount: green; ridges: magenta). Panels (b), (c), (e), and (f) also show the detected eddy edges for AE (yellow) and CE (cyan). Panels (j–l) show buoyancy frequency (color shading) along the 24 kg m^{-3} isopycnal, with eddy edges, centroids, and mean background currents overlaid as in panels (a–c). Panels (m–o) show horizontal stratification gradients (color shading) and potential density isopycnals (23–27 kg m^{-3}; grey contours) in the upper 200 m, along the same transects. Grey shading indicates seafloor topography in panels (d–i) and (m–o).

3. L13, 'to the flow' is not standard phrasing. I suggest revising it to '... decomposition to the high-resolution (3 km) ...'.

R: We appreciate the reviewer's suggestion. We have revised this phrase in lines 17-18 of the revised manuscript as follows:

"

To this aim, we apply vertical mode decompositions to the high-resolution (3 km) simulations during September-December 2015.

"

4. L27, the statement that "Mode-2 energy is completely arrested" is physically striking, but the wording may be too absolute unless supported by very explicit quantitative metric. "Strongly blocked" or "sharply attenuated" may be more appropriate.

R: We recognize and appreciate the reviewer's suggestion. We have revised these statements in lines 31-32 of the revised manuscript as follows:

"

Mode-2 energy, despite initial amplitudes comparable to Mode-1, is strongly blocked at the CE-seamount interface, while Mode-3 remains weak (below 200 W m^{-1}) and less propagative.

"

5. L55, are there more realistic simulations that capture the interaction and trapping processes? It would be helpful to know that these processes does occur and could be examined in more realistic ocean configurations.

R: We appreciate the reviewer's comment. The simulations AMAZON36 presented in our paper are performed with realistic bathymetry and tidal forcing (please see Assene et al., 2024 for AMAZON36 configuration, sensitivity testing and validation). The interaction and trapping processes illustrated with energy flux patterns in our results are fully resolved by the model and occur under realistic ocean conditions. To my knowledge, most (or recent) studies have used idealized simulations to investigate interaction (e.g., Dunphy and Lamb, 2014; Li et al., 2024) and trapping processes (e.g., Wang and Legg, 2023). In contrast, this study almost captures these processes within a fully realistic modeling framework. However, the trapping process is not the main focus of our study.

6. L31, the energy transfer from low mode to higher modes and from internal tides to background is not 'contrasting'.

R: The reviewer deserves our thanks for this comment. We agree that "In contrast" was ambiguous. We have clarified the sentence by explicitly specifying "in the absence of an eddy" and "in the presence of a CE" to make clear that the contrast refers to these two distinct physical situations rather than to the two types of energy transfers. For clarity, we have revised the statement in lines 35-37 of the revised manuscript as follows:

"

In the absence of an eddy, the seamount drives a forward energy cascade (O (10^{-8} W m kg^{-1})) from the Mode-1 IT to higher modes. In contrast, in the presence of a CE, the CE's strong horizontal shear triggers a competing inverse energy cascade (O (10^{-8} W m kg^{-1})) from the background flow to the IT modes.

"

7. L62-65, you may want to elaborate on the motivation for focusing on daily timescale variability – i.e. why variability on this timescale is important.

R: We are grateful to the reviewer for this remark. To address the motivation to focus on daily timescale variability, we have added in lines 69-72 of the revised manuscript this text below:

"

On seasonal timescales, it becomes difficult to distinguish variations in internal tide responses (e.g., incoherence, trapping, and deviation), particularly those induced by changes in submesoscale and mesoscale activity and the background shear. Analyses at daily timescales could better capture the specific background conditions that most strongly modulate the fate of internal tides.

"

8. L107, it would be informative to give a short but explicit resolution argument: what does 3 km imply for the horizontal wavelengths of Mode-1, Mode-2, and Mode-3 in this region.

