Response to the second review of Tchilibou et al ("Internal tide in the Solomon Sea ...")

Referee 1

The paper documents M2 internal tides amidst the Solomon Sea circulation from models with and without tides in different ENSO states and is worthy of eventual publication. The internal tides appear sensitive to stratification and mesoscale activity, which vary over one El Niño and one La Niña. Water mass transformation is greatest at the straits in the Solomon Sea, where the internal tides are generated. In my earlier review, I indicated the manuscript had not addressed 2 major points: (1) mechanisms for internal wave variability and (2) model-data comparison. The same is true of the current version.

(1) The changes are described well between ENSO states, but the reason behind them is asserted to be due to mesoscale eddies or vertical stratification. The authors are likely correct, but nothing is proven. The relevant terms could be calculated from the model results, but the authors contend this is beyond the scope of this work. The path by which baroclinic energy changes is not identified despite having a model available and all terms available to diagnose these changes. Is stratification affecting p' at the generation sites, mesoscale currents refracting internal waves along their propagation paths, or something else? If the authors are unwilling to determine what factors are affecting the internal waves, then they should clearly identify in the text what is proven and what is speculation or coincidence: ie, Stratification and mesoscale eddy activity change with ENSO, internal tides change with ENSO, and due to this coincidence we speculate these ENSO-related changes affect the internal waves.

We understand the interest of the reviewer to know more about the mechanisms by which baroclinic energy changes, and especially the importance of analysing the respective role of mesoscale variability against stratification. Some papers are specifically dedicated to such purpose. We can cite the papers by Zilberman et al. (2011) and Zaron and Egdbert (2014) that investigate the physical terms at the origin of internal tide variability by mesoscale activity and changes in stratification. But it is not the focus of our paper, especially since, the 3 months period of our simulations is not long enough to clearly investigate this question. The changes in stratification may be due both to low frequency and/or mesoscale variability and the short 3 month period covers only 1-2 decorrelation time scales of these eddies.
This aspect is only discussed in sections 4b and 4c, where the discussion is based on Figures 3, 4cd, 5, 8, 10 and 11. The difference in including tides between the El Nino and La Nina cases are highlighted in Figure 8, 10, 11, and are discussed with regard to the mesoscale and stratification conditions shown in Figure 3, 4cd, 5. At this stage, our discussion is not a pure speculation but we agree that we don't clearly separate the factors affecting the internal tides. In the first response to the review, we have added some figures on EKE and APE. We have completed these figures with those for the pressure anomalies, as suggested in the 2nd review. But these extra Figures, based on a 3-month relatively stationary mesoscale field, do not bring any substantial information on the dominant processes at work. This would require a specific study by running a longer simulation to do more rigorous energy budgets, in order to better separate the effects of the time-varying eddies in order to learn more on the role of the low frequency changes. This is why we say that these types of diagnostics are beyond the scope of our present paper, based on these limited period simulations.

We are aware of the limits of our discussion, and the text has been modified taking into account this limitation. The last sentence of the abstract has been modified to be less conclusive. :

> « Finally, the extreme ENSO condition case studies highlight the dominant role of local circulation changes, which have a larger effect than the stratification on the tides. »
>
> →
>
> "Finally, the extreme ENSO condition case studies suggest the dominant role of local circulation changes, as well as stratification changes, in modifying on the internal tides."

(2) The realism of the model's internal tides are also far from proven in the manuscript. Some relatively simple comparisons would help. Internal tide values produced in the model are not compared to readily available observations from Zhao e.g., https://doi.org/10.1029/2018JC014475). Maybe just comparison of mean internal tide energy levels or averaged energy density at the relatively coarse resolution of the altimetric internal tides in the Solomon Sea. In their response, the authors provided some comparison between a mooring, the Honiara tide gauge, and their model. This comparison would also be worthwhile to include to improve confidence in the model. Amplitude and phase of the modelled and observed M2 signal for the mooring and tide gauge could be compared in a table or pretty quickly in the text. Also the manuscript asserts, "Our modeling results show that the diapycnal mixing induced by the internal tides is particularly useful to erode the salinity maximum of the upper thermocline water," but I do not see any estimate of an eddy diffusivity (K) or comparison of model K to observed fine-scale K parameterisations (Alberty, 2017) or previously modelled K (Melet 2011).

