Comments on Ocean Science review (reviewer #1)

This paper presents a broad, rather than in depth, analysis of numerical simulations of the semidiurnal internal tide around New Caledonia. The paper presents 1) a validation of the model results for mesoscales and surface and internal tides 2) internal tide energetics and comparison with analytical models, 3) nonstationary internal tides and the causes of the nonstationarity, and 4) transition scales computed from wavenumber spectra. It is worthy to publish on the strong internal tides around New Caledonia, but I think the paper needs to be improved in several aspects before it can be published. The most interesting and relevant result is presented in Figure 12c.

We thank the reviewer for his/her thoughtful review and for his/her useful suggestion. We agree with him/her that the paper is too long and detailed. Further, we agree on shortening it to focus on information relevant to the story line. This was pointed out by the reviewer #2 as well. However, we disagree with the reviewer's suggestion to omit Chapter 4. The internal tide in the region around New Caledonia has never been described before and the detailed description is of relevance for different research fields (e.g. biogeochemistry).
Based on the suggestion of reviewer #2, and after some discussion with the editor, we concluded to split the paper in two parts to meet both reviewers' suggestions. Part I is dedicated to the model configuration and assessment and the description of the coherent internal tide (including SSH signature). Part II is dedicated to tidal incoherence (temporal variability of conversion, dissipation, energy flux divergence by taking into account a deeper analysis for tidal incoherence.
We suggest the following breakdown in two parts:

Regional modeling of internal tide dynamics around New Caledonia. Part I: Tidal coherence and sea surface height observability
… focusing both on the introduction of the model configuration and assessment and the description of the coherent internal tide

Regional modeling of internal tide dynamics around New Caledonia. Part II: Tidal incoherence
… focusing on tidal incoherence (temporal variability of conversion, dissipation, energy flux divergence)

Following the editor's suggestion, Part I will be treated as a continuation of the current review process, and we would be happy to have you as a reviewer for Part II when submitted as a new submission.
In the following, we reply to the reviewer's **major comments**. We clearly state whether our response addresses Part I or Part II.

1) The paper is long and detailed, and it has many story lines (see above): the paper has 8 pages of references (~120 citations), 835 lines of text (~15,500 words), and 15 figures. To make the paper more digestible, I suggest shortening it to 500-600 lines (30-40% reduction) by omitting text from the introduction, focusing on only information that is relevant to the story line, and focusing on fewer story lines (and go more in depth; maybe (4) can be omitted if the paper focuses on the nonstationarity?).

We agree and, therefore, we decided to split it in two as explained in more detail above. The paper now consists of 13 figures and ~650 lines of text compared to 15 figures and ~850 lines of text in the previous version.

2) It is difficult to learn what the objectives are from the introduction. The introduction needs to clearly state, preferably in the same paragraph, the main objectives of this paper.

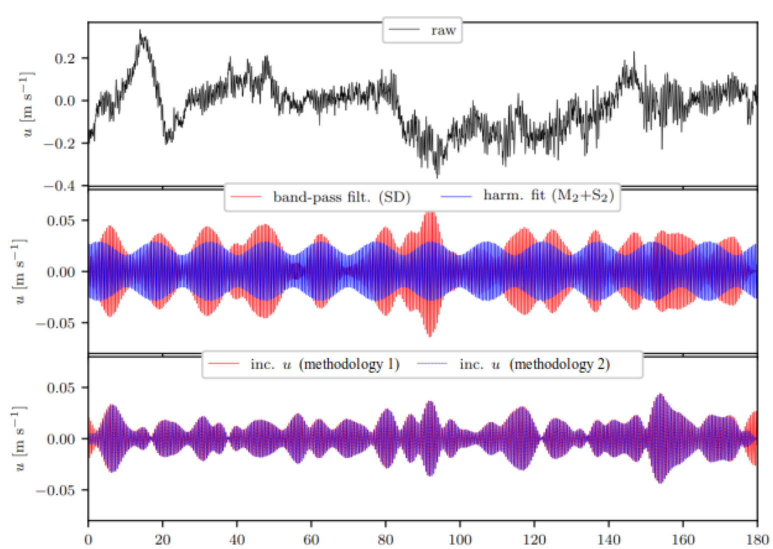
We rewrote and reorganized the introduction, to follow the reviewer's suggestion, and hope that the objectives of the paper are now clearer. Further, it has been slightly adapted to meet the major objectives of Part I.