R: We thank the reviewer for this suggestion. The 3 km horizontal resolution provides approximately 30–60, 20–28, and up to 17 grid points per wavelength for Mode-1, Mode-2, and Mode-3 M2 internal tides in the Amazon region (horizontal wavelengths of ~90–180 km, ~60–85 km, and up to ~50 km, respectively, Tchilibou et al., 2022, 2025), ensuring that all three modes are well resolved. We have added a short resolution argument in lines 118-121 of the revised manuscript this text below:

"

The 3 km horizontal resolution provides approximately 30–60, 20–28, and up to 17 grid points per wavelength for Mode-1, Mode-2, and Mode-3 M2 internal tides in the Amazon region, corresponding to horizontal wavelengths of ~90–180 km, ~60–85 km, and up to ~50 km, respectively (Tchilibou et al., 2022, 2025). This resolution ensures that all three modes are well resolved…

"

9. L124, typo: 'runned' should 'ran'.

R: We humbly thank the reviewer for sharing this good catch.

10. L129, Could you clarify whether the internal tide signal was extracted from a long time-series and subsequently analyzed in 25-hour segments, or whether harmonic fit was applied independently to each 25-hour segments? In the latter case, the potential frequency leakage errors should be carefully assessed and discussed.

R: We thank the reviewer for this important comment. The harmonic analysis was applied independently to selected 25-hour segments corresponding to the selected cases in order to extract the M2 constituent. We chose to focus on 25-hour segments to examine the internal tide response to the eddy, assuming stratification is constant (mean stratification) within each segment but varies between segments.

We acknowledge that applying harmonic analysis over short time windows could, in principle, lead to potential frequency leakage from nearby tidal constituents (e.g., S2, N2, and K2) into the M2 signal. However, in our case, leakage effects are expected to remain small for these reasons. First, the tidal regime in the study region is strongly dominated by M2, accounting for more than 70% of the total tidal energy (Beardsley et al., 1995; Gabioux et al., 2005). For instance, based on 9- to 12-month records, Tchilibou et al. (2022), Barbot et al. (2022), and Assene (2024, PhD thesis) report internal tide SSH amplitudes of approximately 0.7 cm for M2 compared to approximately 0.2 cm for S2. This large amplitude ratio suggests that any potential leakage into the M2 fit arising from the use of 25-hour analysis windows is expected to be reduced relative to the dominant M2 signal.

Second, similar short-window harmonic analyses have been successfully applied in other studies. For instance, Waterhouse et al. (2018) extracted M2 internal tide properties from 17–29 h CTD–LADCP stations, and Byun and Hart (2018, 2020) demonstrated that M2 amplitudes and phases derived from 25-hour segments yield accurate reconstructions with low prediction errors.
Based on these elements, we expect that frequency leakage does not significantly affect the M2 estimates in our analysis. This limitation has now been clarified and discussed in line 171-178 of the revised manuscript as follows:

"

Although performing harmonic fits over short (25-hour) segments may introduce frequency leakage from nearby tidal constituents (e.g., S2, N2, and K2) into the M2 signal, this effect is expected to be minimal in our case because the tidal regime is strongly M2-dominated, accounting for more than 70% of the total tidal energy (e.g., Gabioux et al., 2005; Tchilibou et al., 2022; Fassoni-Andrade et al., 2023). This approach is consistent with previous studies that have successfully applied harmonic analysis to similarly short records to resolve semidiurnal constituents (e.g., 17–29 h M2 tidal observations in Waterhouse et al., 2018), demonstrating that dominant semidiurnal tides can be reliably estimated from short-duration data.

"

11. L175, the phrase “horizontally homogeneous stratification” is misleading. My understanding is that the modes are computed locally at each grid point using a local profile of mean stratification profile; if so, that wording would be more accurate.

R: We are indebted to the reviewer for this remark. We thank the reviewer for pointing out this misleading wording. We agree that “horizontally homogeneous stratification” incorrectly implies the use of a single stratification profile over the entire domain.
We have removed this phrase and reformulated it in lines 182-183 of the revised manuscript as follows:

“

The vertical modes are obtained by solving the standard Sturm-Liouville eigenvalue problem at each horizontal grid point, using the local mean stratification profile (based on the time-mean buoyancy frequency, $\overline{N^2}$), …).

“

12. L174, because topographic scattering is central to the paper, the use of flat-bottom modes might be a potential caveat. Could you add more discussion to elaborate how much this approximation affects the higher-mode projections near steep topography?

