***Reply to the referees*' comments: Manuscript 2023-418 – Assene et al.**

First, we are grateful to the reviewer for the time spent to carefully review our manuscript and for their constructive comments.

In the following, the referees' comments are presented in black color and our responses are in blue color. OM refers to the original manuscript, whereas RM refers to the revised manuscript.

**Response to reviewer 1:**

Manuscript reviewed by Clément Vic on 5th April 2023

The authors investigate the impact of tides on the vertical and horizontal structure of temperature in the Atlantic Ocean offshore of the Amazon shelf. They use twin regional simulations of the oceanic circulation where tidal forcing is switched on and off. They find that the tides tend to cool down the ocean above the pycnocline, leading to a more realistic temperature stratification compared to observations. The analysis of a simplified heat budget leads to the conclusion that vertical (diapycnal) mixing is the process that drives cooling. The intensified mixing in the tide simulation is attributed to breaking internal tides that originate from the shelf slope. Impacts on ocean-atmosphere interactions and regional climate are discussed. The results are interesting and worthy of publication, although they would deserve to be strengthened, notably through more thorough comparison with observations and further quantitative diagnostics. Also, I found the manuscript difficult to read and unnecessarily long. I believe it could be tightened and more focus. I personally refer to these guidelines :
https://aslopubs.onlinelibrary.wiley.com/doi/epdf/10.1002/lol2.10165 if that can help. For these reasons, I recommend the manuscript for publication after major revisions. In the following, I tried to come up with suggestions to improve the robustness of the results and readability of the manuscript.

**Major comments**

1. Introduction is overly long and poorly structured, with many paragraphs containing entangled concepts that are not relevant to the storyline of the manuscript. Here are three examples of irrelevant information:

   *The introduction has been squeezed and restructured following the reviewer comment.*

   (i) lines 75-76, the waveguide concept is not used in the manuscript so it should not be mentioned;

   *The wave-guide concept was mentioned by the author to point out how the ITs displace the thermocline. What is seen in the section IV.4.2. However, it is not used in the manuscript and has been removed.*

   (ii) line 145, the coherence vs incoherence of internal tides is not affecting the results on surface layers cooling;

   *The concept was used to give more ITs characteristics, but it's not relevant for our result, and it has been removed in the RM..*

(iii) line 153, sediment transport is not further addressed in the manuscript. I recommend the authors to restructure and tighten the introduction following a classic plan: introduction of concepts and review of the literature (tides in general, lifecycle of internal tides, mechanisms for temperature cooling, the regional circulation, …), identification of a question that has not been addressed or fully answered (here it seems like Tchilibou et al. (2022) previously hinted some of the results so perhaps try to explicitly build upon their article), how they are addressing it, and very briefly mention the key results and perhaps the limitations.

*The sediment transport is also a complementary characteristic of ITs, but we agree with the reviewer to remove this in the RM.*

*Otherwise, regarding Tchilibou et al. (2022), they only addressed ITs characteristics and not focus on how they can affect temperature or other oceanic tracers. It's therefore in a way useless to start from that point.*

*We thank the reviewer for the orientation given for the introduction (re)writing.*

2. I counted 11 occurrences of "not shown". I find this quite annoying since most of the results are qualitative, based on visual inspection of plots, and do not rely on quantitative analyses. I suggest to remove most statements not backed up by a figure.

   *We followed the reviewer's advices. The text has been modified, and some figures have been included in the main text and appendix of the RM.*

3. I am not sure that section II.3.3. is necessary, as the different terms of equation (7) are not discussed in the manuscript.

   *The section II.3.3 was added to give the reader more clarity about the different components of the total ocean-atmosphere heat flux and their dependence to the SST. So we believe that this information is useful for the reader and did not change this section in the RM. However, we removed this information and refer to related reference (Moisan and Niiler, 1998; Jayakrishnan and Babu, 2013) if the reviewer think that this information is not relevant.*

4. Line 345 and equation (8). It seems to me that P is the fraction of locally dissipated energy (usually labelled "q", following St. Laurent et al. (2002)), but this is not the content of Figure 2f. This should be clarified.