3) The diagnostics section 2.2 is not well organized. Why are modes discussed in section 2.2.1 on coherent internal tides? I suggest the authors present the energy balances for undecomposed internal tides and modal energy balances and then discuss how these terms are computed with either bandpassing or harmonic analysis (see Buijsman et al (2020) and the Kelly papers referenced therein).

We agree with the reviewer, that the organization of section 2.2 was not well organized in the preprint. For clarification on this point, in the original version we first used a tidal analysis tool developed at LEGOS, Toulouse that provides the energy balance, and the modal decomposition based on the harmonic analysis. This was the starting point of our analysis. Once the analysis of the coherent tide was completed, the incoherent tide was deduced from a bandpass filtering on the full signal (subtracted by the coherent tide).
Since we focus in Part I now solely on the coherent tide, the discussion on this point is out of scope. Further, section 2.2 has been modified by suppressing the reference to incoherent tides (section 2.2.3). However, for Part II we will follow the reviewer's suggestion and section 2 will be changed accordingly.

I am confused about the explanation on how the authors compute the total, coherent, and incoh. internal tides near line 247. As in Nash, Pickering, and Buijsman papers the total internal tide signal is the tidally band-passed signal (e.g. 10-14 hours), the coh. signal is computed with a harmonic analysis of the band-passed fields, and the incoh. signal is taken as the difference. Your method differs from this. Can you explain why? It seems you add the coh. and incoh. signals to get the total D2 signal, which should be better emphasized.

Indeed, we added the coherent and incoherent signal to get the total D2 signal. This logic was addressed just above. We agree that this should have been better emphasized in the original version. Even though the methodologies' order slightly differ from each other, we checked that they give exactly the same result, as expected; see below for a given time series:



Computation of the incoherent tidal signal for an exemplary time series. Methodology 1 refers to the reviewer's suggestion: First bandpass filtering on the raw signal before applying a harmonic analysis. Subtract the coherent tide from the bandpassed signal to obtain the incoherent signal. Methodology 2 refers to our methodology by applying harmonic analysis on the raw signal, before subtracting the coherent tide from the raw signal. Apply on the corrected signal the bandpass filtering to obtain the incoherent tide.

4) For section 2.3.6 you need to explain first that you compare the simulated IT conversion rates to these analytical models. The purpose of this section was not clear to me. This section can be shortened too. It is mostly about why these models break down, which is well-known.

We agree. We shortened section 2.3.6 while clearly stating our objective in using these datasets.

5) For section 4, you use the harmonically analyzed fields over 1 year. That means that your dissipation estimates contain the scattering to the incoherent internal tide (possibly a significant fraction). This may be a reason that your q values are too high (L744)? I suggest you redo this analysis and compute the dissipation rates for the band-passed fields.

We agree with the reviewer that the dissipation estimates, derived from Equation 6 and 7, may contain a significant fraction associated with a loss of coherence of the internal tide. However, since we remain in Part I within the context of coherent tide, we keep the dissipation term definition as it is for now, but mentioning clearly in the text now that this "dissipation" term estimate contains both true dissipation and also scattering to incoherent tide (lines 206-208 and lines 609-613). The new calculation (dissipation rates computed for the band-passed fields) will be done and presented in Part II.