R: We recognize and appreciate the reviewer's comment. We acknowledge that the use of flat-bottom vertical modes near steep topography, such as the Ceará Rise seamount, represents a potential limitation of our methodology, particularly for the projection of higher-mode energy. However, we note that this approach is standard in the community and has been widely applied in studies of internal tide energetics over complex topography (Kelly, 2016; Bella et al., 2024, 2026). In this framework, the horizontal gradients of the flat-bottom modes induced by topography are precisely what give rise to the topographic coupling term.
The errors associated with this flat-bottom approximation are expected to be of a few percent for the dominant low modes (Kelly, 2016). We acknowledge that higher-mode projections near the seamount may be subject to uncertainties, and we have added a sentence in the text to explicitly flag this limitation and direct the reader to future work using topography-aware mode decompositions.

We have mentioned this limitation in lines 197-199 of the revised manuscript as follows:

"

It should be noted that the flat-bottom assumption could represent a limitation for higher-mode projections near steep topography such as the Ceará Rise seamount. The errors associated with this flat-bottom approximation are expected to be of a few percent for the dominant low modes (Kelly, 2016).

"

and also mentioned this limitation in lines 179-182 of the revised manuscript as follows:

"

Finally, it should be noted that the use of flat-bottom vertical modes in the vicinity of steep topography represents a limitation of our analysis, particularly for diagnosing higher-mode energy transfers near the seamount. Future work employing topography-aware modal decompositions would help refine these results and provide a more accurate representation of IT energetics

"

13. Since the first three baroclinic modes are emphasized and used for analysis, it would help to quantify what fraction of the total baroclinic energy or flux they represent relative to the first 10 solved modes.

R: We thank the reviewer for this helpful suggestion. We solved for 10 baroclinic modes (modes 1–10) in addition to the barotropic mode (mode 0). We have included a quantitative assessment of the fraction of energy captured by the first three baroclinic modes (modes 1–3), relative to both the total baroclinic energy flux and the total energy flux (baroclinic + barotropic).

The first three baroclinic modes account for 96.2% (33.2%), 96.8% (37.9%), and 97.2% (26.3%) of the total baroclinic energy flux (relative to the combined baroclinic and barotropic flux) in the NE, CEC, and CEE cases, respectively. This confirms that these modes capture the dominant share of baroclinic energy and supports their use as the basis of our analysis. This information has been added in lines 207-210 of the revised manuscript as follows:

"

These three baroclinic modes account for 96.2% (33.2%), 96.8% (37.9%), and 97.2% (26.3%) of the total baroclinic energy flux (relative to the combined baroclinic and barotropic flux) in the NE, CEC, and CEE cases, respectively. They therefore capture the dominant share of the baroclinic energy, supporting their use as the basis of our analysis.

"

14. L269, typo: Weiss, 1991.

R: We would like to acknowledge the reviewer's comment.

15. L272, one important point: Eq. (13) for the Okubo–Weiss parameter. The traditional form of this criteria is that the sign of W=Sn2+Ss2-ζ2. This sign convention should be verified carefully.

R: We commend the reviewer for this good catch.

The sign convention of W has been verified carefully. There was a typo. We have revised in line 287 of the revised manuscript as follows:

"

W=Sn2 + Ss2 - ζ2

"

All other expressions in the manuscript have been carefully verified for correctness.

16. L301, the manuscript frequently refers to CE “core” and CE “edge,” but a quantitative definition would make the case selection much more rigorous, for example in terms of normalized distance from the eddy center.

R: We wish to express our gratitude to the reviewer for this suggestion.
In our study, the three cases are qualitatively distinguished by the presence or absence of a CE, and, when a CE is present, by the geometry of the IT–CE intersection. We have added a quantitative definition for CE “core” and CE “edge” in lines 384-389 of the revised manuscript as follows:

“

It should be noted that the eddy core/center and eddy edge are defined as regions where $r/R \approx 0$ and $r/R \approx 1$, respectively, where r is the distance from the eddy centroid and R is the radius of maximum velocity. This geometric difference in the CE encounter location leads to markedly different energetic behavior, as discussed below.
In this study, the three cases are qualitatively distinguished by the presence or absence of a CE, and, when a CE is present, by the geometry of the IT–CE intersection.
“

17. I think that it would strengthen the physical interpretation to report a few nondimensional measures for the two main eddy cases, such as Rossby number, Froud number.