   *The figure 2f is the depth-integrated baroclinic energy dissipation for M2 frequency, which is different from the local dissipation (of this energy).*

   *The term 'P' is used in the OM for local dissipation, as in Tchilibou et al. (2022) (Appendix A, equation A2). The reviewer is right, the term 'q' is more suitable. The term 'P' has been changed for the term 'q' in the RM (Line 305).*

5. This is not totally clear that adding tides to the model improves the modelled SST as compared to observations. Line 364, it is suggested that the tidal simulation is too cold. I wonder if the authors could come up with a robust comparison of the tidal and non-tidal model temperature with observations, at least through maps (also show the

difference model vs observations), vertical sections (again, show the difference) and time series of averaged temperature over a small domain, offshore off the continental shelf (e.g., adjusted from Figure 3, discard shallow depths).
*We added more relevant comparisons between both simulations with and without tides and the observations.*
*Cela serait bien de mettre quelques figures dans la réponse et de dire si l'ajout des IT améliore la SST comparée aux observations.*

6. Lines 513-521: the discussion on the impact of winds is speculative and not backed up by any diagnostics. I suggest to remove this paragraph.
*The authors didn't deeply focus on the effect of the wind, this discussion is an assumption given to try to explain what observed at the surface layer. But The authors agree with the reviewers to remove the paragraph in the RM.*

**Minor comments**

1. Line 53 and elsewhere. "tides and internal tides" are mentioned several times, I suggest to refer jointly to "tides" as the effects of barotropic and baroclinic tides are not addressed separately.
*We agree with the reviewer. "Tides" in the text will therefore refers to "barotropic tides and baroclinic tides" in the RM. We now refer to Its only for baroclinic tides in the RM.*
2. Line 59. Remove "but with higher vertical velocities"
*It has been removed in the RM.*
3. Line 62. Wrong reference to Zhao et al 2012, please remove. Also, in general, try to avoid putting references in the middle of sentences.
*The reference was removed and the authors try to follow the reviewer recommendation in the RM.*
4. Line 62. Wrong use of "i.e."
*Removed in the RM.*
5. Line 66. Remove "which can be understood as a tidal energy cascade" as there is no analysis of the energetics of tides in the manuscript. In general, I really believe that the manuscript would benefit from removing all statements that are not thoroughly addressed and/or backed up by a reference or a diagnostic. Readers would be less distracted and would grant more credit to the results.
*It has been removed in the RM. We agree and thank the reviewer for his relevant comment.*
6. Line 69. Zhao et al (2016) addresses the propagation of mode-1 internal tides, but not the mixing effects of low vs high modes. Replace with, e.g., St Laurent and Garrett (2002) or Vic et al (2019) or a review article, e.g., Whalen et al (2020)?
*Yes indeed, it is a mistake. We follow the reviewer advice and replace the reference in the RM (Lines 59-60).*
7. Line 73 and elsewhere. The use of "advection" is ambiguous as linear internal tides do not induce a net advection. They propagate energy but do not lead to transport. I suggest to remove statements that mention any "advection by tides".
*Thanks for pointing out this important misleading phrasing.*

*You are right, tidal advection should not affect the temperature as the result of the advection is null, except in some tidal residual circulation. But we verify that in our region the residual tidal circulation is quasi null (Bessières et al. 2008)*

*What we meant, and we clarify it all over the document, is that, in the model, the advection term leads to some diffusivity of the temperature due to numerical dissipation of the advection scheme. In fact, in a non-diffusive advection scheme such as in Leclair and Madec (2009) we would not see any response in our x-ADV, y-ADV, z-ADV terms. In our case, we are using the FCT advection scheme (**Zalesak, 1999**) that includes a diffusive part, that is what we are seeing in our diagnostics, which is expressed as follows:*

$$ADV = \underbrace{\langle U \cdot \nabla T \rangle}_{ADV*} + \underbrace{\langle U' \cdot \nabla T \rangle + \langle U \cdot \nabla T' \rangle + \langle U' \cdot \nabla T' \rangle}_{Non-Linear\ terms} + Numdiff_{ADV}$$