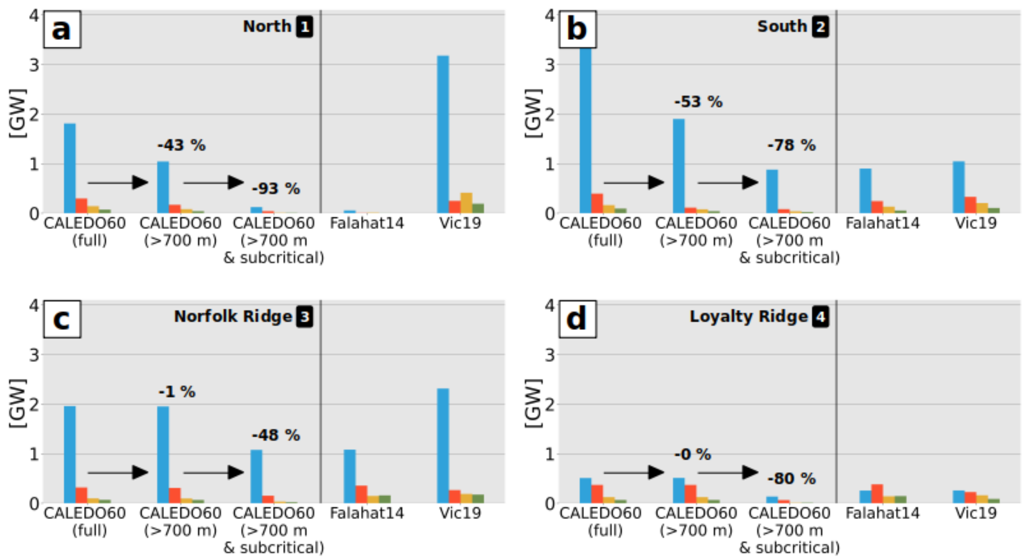
6) Can you explain to the reader how precisely the 'transition scale' is computed? This transition scale should always occur for scales larger than the mode 1 internal tide? On line 797 you write "Correcting our model for the coherent internal tide, we were able to improve SWOT observability (here measured by the transition scale between subinertial and superinertial motions) in winter from 160 km to 50-80 km" How is that possible? You still have the incoherent internal tide in your model?

We define the transition scale by the scale at which subinertial frequencies dominate over superinertial frequencies (see line 646). We argue that observability of meso- to submesoscale dynamics (geostrophic currents) is given for those scales where subinertial (balanced) motion dominates over superinertial (unbalanced) motion.
We refer to Vergara et al. (2023): https://doi.org/10.5194/os-19-363-2023.
It should not necessarily be larger than the mode 1 internal tide, if subinertial processes (mesoscale processes) have a greater amplitude in sea surface than the internal tide.
Indeed, after correction for the coherent tide, the incoherent tide is still contained in the SSH signal, but the whole point of the transition scale is to set the wavelengths where the meso- to submesoscale energy dominates. This is the case for winter (Fig. 15b) down to scales 50-80 km when correcting for the coherent tide. At scales < 50-80 km the incoherent tide (i.e. superinertial motion) becomes more dominant than subinertial motion which does not allow us anymore to derive geostrophic currents and limits (SWOT) observability of mesoscale energy even if we correct for the coherent internal tide.

In the revised version of Part I, we tried to clarify the definition of the transition scale. Since we focus in Part I only on the coherent tide, we adapted in the new manuscript the SSH spectra by replacing the seasonal spectra by the annual spectrum. The seasonal spectra will be presented and discussed in Part II.

7) I suggest the authors redo their comparison with the analytical models (section 4.3) and compute conversion rates below 700 m for all models as in Buijsman et al (2020). This may yield a better comparison.

Thanks for this suggestion. Next to Falahat et al. 2014 and Vic et al. 2019, we plotted in addition to our full model conversion, the model conversion corrected for the upper 700 m and corrected for the upper 700 m and critical/supercritical slopes. This yields a better comparison to those of Falahat et al. 2014 and Vic et al. 2019. Doing so, we want to highlight that a large part of conversion takes place in shallow waters and above steep topography which are not considered in semi-analytical theory. In fact, the correction for the upper 700 m and critical/supercritical slopes may reduce the total conversion by 50-90 %. Thus, pointing out the limited eligibility of semi-analytical models in regions such as New Caledonia.



8) In-depth analysis. Figure 12c shows a nice result. I wonder why the authors do not show the correlation between the incoherence in the open ocean away from the ridge with monthly EKE (see also Zaron et al 2014 and Savage, Waterhouse, Kelly 2020 for metrics that may explain the sources of variability (EKE, N, and vorticity)). That correlation does not exist?

This correlation was hard to find, since incoherence at a specific location is not necessarily linked to the local EKE, but to the EKE found all along the propagation path. Linking the incoherent fraction with the EKE encountered along the propagation beam will be explored more in depth in Part II.

Why is conversion not shown instead of the flux divergence? If C does not have that correlated variability, then the variability is in the Dissipation? This is interesting and worthy of further investigations.

Baroclinic pressure is shown. Conversion was not shown initially since it is highly correlated with the baroclinic pressure. In other words, the variability of the flux divergence is due to the variability of the conversion term which in turn is driven by variations of the baroclinic pressure amplitude. The variability is not in the dissipation, which appears to be relatively constant. This will be shown in Part II.