R: Our thanks go to the reviewer for this valuable suggestion. We agree that Rossby and Froude numbers would provide useful nondimensional characterization of the two main eddy cases. Limiting our analysis to three case studies reflects the primarily qualitative nature of our approach. The eddy properties relevant to our analysis — namely the eddy velocity, location relative to the IT beam (core vs. edge), and radius — are used to obtain information on eddy properties encountered by energy flux. A natural next step of this study (future works) would be to extend it toward more quantitative results by providing nondimensional characterization of the internal tide-eddy interactions.

18. L328, the three selected cases are interesting and well chosen, but a short statistical summary would make the choice and related conclusions much more persuasive. How often do NE, CEC, and CEE situations occur?

R: The authors gratefully acknowledge the reviewer's suggestion.

We acknowledge that statistical analysis of the occurrence frequency of these three cases could be more persuasive. However, it is one year and half more work to assess and quantify all these statistical events. The present study acts as a first step toward this quantification. It is specifically designed as a process-oriented investigation addressing three targeted questions: (1) whether the IT propagates freely, deviates, or becomes trapped by mesoscale features; (2) whether these outcomes depend on the IT's vertical mode, or on the location of the eddy encounters along with the associated background conditions (currents and stratification); and (3) what the synergistic roles of topography and cyclonic eddies are in governing modal energy transfers. In this framework, the three selected cases are chosen as qualitative configurations to dissect these interactions through vertical mode projection and intermodal energy transfer analysis. Limiting our analysis to three case studies reflects the primarily qualitative nature of our approach. A systematic statistical analysis — such as composite maps or frequency-of-occurrence diagnostics — is very relevant and will be done in a second study.
We have added a sentence explicitly acknowledging this as a natural and promising direction for future work (lines 771-775 of the revised manuscript), as follows:

“

Limiting our analysis to three case studies reflects the primarily qualitative nature of our approach. A natural next step would be to extend it toward more quantitative results by conducting composite analyses over a larger set of eddy–IT interaction cases. Grouping configurations by eddy position relative to the seamount, for instance, would allow the IT response to mesoscale variability to be characterized in a statistically robust way.
"

19. L417, since energy fluxes from A and D merge along the propagation, I suggest discussing more explicitly whether some beam geometry changes could also reflect multi-source interference, not only eddy refraction.

R: This comment is warmly acknowledged.

We agree that multi-source interference between A and D may contribute to beam geometry changes. However, since these contributions are present regardless of eddy activity, they act as a background that does not invalidate the eddy-induced modulation, which remains the focus of this study.
We have added a brief discussion acknowledging these effects in lines 648-651 of the revised manuscript as follows:
"
It should be noted that multi-source interference, observed along the propagation paths of the energy fluxes from sites A and D, could also modify the beam geometry independently of mesoscale activity. In this study, we assume that the contribution of multi-source interference is smaller than that of eddy-induced effects. A more detailed analysis would be required to precisely quantify this contribution.
"

20. L426, figure4, the local C_{mn} and H_{mn} maps are informative, but an integrated budget over a clearly defined interaction box would be of great interest and helpful for the dominant-mechanism interpretation.

R: The reviewer deserves our thanks for this helpful comment. We have carefully verified and updated the results for local terms C_{mn} and H_{mn}, as well as the associated comments (in lines 451-456; 460-465; 476; 526-536; 549; 575-578; 585-594; 606 of the revised manuscript). For each term, we have also integrated the budget over a clearly defined interaction box in all three cases and presented it below in Figure in figure RC2.2-4.
We did not find it necessary to include them in the manuscript, as the existing results already support our main conclusions:
In the NE case, the term Cmn is dominated overall by a forward energy cascade between IT modes, both upstream of (Figs. RC2.2c, e) and directly over (Figs. RC2.2b, d) the seamount. For the term Hmn, its symmetric part dominates overall in these same regions (Figs. RC2.2f–i). This indicates an interplay between Cmn and Hmn in the NE case.
In the CEC case, the term Cmn is dominated by an inverse energy cascade between IT modes near the CE core and seamount (Figs. RC2.3c, d). For the term Hmn, its symmetric part likewise dominates overall in these regions (Figs. RC2.3f–i), indicating a similar interplay between Cmn and Hmn as in the NE case.
In the CEE case, both forward and inverse energy transfers coexist for the term Cmn near the CE edge and seamount (Figs. RC2.4b–e). For the term Hmn, its symmetric part again dominates overall in these regions (Figs. RC2.4f–i), indicating a similar interplay between Cmn and Hmn as in the CEC case.