*There are 3 important points that we clarify in the Final Manuscript. The tidal impact on temperature is resumed by : U'dzT+UdzT', where U' is the tidal current, and T' is the anomaly of temperature that is produced by the tides apart the advection.*

*1) Over one tidal cycle, the result of the advection is null, except in some tidal residual circulation. We verify that in our region the residual tidal circulation is very small, but might be important on the shelf (Bessière et al. 2007) https://hal.sorbonne-universite.fr/ECOLA/hal-00409287v1*

*2) Another aspect could come from the second term of the equation. As over one time step the diffusion could modify the temperature, then the advection of the modified temperature could mark the signature of the impact of the tides.*

*3) And it might be the most important point, in the model, the advection term leads to some diffusivity of the temperature due to numerical dissipation of the advection scheme, in contrast to some non-diffusive advection scheme such as in **Leclair and Madec (2009)**. In our case, we are using the FCT advection scheme (**Zalesak, 1999)** that includes a diffusive part.*

*In previous study, this mixing has been quantified to be responsible for 30% of the dissipation (in lower resolution 1/4° resolution model, Koch-Larrouy et al. 2008), as part of the high frequency work of the advection diffusion.*

*It is an important aspect of the dissipation of the tides we should have presented better earlier. We improve that in the data and method section, including a description of the dissipative term (Lines 232-253).*

8. Line 151. Remove "linear non-hydrostatic".
   *It has been removed in the RM.*
9. Line 155. Wrong reference to Munk and Wunsch (1998), please remove.
   *It has been removed in the RM.*
10. Lines 167-169. This has been mentioned previously and should be removed.

*It has been removed in the RM.*

11. Lines 207-210, starting by "Several configurations…" I do not think this is relevant, it could be removed.
*This sentence is relevant since it justifies the decision to introduce a new configuration. However, we have rewritten it to make it easier to understand in the RM (Line 148-150).*
12. Line 225. "assumed" should be replaced by "prescribed" or "enforced"?
*We agree and change it to "prescribed" in the RM (Line 166).*
13. Line 248. Could you develop what the Kelly et al. (2010) method consist of?
*We don't find that further development about the Kelly method is needed in the manuscript, since it becomes a "classical approach" or well-known method. The author kept as it (Line 192).*
14. Line 262. I would avoid the use of acronyms such as "CVR" in equations. Letters are better suited (usually "C" or "E" for conversion)
*The authors used "CVR" as in Tchilibou et al. (2022). However, the authors agree that "C" is more common (Line 206 and elsewhere).*
15. Line 272, equation (4). Is there a vertical integral here? I think U_bt and P_bt do not depend on the vertical coordinate.
*The reviewer is right. This mistake has been corrected in the RM (Line 216).*
16. Line 277. "allows the propagation pathways" is unclear.
*The authors would like to say here that "the baroclinic tide energy flux emphasizes the Internal tidal waves pathway". This sentence has been rewritten (Lines 221).*
17. Line 284, equation (6). "z" should be a subscript of "K" (vertical mixing coefficient).
*We agree and it has been changed in the RM (Line 228).*
18. Line 286. Replace "space" with "spatial"
*We agree and it has been rewritten in the RM (Lines 230-231)*
19. Line 292. What is the "sum of the numerical diffusion"?
*There is a mistake, the appropriate sentence is "Numdiff correspond to the numerical diffusion …".It has been changed in the RM (Line 298).*
20. Line 304 and elsewhere. There should not be "the" before letters attributed to variables. ("Q_t", not "the Q_t", etc)
*It has been considered in the RM (Line 395 and elsewhere).*
21. Line 314. The agreement is not shown, please remove.
*Figure 2a and 2b show both amplitude (color shading)and phase (solid lines) for the barotropic tides, respectively, for FES2014 and the model. Then the agreement is clearly highlighted.*
22. Line 316. Wrong use of "inland". Shoreward?
*We replaced "inland" by "shoreward" in the RM (Line 270).*
23. Line 317. "This is in terms…" is wobbly. What do you mean exactly? Is that a known bias of models? Please amend.
*The sentence has been rewritten (Line 272).*
24. Line 327. "explains" -> no s