We understand the reviewer's concern about validating the model with tides. We have provided a comparison of the coherent SSH signal from our model, compared with the altimetry analysis of Ray and Zaron for this region. It is not possible to give an exhaustive validation, since the altimetry analyses are from a 20-year time series, and our 3-month hourly modelling runs clearly do not cover such a long period. We cannot calculate the phase-locked internal tides from the daily averaged 3-year runs. The reviewer suggests to compared to Zhao's altimetric results that have the advantage of providing information on both mode 1 and mode 2 M2 internal tides, but these estimations are also computed over the whole altimetric period, and in this work we show large variations in internal tide energy between both El Nino and La Nina conditions (Figure 8, also see Figure 4 of the first response). Therefore we don't think that the extra validation with Zhao's result will provide any more information that is really significant.

About the in situ validation, the reviewer suggests to include it in the paper. Although both in situ and model results are in relatively good accordance, there are only a limited number of in-situ sites spanning the model period, and they are often in island regions with complex bathymetry, so its difficult to make robust model-data comparisons. The Honiara tide gauge includes both barotropic and baroclinic components so we don't learn a lot about internal tides that are of interest here. Also, the Honiara's tide gauge is located in a complex area surrounded by several islands and shallow topography and we cannot expect an exhaustive comparison. For the mooring, it is difficult to show the comparison without a detailed explanation of the mooring data, the processing and the method used to compare it with the model : A long paragraph for a limited result. This intercomparison is included in the PhD thesis of Tchilibou (2018), and we have included this reference. But we have decided to not include it in this manuscript.

By comparing the model with and without tides on isopycnal layers, we suggest that the diapycnal mixing induced by the internal tides is particularly useful to erode the salinity maximum of the upper thermocline water. However, this is only presented as an interesting example, we have not investigated these mixing processes quantitatively, only the result of this mixing on the tracer field along isopycnals. It would be interesting to pursue this in a further study, but it is beyond the scope of this present paper motivated by the SWOT mission to estimate any diffusivity in relation with Alberty et al. (2017).

To help address these concerns of the reviewer, we have also added a final paragraph to the discussion/conclusions :
"

> Finally, we note some caveats. Our 3-month hourly simulations in contrasting ENSO conditions represent examples of particular ENSO events, over one season in each case, and including a slowly-varying mesoscale field. Longer simulations covering more interannual events are needed to better understand how the internal tides may be modified under varying ENSO cases, and to better separate the role of mesoscale variability interacting with the internal tides. We have thus not attempted to quantify the energetics of these tide-circulation interactions with such short time series. These short model simulations are also difficult to validate, since in-situ data are scarce, and longer time series are needed to build up robust internal tidal signals from 10 or 35-d altimetric sampling. Future work using longer model simulations, compared to swath observations of the 2D internal tide structure from the future SWOT mission in 2022-2025, should give us a more quantitative picture of the interaction of the ocean circulation and internal tides in the Soloman Seas. "

My minor comments from the earlier review were mostly addressed.

Some further small comments by line number:

- σθ has units
OK

94 - Thorpe scale is capitalized and there are several other capitalization mistakes throughout the manuscript
Ok, we check

97 - 4.1-23 10-8 [W kg-1] - no brackets needed for units
Yes

252 - ∇hH
Ok

265 - ∇h is the horizontal gradient (∂x, ∂y) operator. (∇h ·) is the divergence operator
Ok

Fig 5 - label x axis with units

Ok

Many figures- The repetitive references to the topography are overkill and can be placed instead in the methods, acknowledgements and/or cited in the references or at least shortened considerably- Bathymetry as in Fig. 1. SI convention should be used, e.g., W[space]m-2 and other units too. Also it's good practice to label the colour bar with EKE or whatever is plotted and not just label with units. I realize the editor would like the bathymetry reference but I have never encountered this with any other journal. Sometimes the bathymetry part is longer than the actual caption. By similar logic, anything involving the CARS database must also be similarly referenced.