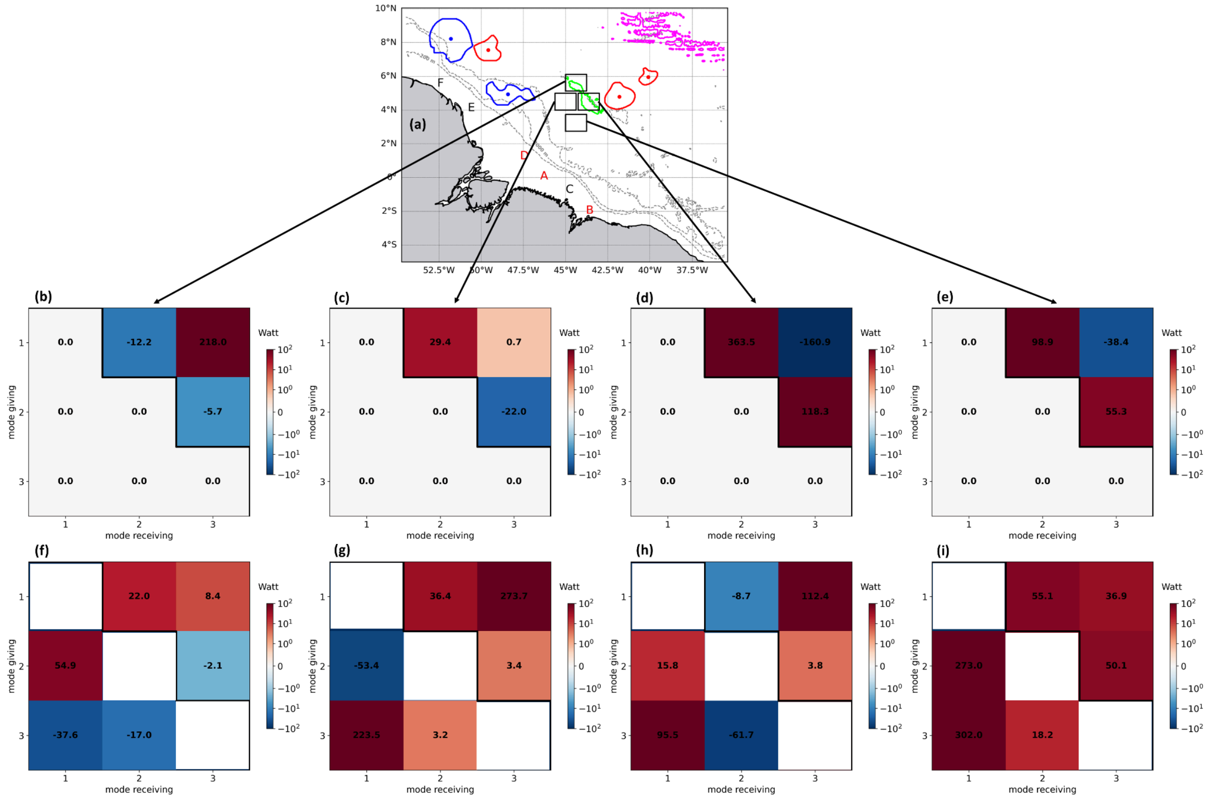

Figure RC2.2. Matrices of energy couplings integrated (in Watt) over a defined interaction box, where each matrix entry represents the coupling between a row mode and a column mode, for the NE case. A positive (negative) value indicates a net energy transfer from the row mode to the column mode (and vice versa). The upper triangle shows the anti-symmetric part of the energy couplings, while the lower triangle and diagonal show the symmetric part. Panel (a) shows the spatial extent of the interaction box, along with the detected eddy edges (blue and red contours). Panels (b–e) show the topographic scattering and stratification term (Cmn; color shading). Panels (f–i) show the horizontal shear coupling term (Hmn; color shading).