*The sentence has been rewritten (Line 283).*

25. Line 328. “The critical slope for the M2…” is misleading. Is 1.2 the criticality parameter? It should be properly introduced and discussed.
*We agree with the reviewer. The critical parameter equation has been introduced and briefly explained in the RM (Line 291).*
26. Line 335. “signs” -> has a footprint/signature?
*The sentence has been rewritten (Line 313).*
27. Line 349. Are you using the same model configuration as Tchilibou et al. (2022)? Please clarify.
*Tchilibou et al. (2022) were using Ruault et al. (2020) NEMO configuration. We use a different configuration of the NEMO model. However, we performed the same analysis in the same boxes as Table A1 in Tchilibou et al. (2022), therefore we think that it is useless to recall the boxes coordinates in the RM (Line 305).*
28. Line 364. Remove the minus sign.
*All the occurrence of the negative sign when talking about cooling was removed in the RM.*
29. Lines 386-387. It is not clear from Figure 3. Could you show the difference?
*The figure 3, which then refers to figure 4 in the RM has been modified to highlight the 27.2°C isotherm on the seasonal mean map. The difference between tidal and non-tidal simulations is therefore more visible.*
30. Line 401. “as”?
*The sentence has been rewritten as following in the RM (Line 397).*
31. Line 410. There is no Figure 9 in the manuscript.
*It was the figure 5e instead, which now refers to Figure 5e in the RM (Line 409).*
32. Line 423. Remove “the”
*We removed it in the RM.*
33. Lines 438-440. “During […] open ocean” is that supporting any conclusion? Try to avoid statements that are essential to the narrative. (**that are not ??**)
*The information about MLD and thermocline depth for the two seasons is useful to highlight to fact that the model reproduces their seasonal behavior, and how the tides can affect them. See in the RM, Line 447-453)*
34. Line 445. “following the propagation paths of the IT energy flow”? please clarify.
*We rewrite the sentence, see in the RM (Line 445)*
35. Line 469. Density is not shown. Please amend.
*We used* $\sigma_\theta\,[\rho - 1000]$ *to represent density in the RM, and it was introduced in section III.2 (Line 355).*
36. Line 479. Section title is not grammatically correct.
*The title has been replaced by “what are the processes involved ?” (Line 486)*
37. Line 488 and elsewhere. The rate of change of temperature is huge, I wonder if there bould be a missing scaling factor?

*This value is the mean value for the whole season, which cannot be understood as the daily rate (by this way could be increasing day by day).*

38. Line 495. “closed” -> close

    *We changed this sentence in the RM (Line 500)*

39. Line 528. Remove “the vertical gradient” (stratification already refers to a vertical gradient)

    *We changed this sentence in the RM (Line 526)*

40. Line 532. “closed” -> close

    *Corrected (Line 530).*

41. Line 533. Remove “(and thus stratification)”

    *Corrected (526).*

42. Lines 535-538. This is true but not demonstrated through any diagnostic. Check out de Lavergne et al (2020) for a review of how dissipation scales with stratification.

