Review of manuscript 2023-418 *Internal tides off the Amazon shelf Part I: importance for the structuring of ocean temperature during two contrasted seasons*, by F. Assene et al.

Manuscript reviewed by Clément Vic on 25th August 2023.

This is my second review of the manuscript. The authors brought significant changes to the manuscript as compared to the initial version. They addressed most of my comments favourably. The manuscript has improved and the science is more clearly presented, although the manuscript contains unnecessarily long (and sometimes not relevant) paragraphs. I think the length of the manuscript (770 lines) could be reduced to ~500 lines while gaining in clarity. Importantly, I still have one major comment regarding how the heat budget is derived – I am confused with some terms. Also, the manuscript would strongly benefit from an in-depth proof-reading by someone knowledgeable in English writing. I tried to give some advice on writing but could not do it thoroughly throughout the whole manuscript.

Major comments

- **Major comment #1:** Regarding section II.3.2, I do not understand how the term ADV is broken down into the terms called ADV* and Non-Linear terms. I would like the authors to clarify the following points:
  - The Reynolds decomposition is not explained. I am not sure how the averaging operator is defined. If the decomposition is something like *low-frequency signal + tidal signal*, how is it computed online?
  - In the classic Reynolds decomposition, the terms $<U'.\text{grad}(T)>$ and $<U.\text{grad}(T')>$ should vanish and one is left with the turbulent term $<U'.\text{grad}(T')>$.
  - How is the term Numdiff_ADV computed? Is it what is called "residual" (line 238)? Then, I guess it is computed as the residual of equation (6)? Is it a diapycnal or isopycnal term? Then, does it belong to ADV or to ZDF? Lines 594-597, it is written that the numerical dissipation of the diffusive part of the advective scheme belongs to z-ADV.

  Also, at the end of the manuscript, I did not have a very clear idea of the different contributions from the diffusive and advective terms. I think it would be good to compute the overall contribution of each term, averaged in a box or two boxes, inshore and offshore of the shelf break.

- **Major comment #2:** Section III.2. There is a strong temperature bias to the northwest where the NOTIDE simulation performs better than the TIDE simulation – see Figure 1 in the present document. It should be discussed. Also, Figure 3f, NOTIDE performs better than TIDE for 7 months (March-July and Sept-Oct), which is more than half of the year. Overall, this is not clear that including tides improves the quality of the simulation.

- **Major comment #3:** The manuscript would strongly benefit from tightening. Make sure that all statements are backed up either by a Figure or a reference, and avoid speculative and confusing statements. I think some of the discussion could be moved to the result section. For example, the discussion on mode-1 wavelength related to dissipation hotspots (L. 517).
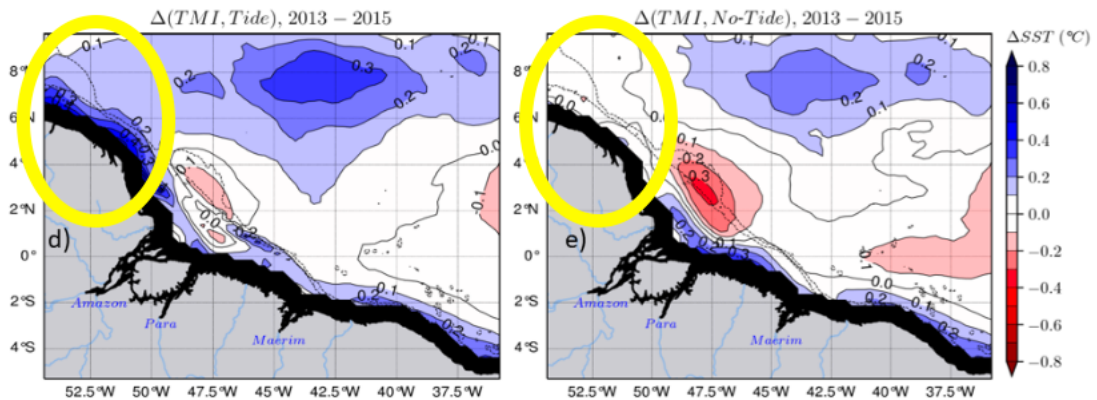
Figure 1: Partial screenshot of Figure 2 in the manuscript. Within the area circled in yellow, the NOTIDE simulation has a smaller bias than the TIDE simulation.

