Submitted to Ocean Science: Review of

Regional modeling of internal-tide dynamics. Part 2: Tidal incoherence and implications for sea surface height observability

Reviewer #2

The study describes the temporal variations of internal tide around New Caledonia from an energy perspective and serves as a companion to Bendinger et al. (2023). The authors conclude that mesoscale eddies contribute to the variations of internal tide generation, as shown through the decomposition of the incoherent term C^{inc} , and propagation, analyzed using a ray-tracing model. Specially, at the generation site (the near field), variations in conversion are linked to changes in baroclinic bottom pressure, which correlate positively with mesoscale-induced stratification changes. After propagation (the far field), regions with strong eddy activity exhibit enhanced incoherent tides and refraction in the propagation direction. The study also explores implications for SSH observability , highlighting that incoherent tides and the orientation of altimetry tracks can impact the separation of balanced and unbalanced motions.

Overall, the study is well-executed, providing solid evidence for the proposed mechanisms through quantified analysis. The discussion on the impact of altimetry track orientation on SSH wavenumber spectra is novel and relevant to the SWOT mission. However, the manuscript is somewhat lengthy, and certain sections could be condensed for clarity. Additionally, I have several questions and comments that would like to be addressed before publication.

First of all, we would like to thank the reviewer for taking the time to go through our manuscript, for the thorough comments and recommendations which helped improve clarity while pointing out the key findings of our study. We tried to shorten the manuscript as recommended, when possible. In the following, we will address the reviewer's comment and state where changes have been made in the manuscript.

Introduction

The recap for Part 1 (l.34-54) can be shorten by focusing on those related to incoherence.

The paragraph was slightly shortened. We prefer not to shorten it further since it is important for the rest of the paper, particularly for those readers which are not familiar with Part 1.

The energy dissipation is introduced in l.71-81, and understanding its temporal variation is stated as one of the objectives in l.106. However, the analysis of energy dissipation is only addressed at the annual mean timescale and described in a single paragraph (l.241-251). The emphasis on the dissipation term in the introduction does not align with the following analysis focus.

Understanding temporal variations of dissipation was removed as one of the objectives. It is an objective in a broader sense, but as pointed out by the reviewer we did not intend to address this in our study. We insist on having it as a perspective (see Sect. 7.4). We included it also in the abstract: "Variations in conversion are not consistently proportional to those in energy flux divergence suggesting that variations in energy dissipation are linked to additional mechanisms that deserve further investigation." See Lines 12-14

Section 2

Equation 1 can be omitted to maintain relevance and save space.

Eventually, Equation 1 was omitted. However, we think that it is important to mention that Equation 1 in the revised manuscript neglects the tendency term of total (kinetic + potential) energy. Further, the energy flux considers only hydrostatic pressure work. See Lines 145-150

Equation 7, why is the energy flux intergraded from 0 instead of η.

We thank the reviewer for spotting this. It is indeed integrated from η. This is now corrected in Equation 3, 6, and 7 in the revised manuscript.

Equation 11, the right parathesis and "dt" for $D_{\mathrm{bc}}^{\mathrm{coh}}$ term are missing.

We thank the reviewer for spotting this. This is now corrected in Equation 10 in the revised manuscript.

At the end of Section 2.2, the authors state that the $D_{\mathrm{bc}}^{\mathrm{inc}}$ represents the overestimated portion of $D_{\mathrm{bc}}^{\mathrm{coh}}$. A similar statement appears in Section 3 (l. 246-247). However, I think the $D_{\mathrm{bc}}^{\mathrm{inc}}$ includes both the overestimated coherent portion AND the actual dissipation from the incoherent tide. The presence of real incoherent dissipation can be verified by the net incoherent dissipation at North (1). Nevertheless, the conclusion regarding the overestimation remains unchanged. Section 2.3 is similar to Bendinger et al (2024) and can be shorten.

The reviewer is absolutely right. We forgot to mention this. We modified the associated paragraph in Section 2.2: "While $D_{\mathrm{bc}}^{\mathrm{D2}}$ accounts for actual energy dissipation, $D_{\mathrm{bc}}^{\mathrm{inc}}$ consists of both the fraction by which $D_{\mathrm{bc}}^{\mathrm{coh}}$ is mistakenly associated with true energy dissipation (or the error by which energy dissipation in $D_{\mathrm{bc}}^{\mathrm{coh}}$ is overestimated) and incoherent energy dissipation." (see Lines 190-193). Similarly, we modified this paragraph in Section 3: "$D_{\mathrm{bc}}^{\mathrm{inc}}$ consists of incoherent energy dissipation and energy transferred from the coherent tide to the incoherent tide." (see Lines 231-232).