It may also be interesting to evaluate the mode-mode coupling term (see Kelly 2019 and Zaron 2022 papers) because the internal tide in this area must be affected by topographic scattering.

We fully agree this would be interesting. However, we are afraid that this analysis will be too extensive and time consuming, and beyond the scope of this paper. It would deserve a dedicated study.

Selected minor comments (if not commented, approved and taken into account)

L31. For the modes cite Gill here.

Thank you for pointing this out. The reference (Gill 1982) was added in line 24.

L43. Who is 'they'?

'They' refers to the satellite altimetry products that reconstruct internal tide SSH signatures. This is now clearly stated in line 36-38.

L153. Incorrect citation year for Nelson. I think it should be 2020? You may also cite Siyanbola et al (2023; https://doi.org/10.1016/j.ocemod.2022.102154) here.

We thank the reviewer for spotting this mistake, it has been corrected. We also added the reference from Siyanbola et al. 2023.

L675-676. Please see Xu et al (2022; https://doi.org/10.1029/2022JC018769) in which this difference is illustrated.

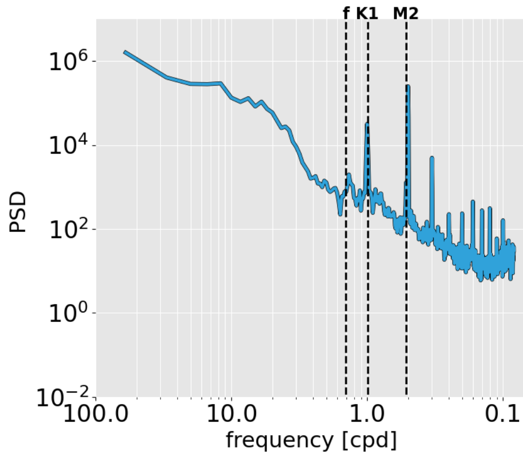
We thank the reviewer for this citation. We keep it in mind for Part II.

L729. It is generally well understood that tall supercritical ridges generated strong mode 1s. Fig 1b and c. Fontsizes are hard to read. Please make a smaller and b and c larger.

Even though this is often stated in literature, we struggled to find publications clearly showing this link, and we still find this relationship vague since those findings only rely on simulations with simplified topography (e.g. Legg and Huijts 2006). It would be helpful if the reviewer could refer to another study that emphasizes the above relationship that we could cite. The original Fig. 1 was splitted in two (Fig.1 and Fig. 7 in the new manuscript) for the sake of better visualization.

Fig 4. What is the peak at SD in the blue line? Residual tides?

This is an interesting remark that we also noted before and which we have also seen in other numerical simulation outputs. The SD peak in the blue line (located at exactly 2 cycles day^{-1}) can not be related to oceanic tidal motion (since it is in a simulation with no tidal forcing), but it is probably related to the model atmospheric forcing (ERA5). When zooming in, it is also visible in the simulation with tides and in the mooring data. We conclude that is related to atmospheric tides (atmospheric S1/S2 tides, Chapman and Lindzen 1969, https://doi.org/10.1007/978-94-010-3399-2), and more recently Balidakis et al. 2022, https://doi.org/10.1029/2022MS003193) that are linked to periodic heating of the atmosphere that expresses particularly in surface pressure oscillations. This signal is contained in ERA5 (see below the PSD of ERA5 surface pressure at our mooring locations). This is briefly mentioned now in Fig. 4.



Plot axis in cycles per day to facilitate easier reading of the tides and their higher harmonics.

Thank you for the suggestion. The axis unit was changed to cycles per day.

Fig 5. How many modes are used for (e) and (f)?

In total we solve for 10 modes, where n = 0 is the barotropic mode and n >= 1 the baroclinic modes. Therefore, baroclinic modes 1-9 were used for (e) and (f). Note that the baroclinic SSH is now shown in Fig. 13 with mode 1 and mode 2 being compared to HRET following the suggestion from reviewer #2.

Fig 7. Dbt is mentioned twice in legend of (a)

Thank you for spotting this mistake. It is now corrected (Fig.8 in the new manuscript).

Fig.8 Please better explain the percentage values. What variables are in these ratios?