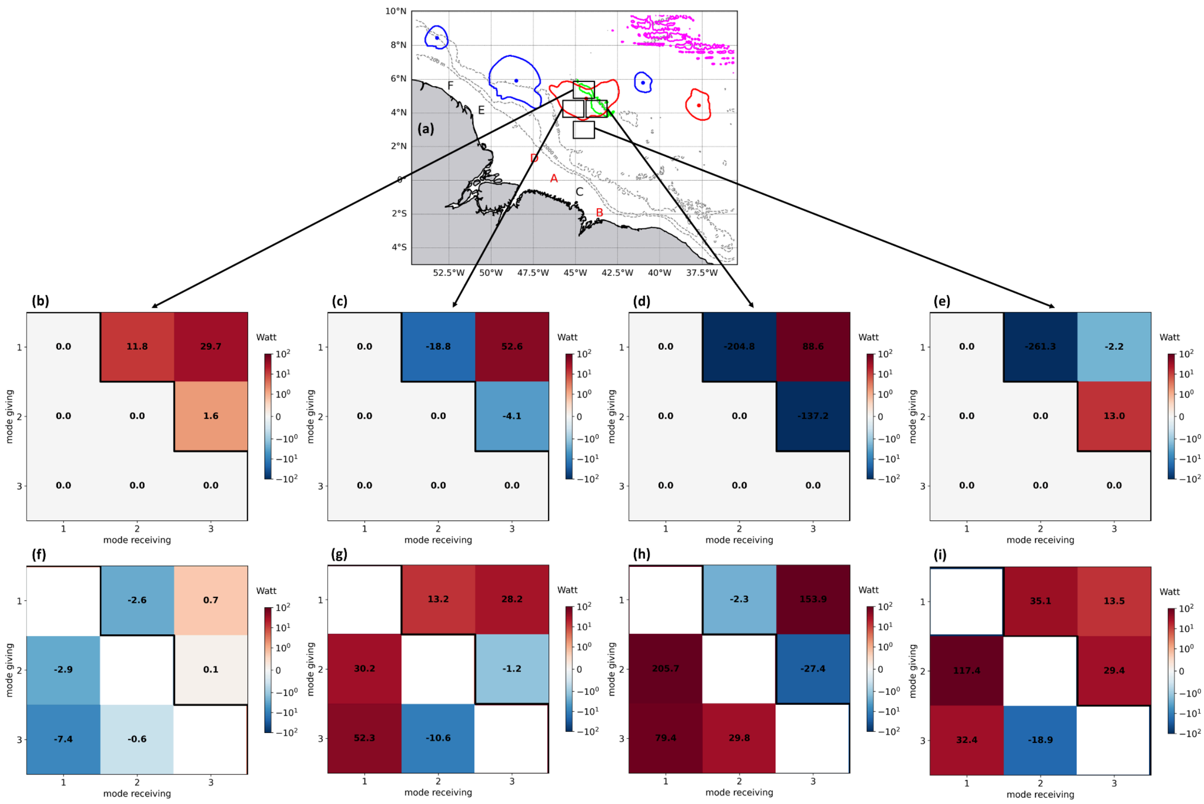

Figure RC2.3. Matrices of energy couplings integrated over a defined interaction box, where each matrix entry represents the coupling between a row mode and a column mode, for the CEC case. The format follows that of Fig. RC2.2.