We prefer to keep Section 2.3 to ensure that the ray methodology can be understood without referring to Bendinger et al. (2024). Furthermore, by addressing a comment from reviewer #1, the methodology has changed. Briefly, Section 5 has slightly been enriched by considering effects of stratification as well to quantify the extent by which the rays are refracted by mesoscale currents and stratification. Section 2.3 and 5 were accordingly adapted.

Section 4

It is unusual to have only one subsection. I suggest reorganizing the structure. The same applies to Section 5.

The structure has been reorganized: Section 4 is now splitted in two subsections. The subsection for Section 5 is omitted.
4 What drives semidiurnal barotropic-to-baroclinic energy conversion variability?
4.1 Coherent vs incoherent contributions
4.2 Mesoscale-eddy-induced conversion variations
5 Mesoscale-eddy-induced refraction of tidal beams lead to increasing tidal incoherence in the far field

The explained variability is expressed as (%) in text (e.g. l.259-260) while as decimal (0-1) in Table 2. Please ensure consistency.

The explained variability is now also expressed as decimal in the text when assigned to γ.

L.315: "The negative conversion/bottom pressure amplitude anomalies in Fig. 5b". Should likely refer to "Fig. 5a"?

Yes, well spotted. This paragraph was slightly adapted for the sake of clarity to the following: "The monthly time series of bottom stratification $N^2(-H)$ (extracted from the bottom most grid cell), mesoscale SLA, and mesoscale EKE (similarly computed to Sect. 3.2 in Part 1) suggest that conversion variations through dP_A are linked with mesoscale-eddy-induced stratification changes (Fig. 5a and b)." See Lines 301-303

L.327: "In phase with the local tidal forcing, $p_{\mathrm{bc}}^{\mathrm{inc}}(-H)$ induced..." If $p_{\mathrm{bc}}^{\mathrm{inc}}(-H)$ is "in phase", why it is incoherent?

Local effects are not expressed by phase variations as stated in Lines 289-290. In other words, local effects are linked with amplitude variations only with the phase remaining unchanged. We refer to Zilberman et al. (2011). In our case, amplitude variations are linked with mesoscale-eddy-induced stratification changes.

Figure 3

The bathymetric labels are too small to be clearly visible.

Bathymetry labels have been increased in size in Figure 3 and 4.

Figure 5

5(a): What is the physical meaning of the negative values for the ratios of $C^{\mathrm{cross1}}/C^{\mathrm{D2}}$ on the y axis?

Please note that these ratios represent anomalies referenced to the coherent conversion, which explains why there are positive and negative values. Positive (negative) values for this ratio correspond to an increase (decrease) of conversion relative to the coherent conversion.

Table 3

The unit for "delay" should be [days].

In fact, the delay is indeed in [hours]. See the comment below for more details.

Section 5

The "group arrival time" is not clearly defined in Section 5.1. Based on the caption in Table 3, which states "equivalent to 500 km", I assume the "group arrival time" refers to the time taken to propagating 500 km. If so, for South (2) domain, both mode-1 and mode-2 tide propagate faster with the mesoscale currents, which is consistent with the negative "delay" time. If my understanding is correct, the statement in l.409-410, "Mode 2 is substantially more delayed than mode 1", is incorrect for the South (2) domain, as both mode-1 and mode-2 arrive earlier, with negative delayed time.

This statement of mode-2 being more delayed is indeed wrong. Negative delay would imply that mode-2 is faster compared to mode-2 without currents.

As stated above, Section 5 has been modified to meet suggestions from reviewer #1. With the new analysis, we decided to omit the group arrival of semidiurnal rays while focusing on their cumulative refraction due to mesoscale stratification and/or currents. The latter was achieved by integrating the orientation change or angle of the group velocity vector along the propagation path. Note that we integrate absolute values of angular deviation. The group arrival delay relative to the reference ray with annually averaged stratification and currents is relatively small: a maximum of 1-2 hours during a propagation of >2 days. However, we plotted in Figure 8 the daily location (day 1-2) of the rays. Also, note that we omitted the discussion on mode-2 since the semidiurnal energy flux is strongly dominated by mode 1.

I also speculate on the "delay" for South (2) domain. The standard deviation is large enough to cause the "delay" time to switch signs.

In fact, the sign of delay depends whether the ray is refracted northward or southward relative to the reference ray. Since the refraction is of stochastic nature, the mean delay (averaged over all rays) can be close to zero with a standard deviation, which is large enough to cause the delay to switch signs as pointed out by the reviewer.

Section 6

The paragraphs before 6.1 (l.421-445) serve as a recap in Part 1 and an introduction, so they can be condensed. The same applies to l.459-465 in Section 6.2.

l.421-445 were condensed (see Lines 405-419 in the revised manuscript). Particularly, the results recap from Part 1 was omitted since it is already mentioned in Section 2.1 (see Lines 133-139). l.459-465 are already very concise, but we decided to generalize the findings from Part 1.

Below are some minor comments, which I leave to the authors' discretion to consider.