The percentage values give (i) (in white) the ratio of the barotropic energy flux divergence that is either dissipated through bottom friction or converted to baroclinic energy ; and (ii) the ratio of the energy conversion term that is either dissipated or that radiates away. Caption has been modified accordingly (Fig. 9 in the new manuscript).

Fig.10? Blue patches in (b) are because the residual is based on coherent tide only? Nonlinear terms are missing?

Yes, tendency term and nonlinear terms are not considered here. For now, we cannot tell whether these missing terms are responsible for the blue patches. This needs further investigation.

Fig 11. I do not see a red box in (c). Can you indicate the transect line for (d) in (c)?

Indeed, we have forgotten to plot the red box. We will add it in Part II. The transect lines for the SSH spectra are now clearly shown in Fig. 13 in the new manuscript for (i) the northern and (ii) the southern tidal beam.

Fig 15. Text in legend is too small.

We agree with the reviewer. The legend has been increased in size and put outside Fig. 13 in the new manuscript.

Comments on Ocean Science review (reviewer #2)

This paper investigates the internal tide dynamics around New Caledonia using a dedicated one-year long high-resolution numerical simulation. The results presented are interesting in several aspects: first, it provides an unprecedented, model-based quantification of the internal tide energetics and some aspects of its dynamics (including interaction with the mesoscale fields, albeit to a limited extent) of a region of the ocean. Second, this region is of particular interest for the recently-launched SWOT mission and the imminent in-situ campaign associated with the CalVal phase, but also for several biological aspects. On the downsides, the paper is rather long -- because it aims at treating several, somewhat distincts aspects of the internal tide dynamics. In particular, sections 5 and 6, which address the coherent/incoherent part and the SSH signature of ITs, could justify a separate paper with more thorough investigation. That being said, the results are rather well presented on average (with some noticeable exceptions listed below) and the figures are well designed, clearly displaying the informations. Moreover, I appreciated the effort put into validating the simulation using several external datasets.

Below are my comments: first the remarks and question which must be addressed prior publication, unless the authors justify otherwise; then some minor comments and typos, which I leave up to the other decision whether to take it into account or not.

We thank the reviewer for his/her thoughtful review and for his/her useful suggestion. We agree with him/her that the paper is too long and detailed. Further, we agree on shortening it to focus on information relevant to the story line. This was pointed out by the reviewer #1 as well.
Based on the reviewer's suggestion, and after some discussion with the editor, we concluded to split the paper in two parts to meet both reviewers' suggestions. Reviewer #1 suggested to omit Chapter 4 to shorten the paper while focusing on tidal incoherence. Since we do not agree with this (the New Caledonia internal tide field has never been described before and the detailed description is of relevance for different research field, e.g. biogeochemistry), we proceed as follows: Part I is dedicated to the model configuration and assessment and the description of the coherent internal tide (including SSH signature). Part II is dedicated to tidal incoherence (temporal variability of conversion, dissipation, energy flux divergence) by taking into account a deeper analysis for tidal incoherence.
We suggest the following breakdown in two parts:

Regional modeling of internal tide dynamics around New Caledonia. Part I: Tidal coherence and sea surface height observability
… focusing both on the introduction of the model configuration and assessment and the description of the coherent internal tide

Regional modeling of internal tide dynamics around New Caledonia. Part II: Tidal incoherence
… focusing on tidal incoherence (temporal variability of conversion, dissipation, energy flux divergence)

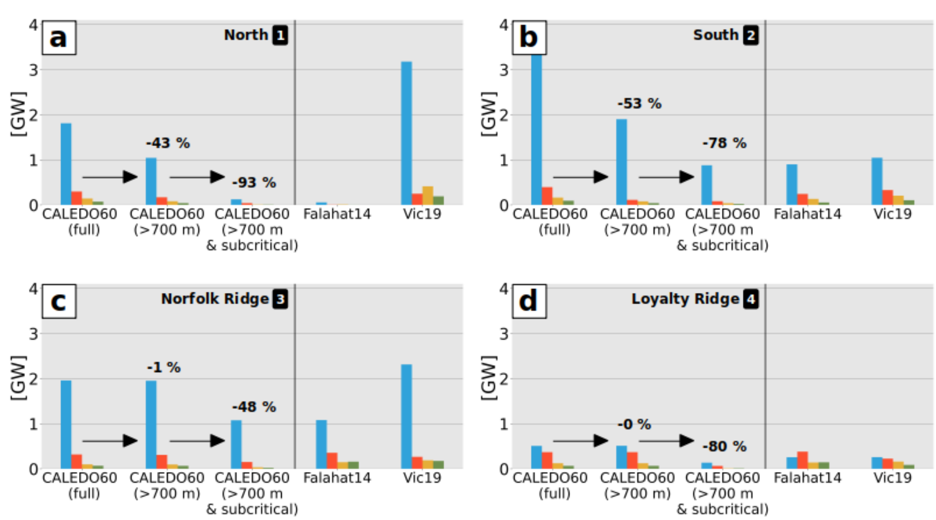
Following the editor's suggestion, Part I will be treated as a continuation of the current review process, and we would be happy to have you as a reviewer for Part II when submitted as a new submission.