Minor comments

- L. 15: Use "baroclinic and barotropic" or "internal and external" but try not to mix them together.
- L. 16: contrasting -> contrasted
- L. 17: Twinned -> Twin
- L. 19: thaw ? you mean warm ?
- L. 20: avoid using "could" or "may" etc. when the conclusion is clear cut.
- L. 27: remove "horizontal propagation"
- L. 29: we found THAT
- L. 32: what are the first and second seasons?
- L. 38: The sentence reads bizarrely. Temperature (per se) is not a mechanism so I am not sure one can say that it plays a role in ocean dynamics.
- L. 55: remove "also called internal tidal/gravity waves" and "may"
- L. 57: remove "The precise location of this dissipation is a big unknown" as I do not think this is true anymore. Many efforts have been carried out in the recent years to narrow down uncertainties and map tidal energy dissipation (Kunze 2017, MacKinnon et al. 2017, de Lavergne et al. 2019, 2020, etc.)
- L. 62: "encounter others" you refer to wave-wave interactions?
- L. 64-71: The links between ISW, linear and non-linear waves are unclear. Also, I do not think that all the information is relevant to the paragraph. For example, the phase-locking of ISW with IT troughs brings confusion. Also, that ITs advect water masses along their propagation is very surprising to me. Do you have a reference to back this up? If so, it needs to be developed as this is quite an unusual mechanism (most of the time, as far as I know, internal waves are close to linear and do not perform any advection). If not, please remove.
- I would move paragraph from L. 103 to L. 112 to L.72 as it describes some mechanisms regulating temperature variability and would be logically articulated towards the beginning of the introduction.
- L. 83: typo retroflection
- L. 87: "possesses"? features?

- L. 93: Sentence lacks a verb.
- L. 96: I do not see why incoherent / non-stationary ITs are brought in here. The rest of the paragraph (L. 97-102) is not related to the present study, it should be deleted.
- L. 114: enterS. Runs?
- L. 125: the previous questions are not explicitly written. It would be good to repeat them explicitly in this paragraph.
- L. 135: remove "used"
- L. 136: IS derived?
- L. 140: I do not think acronym RSS is used later; it should be removed.
- L. 152: remove "can"
- L. 153: remove sentence starting with "Unlike…"
- L. 164: "the vertical diffusion coefficient" -> vertical diffusion (it parameterizes the vertical diffusion through a coefficient)
- L. 170 and 174: move http to references?
- L. 177: (i) and (ii) are confusing. You mean that SSH and U,V are prescribed?
- L. 182: "derived"? through thermal wind balance?
- L. 184: "3-years" -> three-year (no s as this is an adjective)
- L. 185: remove "of run"
- L. 186: remove "the" before tides
- L. 188: remove "used in this study"
- L. 188: what is the "current's circulation"?
- L. 193 and 194: what are "vertical propagation modes"? I think you refer simply to vertical modes.
- L. 194: "harmonic" -> tidal frequency?
- L. 196: remove sentence starting with "Even…"
- L. 200: remove "as a first order approximation"
- L. 210: I do not understand "the energy loss of other tidal harmonics".
- Eq. (3)-(5): what are the asterisks referring to?
- L. 221: remove "and emphasizes the pathway of the respective tides (external or internal)"
- L. 230: "velocities component" -> velocity components
- L. 252: "advection diffusion"? please clarify
- L. 252: typo expect
- L. 258: Why is FOR_z not shown? Is it negligible?
- L. 262: remove "model's"
- L. 265: remove "for the period"
- L. 267: remove "at"
- L. 268: amplitude and phase are not shown in Fig. 2. Please make sure that the figures are adequately referred to.
- L. 270-272: show maps with relative differences?
- L. 278: Is it really the Mid-Atlantic Ridge? To me this is still the Brazil Basin… Maybe show deep (4000 m? 5000 m?) contours of bathymetry.
- L. 279: "It is worth noting…" this is not investigated in the study and should be removed. Also, L. 282, I am not sure that comparing NEMO and FES only through their resolution is relevant as they are very different models, resolving different sets of equations.
- L. 284: "later"?

