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)

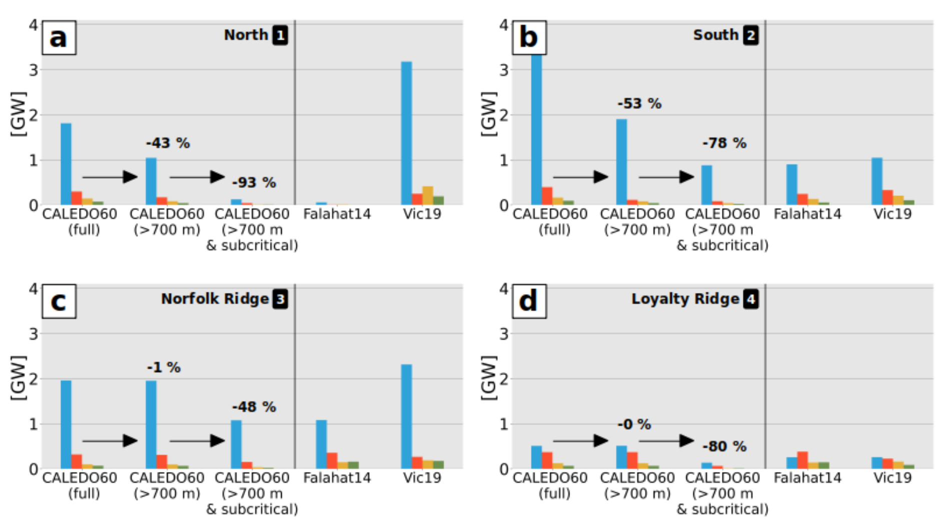
Following the editor's suggestion, Part I will be treated as a continuation of the current review process, and we would be happy to have you as a reviewer for Part II when submitted as a new submission.

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.

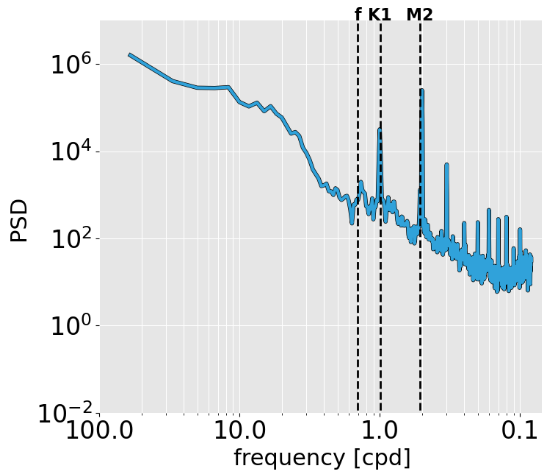
l.590-595. I found the procedure a bit puzzling. Using one month averages, you discard variability that occurs over shorter time scale, thereby not taking into account a potentially substantial impact of mesoscale currents.

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.