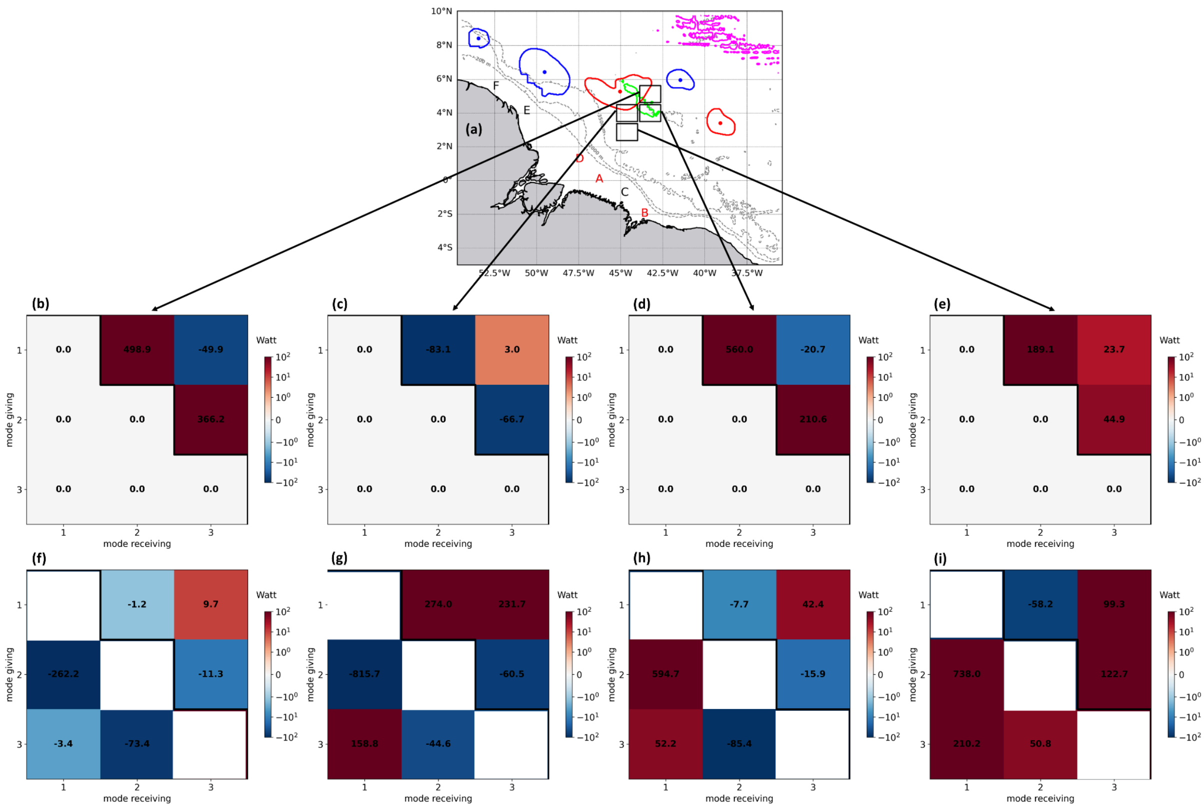

Figure RC2.4. Matrices of energy couplings integrated over a defined interaction box, where each matrix entry represents the coupling between a row mode and a column mode, for the CEE case. The format follows that of Fig. RC2.2.

21. L452, it is very interesting that the mode 2 is mostly blocked but mode 3 appears weaker and more spatially spreading. It would be helpful to clarify the physical reason for this difference in behavior. Is mode 3 less clearly blocked because it is already less coherent, or more locally generated near the seamount?

R: We take note of and sincerely thank the reviewer for these comments and suggestions. We thank the reviewer for this insightful comment. We agree that the difference in behavior between modes 2 and 3 is physically interesting. The weaker and more spatially diffuse signature of mode 3, in contrast to the clearly blocked mode 2, likely reflects local generation near the seamount — possibly through scattering of the internal tide energy beam at the seamount flanks — and/or a loss of coherence induced by the overlying surface eddy. We note that the seamount height-to-depth ratio of 0.2 may also play a role, as it implies only partial blocking of the water column. Given the complexity of these potentially concurrent mechanisms, a full physical explanation is beyond the scope of the present study.

We have added a sentence to acknowledge this open question in lines 757-759 of the revised manuscript as follows:

"

This weaker and more spatially diffuse signature of mode 3, in contrast to the clearly blocked mode 2, likely reflects local generation near the seamount and/or a loss of coherence induced by the overlying eddy, and deserves future investigation.

"