- L. 288: "flows" -> propagates
- L. 296: remove "once generated". Also, importantly, I am not sure there is straightforward link between slope criticality and the direction of propagation. If you do not have a reference for this statement, please remove.
- L. 302: "for the" -> over
- L. 302: remove "can"
- L. 304: is q shown anywhere? Does it bring any piece of information relevant to the study?
- L. 309: "extensive" -> important?
- L. 311: "extensive" -> pronounced?
- Fig 2g: there are horizontal stripes on the shelf. I guess they come from the graphic treatment of the data that are likely missing shoreward of a given distance. This area should be masked.
- L. 322: As far as I understand, the Reynolds decomposition gives you the coherent part of the M2 tide? If so, it should be explicitly written.
- L. 324: "neglectable" -> negligible.
- L. 325: I do not see any logical link ("which means") between seasonality and the cumulative effect of coherent and incoherent tidal harmonics.
- L. 329: remove "it should be noted"
- L. 330: "into" -> onto
- L. 339: TIM -> TMI
- L. 342: remove "corresponding"
- L. 355: remove "hereabove and elsewhere"
- L. 357 and elsewhere: remove "(black line)" (etc.) as this belongs to the figure caption.
- L. 362: "eroded"?
- L. 362: "petty" -> small
- L. 367: "heat" -> heat exchange?
- Figure 4: could you show the difference between seasonal TMI data and model data? I find difficult to follow the discussion in section IV.1.
- L. 381: I do not understand the link between tides and a speculative upwelling.
- L. 403: I am not expert in air-sea interactions but is it really unexpected that a negative SST anomaly induces a positive Qt anomaly?
- L. 406: remove sentence starting with "As it…"
- Figure 5e: "corr coef" is R? Is it worth showing R^2 then?
- L. 442: remove sentence starting with "Note that…"
- Section IV.3. is quite descriptive and lacks some kind of dynamical interpretation.
- L. 460: remove "therefore"
- L. 475: remove "Stratification"
- L. 476: "isodensities" -> isopycnal layers?
- L. 478: remove "the isodensities are thicker…" and "As a result of this"
- L. 482: "ITs and likely the barotropic tides" should be referred to jointly as "tides" as the analysis does not allow to separate them.
- Figure 8 and associated discussion. Why showing only the 2-20m contribution? I think that internal tides do not formally exist in homogeneous mixed layers and will break preferentially where N2 is large (see references in de Lavergne et al. 2020). It would be more relevant to show the variables integrated over larger depths, maybe down to the thermocline.

- L. 505: remove "Whatever"
- L. 509: "inverted"?
- L. 512: "extension" -> extent?
- L. 534: remove "waves"
- L. 561-562: this is not obvious, and the overall view on the temperature budget would benefit from showing integrated values in boxes (see Major Comment #1).
- L. 572: I do not see a clear link between a tidal residual mean transport and a cooling/warming effect.
- L. 590: I do not understand the last sentence of the paragraph.
- L. 595: "nonlinear effect between the temperature and the currents" are you referring to IT breaking?
- L. 600: "velocity of the (mode-1) internal tidal waves is maximum in the thermocline"? I do not think so, the vertical group speed is maximum where the stratification is weak.
- L. 601: "working harder"?
- L. 629: ISW and mixing. This is a bit speculative and not properly addressed in the study. Please remove.
- L. 704: remove sentence "This hits the …"

References

de Lavergne, C., Falahat, S., Madec, G., Roquet, F., Nycander, J., & Vic, C. (2019). Toward global maps of internal tide energy sinks. *Ocean Modelling*, *137*, 52-75.

de Lavergne, C., Vic, C., Madec, G., Roquet, F., Waterhouse, A. F., Whalen, C. B., ... & Hibiya, T. (2020). A parameterization of local and remote tidal mixing. *Journal of Advances in Modeling Earth Systems*, *12*(5), e2020MS002065.

Kunze, E. (2017). Internal-wave-driven mixing: Global geography and budgets. *Journal of Physical Oceanography*, *47*(6), 1325-1345.

MacKinnon, J. A., Zhao, Z., Whalen, C. B., Waterhouse, A. F., Trossman, D. S., Sun, O. M., ... & Alford, M. H. (2017). Climate process team on internal wave–driven ocean mixing. *Bulletin of the American Meteorological Society*, *98*(11), 2429-2454.