    *We changed this sentence in the RM (Lines 534).*

43. Line 541. “weak extreme”? -> weak extrema?

    *The complete expression in the text is "weak extreme values", which we think to be the same as "weak extrema", see in the RM (Line 544)*

44. Section IV.4.2. The patterns are not discussed. Is there any secondary circulation that shapes the patterns? Also, the averaging period is likely too short to smear out the mesoscale variability. This is discussed in Colas et al (2013). (**section IV.4.1 instead ?**)

    *Yes, you are right we have not discussed these patterns related to the variability of the mesoscale circulation within the strongly dynamic retroflexion in our two twin simulations. Indeed, the seasonal period chosen is certainly too short to eliminate this meso-scale variability We added this information in the discussion part of the RM (section V.5).*

45. Line 576. “extreme values are shifted” is not clear visually.

    *We choose to remove this statement in the RM, since we didn't deepen the analyses.*

46. Line 585. As mentioned earlier, IT should not play a role in advection. To me the patterns are more likely explained by the mesoscale circulation.

    *We agree with the reviewer, the role of the was discussed in section V.1 in the RM.*

47. Line 595. “more pronounced” is not clear visually.

    *We replaced by "slighty stronger" and added the values in the RM (Line 566).*

48. I wonder if it would be helpful to plot a bar chart with the area-integrated and depth-integrated (over a relevant depth range) contribution of each term of the temperature budget to discuss the overall contributions.

    *We already perform that kind of analysis to assess whether the temperature equation is well-balanced between all the terms (adv, diff, ), with 2D maps at different depth levels and with transects along ITs pathways (see figures below). Since the vertical diffusion of temperature is the dominant term in the surface layer and explain the temperature change observed, we* *think that this kind of analysis will not help the reader to understand the results.*