In the following, we will reply to the reviewer's **major comments**. We will clearly state whether our response addresses Part I or Part II.

Organisation of section 2.2 is somewhat misleading. You are talking about the vertical mode decomposition in the "coherent tide" subsection. Besides, I think the energy equation should be applied to the full (coherent+incoherent) tidal signal, otherwise there is an additional term representing the loss-of-coherency. If the results in Figs. 7, 8 and 10 are computed using the coherent tide computed from a year-long harmonic analysis, then the high values of the inferred dissipation misleadingly includes loss-of-coherency. I would suggest either doing the diagnostics presented in these figures using the signal over the whole semi-diurnal band (which I presume you can do, given the results presented on the incoherent tide) or modifying the text to explicit this subtlety.

We agree with the reviewer's comment (which is completely in agreement with reviewer #1 comments). For clarification on this point, in the original version we first used a tidal analysis tool developed at LEGOS, Toulouse that provides the energy balance, and the modal decomposition based on the harmonic analysis. This was the starting point of our analysis. Once the analysis of the coherent tide was completed, the incoherent tide was deduced from a bandpass filtering on the full signal (subtracted by the coherent tide).
Since we focus in Part I now solely on the coherent tide, the discussion on this point is out of scope. Further, section 2.2 has been modified by suppressing the reference to incoherent tides (section 2.2.3). However, for Part II we will follow the reviewer's suggestion and section 2 will be changed accordingly.
Further, we agree with the reviewer that the dissipation estimates, as computed, may contain a significant fraction associated with the loss of coherence. However, since we remain in Part I within the context of coherent tide, we keep the dissipation term definition as it is for now, but mentioning clearly in the text now that this "dissipation" term estimate contains both true dissipation and also scattering to incoherent tide (lines 206-208 and lines 609-613). The new calculation (dissipation rates computed for the band-passed fields) will be done and presented in Part II.

- sect. 2.3.6 and around l.505: I was not able to understand whether you used the data available from the literature or did your own computation using the method of Falahat et al 2014 and Vic et al 2019. Could you please specify? In particular, is the same bathymetry dataset used



We used the data products available from literature from Falahat et al. 2014 and Vic et al. 2019; and therefore the bathymetry products differ from each other (this is now clearly specified). In the updated version, both the semidiurnal model estimates (Falahat, Vic) and our numerical model estimates were corrected for conversions in the upper 700 m. Additionally, we corrected our numerical model estimates for critical and supercritical slopes to ensure the best comparison to Falahat et al. 2014 and Vic et al. 2019. Based on this, we modified section 2.3.6 (and also 4.3) while clearly stating our objective.

- end of sect. 4.4: regarding the energy dissipation, what about the potential impact of the bottom drag, and other forms of parameterized dissipation? Do you reckon this could constitute a non-negligible contribution? If so, please mention it.

We did not discuss/consider this. I am afraid we can barely discuss this thoroughly since we didn't test different bottom drag parameterization. However, we agree that the dissipation term can be dependent on the parameterization chosen in the simulation. We mention it in the new manuscript in lines 613-615.

l.585 -- 590: it has also been shown that the presence of background currents could impact the generation of ITs (e.g. DLamb & Dunphy (JFM 2018), Shakespeare & Hogg (JPO 2019).

Absolutely, we thank the reviewer for the references, we added them in the updated version.