Yes, we agree with you about the bathymetry reference but it is the editor's will.
The labeling of the figures have been changed

441 - "higher dissipative modes" - no evidence to support them being dissipative. How about just higher modes?

Ok, the text has been modified

Fig 9 and 10 - some spurious vectors in the baroclinic flux and illegible arrows in the barotropic flux. Consider scaling or averaging over some number of adjacent cells.

Yes, the figures has been improved

Table 1 has inconsistent notation in the headings.

Headings are changed in accordance with the expression in the text.

588 - 0.06 psu

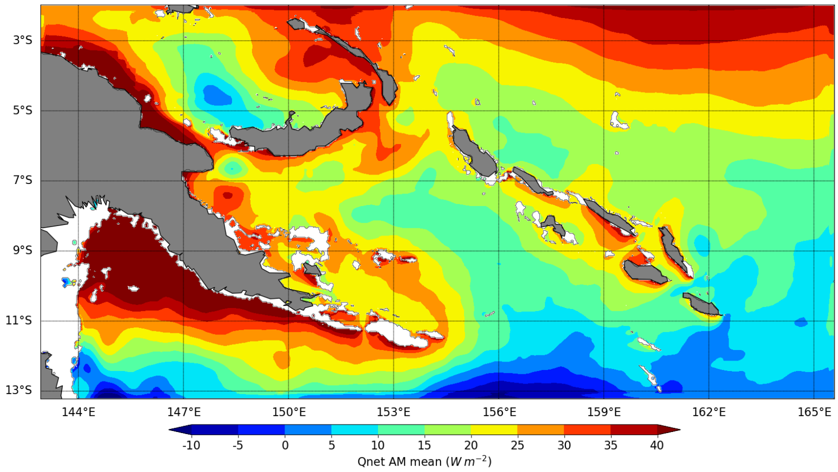
Ok

628 - What is the value of Qnet? What fraction of the incoming solar radiation is it?

The mean Qnet and the difference TIDE-NOTIDE of Qnet is shown on the plot below. It is positive in most part of the Solomon Sea and the distribution of the differences is in accordance with the difference in SW temperature (Fig. 13). Over the Solomon Sea, Qnet varies from 10 to 30 W m^{-2} and the differences reach 10 W m^{-2} in area of high Qnet, that is a 30% change. Averaged over the Solomon Sea, we estimate a 15% change of Qnet.

The text has been modified :

« This corresponds to a positive Qnet anomaly between the simulation with and without tides that matches the pattern of SW temperature difference in Figure 13, and represents a 15% increase in Qnet when tides are included. It acts to reduce the SST cooling induced by internal tides. »



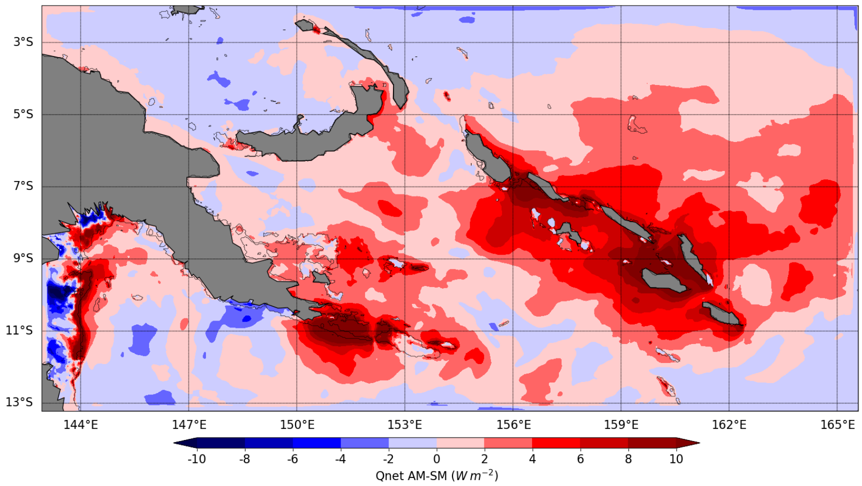


Figure : Top) Qnet flux of the TIDE simulation. Bottom) TIDE-NOTIDE difference of Qnet.