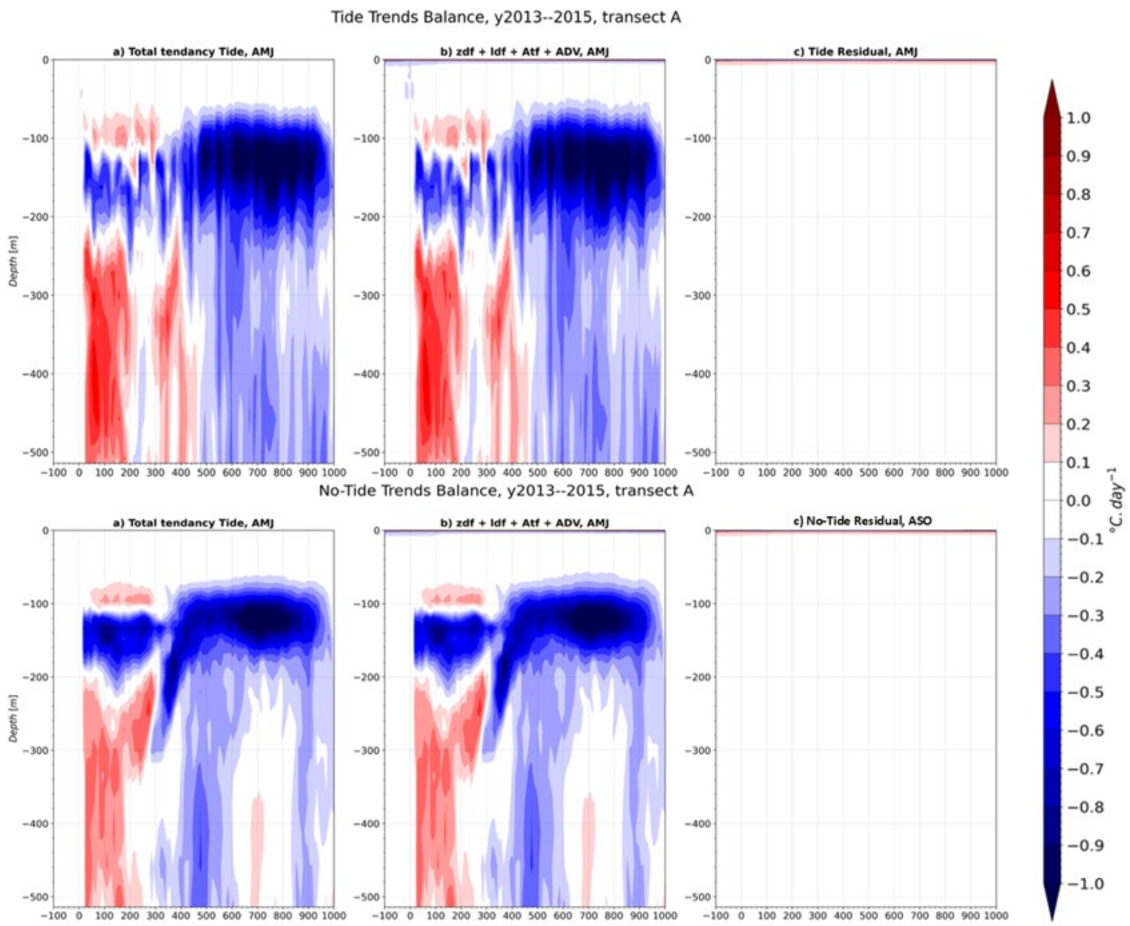
*For the figure below :*

*zdf : vertical diffusion*

*ldf : lateral diffusion*

*atf : numerical diffusion*

*ADV : total advection (x+y+z)*




49. Line 629. "reflection"? I don't think it is a reflection. Do you mean through a few wavelengths?

*We changed "reflection" by "propagation" in the RM (Line 718)*

50. Like the introduction, the summary and discussion should be restructured to highlight

key points and then, discuss the impacts and limitations of the study in thematic and standalone paragraphs.

*The summary section has been squeezed and more structured in the RM, and we add a standalone discussion section.*

51. Line 668. Could you justify that the atmospheric forcing term has not been taken into account in the temperature budget? Actually, this should be discussed earlier, when introducing the budget.

*The atmospheric heat forcing is taken into account by the model and thus in the temperature budget. However, we didn't focus on this forcing in our analysis as this forcing in turn induces a warming of the SST and cannot explain the cooling of the SST due to tides in the studied area.*

52. Line 691. "recovered"?

*We changed this sentence in the RM (Line 608 and Line 746)*

53. Line 704. "anchorage"? mooring?

*We replace with "mooring" in the RM (Line 761).*

54. Line 705. Wrong date in reference, should be 1999 I think.

*The review paper of "Bourlès et al., (2019)" describing the PIRATA mooring and results, was added as reference in the RM (line 761).*

55. Figure 1. Remove points C,D,E ? they are not discussed.

*These points C,D and E are in Figure 1 to make the link with the previous study about ITs, even if we focus our study in the following of the paper on the A and B.*

56. Figure 2 and associated text. Usually the conversion is positive when from barotropic to baroclinic. I suggest to change the sign of the conversion. Same for energy dissipation.

*The negative sign is to fit the studies of Tchilibou et al. (2021) and Barbot et al. (2021). It is an arbitrary sign. This just means a loss for the barotropic tides and a source for the baroclinic tides.*

57. Figure 3. Subplot titles are not consistent, e.g., TMI SST vs SST_TMI, etc. Also in panel (d) title : "mensual" -> monthly

*We agree the reviewer. This was corrected in the figure 3 and 4 in the RM .*

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***Reply to the referees*' comments: Manuscript 2023-418 – Assene et al.**

First of all, we would like to thank the evaluator for the time he took to evaluate this work, as well as for his constructive comments and recommendations.
In the following, the referees' comments are presented in black color and our responses are in blue color. OM refers to the original manuscript, whereas RM refers to the revised manuscript.

**Response to reviewer 2:**

Review of 'Internal tides of the Amazon shelf. Part I' by Assene et al.

This article investigates the role of tides, and in particular of internal tides, on the temperature field around the mouth of the Amazon river. The authors perform two NEMO multi-year runs, one with tides and the other one without. Whenever possible, they validate their results with observations, or a model fed by observations in the case of SSH. They then proceed to explain the differences in the temperature fields by carefully analysing various terms in the temperature evolution equation. In particular, after presenting the main features of the stratification and SSTs in each simulation over each analysis period (April-June and August-October), the authors investigate the role of a few proxies and processes to explain these features: air-sea heat fluxes; vertical temperature diffusion as a proxy for irreversible mixing; vertical temperature advection; and horizontal temperature advection.