Vague pronoun reference: please explicitly state the references for clarity.
L.10: "it"
it" is now replaced with "incoherent conversion"

L.77: "this can have..."
"this" is now replaced with "energy dissipation associated with the incoherent tide"

L.141: "their Fig. 13 a-d"
"their Fig. 13a-d" is now replaced with "see Fig. 13a-d in Part 1"

L.259: "it explains"
"it" is now replaced with "the coherent conversion"

Overuse of "i.e.": here are some suggested replacements.
L.141: "... Caledonia, including the location..."
This suggestion was taken into account.

L.163: "... incoherent parts for u and the pressure perturbation p"
This suggestion was taken into account.

L.255: "... spring-neap cycle, driven by the interaction of M2..."
This suggestion was taken into account.

Formatting and grammar corrections We thank the reviewer for comments on formatting and grammar. They were all taken into account, unless specified otherwise.

L.5: "...from coherence, in...", add space after the comma.
This suggestion was taken into account.
L.8 and l.255: The phrase "astronomically forced fortnightly modulated spring- neap cycle" is wordy and grammatically incorrect, which reduces readability. Consider a revision.
"astronomically forced fortnightly modulated spring-neap cycle" is now replaced with "interaction of M2 and S2 barotropic tidal currents" and "...due to the spring-neap cycle, driven by the interaction of M2 and S2 tidal constituents" See Lines 6-7 and Lines 240-242, respectively
L.9: use an "em dash" rather than a hyphen. The same applies to others in the manuscript.
Thank you for this comment. We were not aware of the usage of the "em dash". All hyphens were replaced by the "em dash".
L.29: "semi-analytical theory"

This suggestion was taken into account.
L.45: "representative to the coherent"
We believe that "representative of" is the correct usage here, but we will check with the Copernicus editorial service during proofreading.
L.57: I think "near-field" and "far-field" that have a hyphen in between, are commonly used as adjectives, rather than nouns.
We removed all hyphens in "near-field" and "far-field", unless it is used as an adjective.
L.64-65: suggested revision "The mechanisms governing the temporal variability of internal tide vary geographically, and cannot be generalized as the importance ..."
This suggestion was taken into account.
L.67-69: suggested revision "New Caledonia is a particularly challenging region as it is a hot spot of internal tide generation and a region of strong mesoscale variability, making it potentially ..."
This suggestion was taken into account.
L.94: "estimate the length scale at which unbalanced motions ..."
This suggestion was taken into account.
L.186: "taken from the harmonic analysis and vertical mode ..."
This suggestion was taken into account.
L. 215: missing the right parathesis after "(see Fig.1)"
This suggestion was taken into account.
L. 220: "mimic"
Section 2.3 was rewritten, and the associated sentence was removed.
L.239: "distance from the generation"
This suggestion was taken into account.
L.247: "This accounts for 10%, 9% ..."
This suggestion was taken into account.
L.276 (Figure 3 caption): The second sentence lacks a verb.
The second sentence was changed to "Explained variability shown for..."
L.280: "Similarly to the analysis above, we show in the South (2) domain, the contribution of different terms that make up C^{inc} ..."
This suggestion was taken into account.
L.282-283: "While the three terms feature similar amplitudes, their spatial patterns differ." This suggestion was taken into account.
L.283: "Based on the area-integrated explained ... "
This suggestion was taken into account.
L.317 and 319: "compute 5-day mean" and "period of 180 days"
Note that the methodology description is now in the caption of Figure 5. We will double check with the Copernicus editorial service during proofreading. For Part 1, it was suggested to use the notation of "5-d mean" and "180 d".
L.324: "conversion and mesoscale variability ...", which conversion?
The term conversion refers to barotropic-to-baroclinic energy conversion throughout the manuscript. Note that the associated sentence was slightly rewritten/rephrased. See Lines 309-321.
L.335: "pressure amplitude variations are very pronounced, suggesting the influence of the local effects"
This suggestion was taken into account.
L.379: "closely correlated to that of semidiurnal"
This suggestion was taken into account.
L.612: "concerns the impact of conversion variability on outward energy propagation and local energy dissipation"? The logic between these three terms in Equation 2 is unclear to me.

We slightly rewrote the paragraph to the following to improve clarity: "One open question arising from our analysis is how variability in barotropic-to-baroclinic energy conversion influences both the outward propagation of internal-tide energy and local dissipation. According to the baroclinic energy budget (Equation 1), dissipation is defined as the residual between conversion and energy flux divergence. We therefore ask: do variations in conversion directly translate into proportional changes in energy flux divergence—and, by extension, in dissipation—or are they partially decoupled? For instance, does increased conversion always imply stronger outward flux and higher dissipation (Falahat et al. (2014)?". See Lines 585-590. This serves as a perspective. According to Figure 12 in the revised manuscript, variations in conversion and energy flux divergence can be decoupled.