Referee 2

Overall comment:
The revision is much improved. The authors' responses are comprehensive and convincing. I am happy to see that the review process prompted the authors to go over their calculations and identify an error (re Fig.14). Their rewrite of section 5 is excellent and clear. The figures are much better. In my opinion, the paper is now very close to acceptance. I have only minor suggestions for the authors to consider as they prepare their final version.

We are pleased to read that the reviewer is now satisfied by this new version, and we thank him very much for this improvement. We thank him again for the careful reading and English corrections.

Specific comments/suggestions:

Re the authors' response:

Fig.1 of the response to R1 shows Honiara tides, but is labeled 2S,147E. Honiara is actually at 10S,160E. Just a mislabeling?
You are right, the location 2S 147^{E} is wrong : it is Honiara tides

Figs.3-4 of the response to R2 are very interesting. Is a figure like 3 published anywhere? (no reference is given where the result is stated on L392)
You can find a figure like 3 in the Tchilibou's thesis (its Fig. 8.6). We add this reference in L392.

Minor comments:

L29: 'where' is unclear. As written it appears to refer to the equator, but I think you mean the Solomon Sea. How about:
'Intense equatorward LLWBCs transit the Solomon Sea, where active ...'
You are right, the text has been modified.

L30: 'constraints' is not the right word here. Also, is this 'dynamical'? How about '... the mixing induced by these features can play a role...'
Ok, the text has been modified :
« In this marginal sea, the mixing induced by these features can play a role in the observed water mass transformation. »

L36 and elsewhere: Should say 'Vertical (modes)'. Especially in the abstract, the reader may not know what kind of mode is referred to.
You are right, we change mode by vertical mode when necessary.

L44: 'far from the strong currents' => 'in quieter regions'
Ok, the text has been modified

L50-51: The last sentence is unclear. I think you are trying to say that the local circulation is a stronger influence on the tides than the stratification, right? How about:
'dominant role of local circulation changes, which have a larger effect than the stratification on the tides'
Yes, the text has been modified

L73: I'm a bit embarrassed to pitch my own work, but in a sentence listing work describing the circulation of the Solomon Sea, probably Kessler, Hristova and Davis (2019, Prog.Oc.) should be listed. Perhaps especially on L714-715 where the issue of the merging of the NVJ/SEC into the NGCU is a major topic of that paper.
Yes, you are right. We add this reference.

L290: 'We verified' => 'Inspection of the model velocity and temperature showed that...'.
Ok, the text has been modified.

L298-302: This sentence is unclear. It raises several questions:

- Why does the bathymetric control at Vitiaz have a stronger effect during El Nino? I thought the effect was that it limited the amount of water that could be pushed through Vitiaz, thus the El Nino flow increase through Vitiaz is not as large as it would be if the strait was wider. That does not seem to be 'a stronger effect'.

Yes, that is the meaning of our sentence, and the effect of this bathymetric control is stronger during El Nino when the flow increases than during La Nina. The text has been modified :

« …because of a bathymetric control at Vitiaz Strait particularly noticeable during El Nino when the NGCC intensifies… »

- Although the Melet result seems correct, it was one experiment with one model. I keep hoping someone will try this with another model, or at finer resolution. In any case it would be worth noting this: 'Some model experiments suggest ...'. We have no observational confirmation of this.

Yes, the text has been modified :

"This may be due to the bathymetric control at the narrow Vitiaz Strait that limits the outflow of the stronger NGCC during El Niño, whereas the additional inflow through the wider Solomon Strait flows more freely during La Niña when the NGCC weakens (Melet et al., 2013)."

L607-8: The difference between CARS and either model run east of the Solomon Sea is more than a shift in latitude (Fig.12). It is described as a bias on L602, which seems more accurate.

- Is it timing? CARS is an average over decades, right? Thus the difference might be temporal, as suggested by the CORA05 comparison.

Yes, that is what we mean when we write :

This comparison with the long-term CARS climatology has some limitations with regard to the particular conditions of our 3-year simulation including strong El Ni.o and La Ni.a events. ». This includes both the bias and the shift. And it is the reason why we write about results from the CORA05 database.