The authors compare these metrics between two different seasonal averages: April-June and August-October, themselves averaged for three consecutive years. Their analyses also often focus on two transects that are roughly perpendicular to the shelf, along which elevated SSH variance indicates that the baroclinic tide has a greater amplitude there.

The upshot is that tides cause mixing along the thermocline, cooling the mixed layer and warming the sub-thermocline layer. Interactions with the atmosphere tends to damped the signal immediately under the surface, but said trend is strong around the thermocline. Horizontal and vertical advections of T play additional roles in setting the structure of the T field, although I have a few questions about this.

I have no reason to doubt the results of the paper, and the overall structure of the presentation is sound. I do have a few major comments about the presentation, albeit nothing that would put the publishability of the article in jeopardy. Within sections and paragraphs, the explanations are often hard to follow, sometimes because of long sentence structures, and sometimes because I could not distinguish between key points and details.

A. I was not able to wrap my head around the necessity to look at advection, at least in so much detail. I feel like the story is mainly in the mixing and heat fluxes. Advection seems to me that it is only here to balance the other irreversible processes. In other words, help me understand what I learn when looking at advection in such detail.

B. On this note, I don't recall the authors explaining if the terms they investigate, the ones they deem most important, actually balance. Apologies if I missed it.

C. Often, figures are not cited in the right order. For example, panel (d) of figure X will be cited in the text before panel (a), when panel (a) is cited at all… If you could tighten up the organization of the figures (you could also consider removing a few panels here and there), it might make the flow of the article much smoother.

Here are medium-to-minor comments. Due to time constraints, I will not list English mistakes, typos and other minor sub-sentence edits I noted. Instead, I am attaching an annotated version of their pdf to this review. I want the authors to address my comments in this present review, but I do *not* need to see how the authors address my smaller comments on the pdf, even if they disagree with my suggestions. This would clutter the discussion too much.

1. Abstract is probably too long, be careful about word limits (and even if there aren't any, trimming the abstract certainly wouldn't hurt).
*We thank the reviewer for this comment, we squeezed the abstract for more readability.*
2. The 'IT' abbreviations: before streaming services, we used to listen to music on CDs. We withdraw money at ATMs. And we study the internal tide (IT), or we study internal tides (ITs).
*Thanks for this pedagogic orientation. We appropriately used the abbreviations in the RM.*
3. All these parentheses in the middle of sentences make the text a little harder to read. You don't need half of them.
*We agree with the reviewer, we reduced the use of parentheses in the RM.*
4. Ll. 31-33: I am not sure what the sentence there means.
*The sentence was too long and therefore hard to understand. The overall abstract has been revisited for more consistency in the RM.*
5. Ll. 61-62: 'propagates it and dissipates it in the global ocean, *causing* diapycnal mixing…'. Diapycnal mixing is only a fraction of the energy dissipation.
*We agree that diapycnal mixing isn't the only dissipation process in the ocean. The sentence was rewritten in the RM (Line 56).*
6. Ll. 75-76: about the thermocline being a waveguide for internal tides. I do not remember Bordois' work, but assuming my Ph.D. thesis wasn't completely wrong, this statement is only true for high-frequency internal waves, typically internal solitary waves. Later, you observe significant mixing along the thermocline, so, this point might be relevant. But you don't do anything with it, I don't think you do a frequency analysis or attribute mixing to low-mode vs. ISW mixing. You could, which would require quite a bit of extra analysis. Or you could simply refrain from opening this can of worms…
*We agree with the reviewer and remove this statement in the RM*
7. L. 88: 'and can thus modify temperature': what is the connection with the beginning of the sentence?
*Here we are talking about barotropic bottom frictional effect on temperature. The sentence was rewritten in the RM (Line 51).*
8. L. 118: '(first 50 km)': starting where?
*In Tchilibou et al., (2022) and Barbot et al., (2021) context, the "first 50 km" refers to 50 km oceanward starting from the slope, precisely from the 100 m isobath. We removed '(first50 km)' in the RM as it is not essential to understand the results (Line 94).*
9. L. 158: ISWs don't really have wavelengths, which is a concept that applies to sinusoidal

waves. You are probably thinking about the distance separating ISWs.