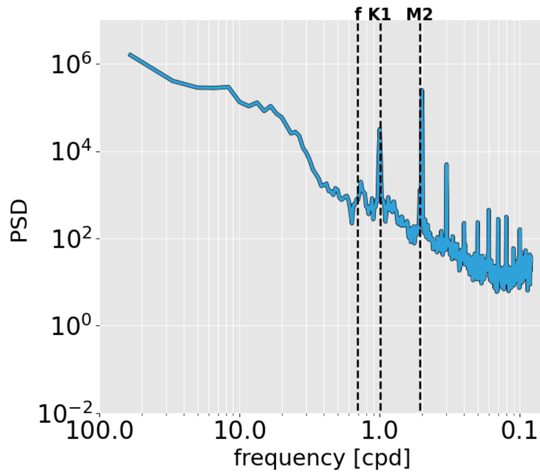
l.590-595. I found the procedure a bit puzzling. Using one month averages, you discard variability that occurs over shorter time scale, thereby not taking into account a potentially substantial impact of mesoscale currents.

We believe that there is a little misunderstanding. We did not use monthly averages. The analysis shown in Figure 12 is based on the harmonic analysis applied on monthly datasets as described in l.590-592 in the original manuscript. We used it to show the month-to-month variability of the internal tide. One month (28 days) is the minimum period needed to be able to distinguish semidiurnal or diurnal tidal constituents among each other. Eventually, following both reviewers' suggestion, this analysis might be reproduced in Part II but based on the band-passed fields.

Selected minor comments (if not commented, approved and taken into account)

Figure 4: I was surprised by the peak just above the SD frequency in the no-tide run.

This is an interesting remark that we also noted before and which we have also seen in other numerical simulation outputs. The SD peak in the blue line (located at exactly 2 cycles day^{-1}) can not be related to oceanic tidal motion (since it is in a simulation with no tidal forcing), but it is probably related to the model atmospheric forcing (ERA5). When zooming in, it is also visible in the simulation with tides and in the mooring data. We conclude that is related to atmospheric tides (atmospheric S1/S2 tides, Chapman and Lindzen 1969, https://doi.org/10.1007/978-94-010-3399-2), and more recently Balidakis et al. 2022, https://doi.org/10.1029/2022MS003193) that are linked to periodic heating of the atmosphere that expresses particularly in surface pressure oscillations. This signal is contained in ERA5 (see below the PSD of ERA5 surface pressure at our mooring location). This is briefly mentioned now in Fig. 4.



Also, blue and orange lines are inverted in the caption.

Thank you for pointing out the mistake in the caption - this is now corrected in Fig. 4.

- l. 403: why not using only the mode 1 only, or mode 1 + mode 2, for a better comparison with HRET?

We thank the reviewer for this suggestion. We agree and after some consideration, we have decided to move the comparison of the baroclinic SSH from Sect. 3.4 to Sect. 5.1. Further, we show in the new manuscript mode 1 and mode 2 for both our model and HRET.

- Fig. 6a & l.420: I would suggest adding an inset zooming over the first few hundreds of meters for the stratification profiles.

We thank the reviewer for the suggestion. We have added an inset zooming over the upper 500 m.

- Fig. 7: there are some weird patterns close to the edge of the left panel, which are hopefully plotting or diagnostic computation artefacts.

These patterns are related to the bathymetry product which becomes smoother close to the nesting boundaries (relaxation zone between nesting and host grid). This smoothing results partially in zero bathymetry gradients and therefore zero energy conversion.

- Fig. 15: for the sake of clarity, I would suggest displaying these panels in a 2x2 grid, with the legend in the fourth -- unused -- panel. Font size in the legend is too small, and curves are too steep to clearly distinguish them.

We thank the reviewer for the suggestion. In the new manuscript, this concerns Fig. 13. The legend is now increased in size and put outside the plot. Note that we removed the seasonal spectra and replaced them by the annual spectra for (i) the northern and (ii) the southern tidal beam (indicated by the red in Fig. 13a). The seasonal spectra will be presented and discussed in Part II.

- l.665 - 670: this result is interesting, and a bit surprising, as one would think that the IT would be more incoherent in winter because of a more energetic submesoscale eddy field.

It is indeed interesting, and relates to the modification of the internal tides SSH signature, which is different depending on the stratification. Further investigations are now done in Part II, and more discussions will be added.

- section 7: I think you could use proper sub-sectioning for this section.

We agree and we now use proper sub-sectioning for Sect. 6 in the new manuscript.