This is not crucial to the paper, but it is unfortunate.

I especially wonder about the S-max just south of New Britain (6.5S,151E) in CARS, that does not appear in either model solution (Fig.12).

Yes, we agree with your point but at this point we can only speculate. This salinity maximum corresponds with a recirculation zone that takes place most of the time and that is supposed to be influenced by high salinity waters of the SSI inflow (Gourdeau et al., 2014, 2017). But we're running out of arguments to discuss this point

L639: How was the transit time determined? That would be an interesting calculation to do.

Here, we make reference to Melet et al. (2011). The transit time is estimated by the time of Lagrangian particles to enter and exit the Solomon Sea. The transit time varies between 50 and 100 days depending on the pathway of the particles. We add this reference.

L643: 'within' => 'averaged over' (isn't the point here that averaging over the different impacts east and west gives a small value because those have opposite signals?)

No, as we use it, within doesn't mean « averaged over » but just « in ». The text has been modified :

« …with maximum differences in the Solomon Sea… »

Fig.10: Perhaps use the same scale for the two modes so the difference would be obvious. There is no reason that the details of mode 2 need to be very visible. At least note this in the caption.

Yes, you are right, the color code has been changed

Fig.12 needs a vector scale. It looks like the vectors are the same for both panels a and b But Fig.14 suggests they are substantially different. Please check.

Yes, we add the vector scale in Fig. 12. Note that in Fig.12 the vectors are for the UTW waters and in fig. 14. For SW waters.

Fig.14: This is a very informative figure. It looks like the temperatures are differences, but the velocity vectors are totals. The caption should say this.

Yes, you are right, the caption has been modified.

Is there a way to say or show how large the total temperature changes are between El Nino and La Nina? That would give context to the values shown here. If you don't want to add two more panels, perhaps just say something like 'While the SW temperature differences between Tides and NoTides are about 0.5C, the changes between El Nino and La Nina are about X', and are concentrated in the region Y'.

Yes, what you suggest correspond to the Fig.14 of the initial manuscrit showing the temperature temperature anomalies referenced to the 3 year mean during El Nino and La Nina. But we remember that the files was wrong.

For clarity, we do not want to add a figure but we add a sentence like you suggest :

« The tidal differences are weaker in the La Niña condition with a temperature difference of 0.018°C compared to 0.05°C for El Niño when averaged over the Solomon Sea. These values are of same order as the mean tidal effect (e.g. 5b) but they are an order of magnitude smaller that the changes between El Niño and La Niña (0.8°C, e.g. 5a).

Minor notes of English usage and etc (the English is generally very good):

L30: cumulatED => cumulatIVE
The text has been modified

L39 and many other places: Don't use season names (fall, summer) which are ambiguous (especially since this is the southern hemisphere!). Does OS allow, e.g., JFM, etc? That would be clearer.
Done

L40: 'complexity OF predicting'
Done

L106: 'some' => 'a few' (goes better with 'specific locations')
Done

L110: 'A lot' is too colloquial (familier). => 'Several studies have focused ...'
Done

L227: 'we will use the vertical mode decomposition (3) to define...'
Done

L243: The verb 'resume' in English means 'reprendre'. => 'The equations can then be written'
Done

L288-289: 'summer', 'fall' again. (Also L665).
Done

L371: Is it necessary to use the abbreviation 'SSI'? I had to go back to find out what this meant. It seems to be used infrequently enough that it could just be written out.
We use abbreviations for the different currents, so we think that SSI makes sens here. Also this abbreviation is also used in others publications (Gourdeau et al., 2014, 2017..), but you are righ it is infrequently used so we recall what is it when we discuss about it.

L445: 'drastic' is a very strong word. How about 'substantially'?
Done

L592: 'closeR' (need the 'r' since this is a comparison)
Done

L594: 'discrepancy' is not the right word; it implies that something is wrong. => 'difference'
Done

L611: '..., but inside the Sea only long-term comparisons are possible'

Done

L612: 'on' => 'in'
Done

L627: 'that in turn affects' (not 'return')
Done

L630: omit 'of'
Done

L664: 'is under the influence of ENSO' => 'is influenced by ENSO'
Done