*The reviewer is right, the correct expression is "inter-packet distance". The sentence was rewritten (Line 64). And the link with ISW is discussed in the section V.3 (Line 629).*

10. Ll. 249-250: 'then tidal (…) all propagation's modes'. I don't understand this phrase.

*We would like to say that we don't study each mode separately, so the result shown is the total energy of all propagation modes.*

*This sentence was modified in the RM (Line 194).*

11. L. 328: 'The critical slope (…) 1.2'. I don't understand this phrase.

*We here refer to critical parameter as in Nash et al., (2007). We added more context and details in the RM (Line 291).*

12. In the same paragraph: careful about referring to Zaron's product as 'observations'. It is a product, derived from observations, via a model. He has many competitors, all of which produce quite different IT fields, even though they use the same data (see Carrère et al. 2021). So, it doesn't have the 'objective' quality other observational products have, even though it's all relative of course.

*We agree with the reviewer, the Zaron's product is an estimate from altimetry using a model and not direct satellite observation.*

*We replaced "observations" with "estimate or product" in the RM (Line 314).*

13. Ll. 340-341: 'This longer period (…) observations.' Except that Zaron's product looks less smooth than yours… Maybe. Substantiate or drop.

*The reviewer is right, the word "lower" is more suitable than "smooth". It has been modified accordingly in the RM (Line 318).*

14. Around the bottom of p. 11 and Fig. 2f: dissipation as I know it has one sign: all negative or all positive, depending on… well, whether you put a minus sign in front of the definition. Figure 2f shows a dissipation field that is mostly blue, but in some places, it is red. What does it mean for the baroclinic wave field to *gain* energy from dissipation?

$$D_{bc} = div(F_{bc}) - C$$

*This would normally be strictly negative, but we are in time splitting mode, and there is a phase shift between the fast barotropic mode (time step 150 seconds) and the slow baroclinic mode (time step 20 min).The same patterns have been observed in Simmons et al., 2004, Zilberman et al., 2011, Nugroho et al. 2018, Tchilibou et al. 2022.*

*Note that the positive conversion rate in Figures 2f (energy directed from the baroclinic towards the barotropic tides) can occur when the phase difference between the baroclinic bottom pressure perturbation and the barotropic vertical velocity exceeds 90° (Zilberman et al., 2011). Typically, this will happen at some distance of the generation site, at non-flat bottom locations, as the phase speed of the baroclinic tides is much slower than the one of barotropic tides, making the phase difference vary quickly in the propagation direction.*

15. L. 364, but also in other places: 'simulation is about -1ºC cooler': this might be nit-picky of me since we know what you mean, but the '-1ºC cooler' could be understood as a double-negative, that is, '1ºC warmer'.

*We thank the reviewer for this valuable remark. We took this remark into account in the RM (Line 338 and elsewhere).*

16. L. 379-380: did you ever explain why you chose AMJ and ASO over other periods? You compare AMJ with ASO (you do so in the abstract, by the way, which is not the appropriate place). But you didn't explain why ASO was better than e.g. JFM. Note that in order to come to this conclusion, I did Ctrl+F on the abbreviations 'AMJ' and 'ASO', and there were no hits between the abstract and Section IV… If I'm wrong and you did explain it in one of Sections I to III (where it should be explained), then you need to introduce the abbreviations along with it. (This is one of the many instances where by suggesting something minor, I am actually hinting at a significant structural flaw in the paper… Often, information seems to be either missing or hard to find.)

*We are very thankful for this critical remark, there is no doubt that in the introduction we didn't sustain the choice of these seasons in the introduction. By this way, it becomes harder for the reader to get it.*

*The seasons are chosen according to contrasted characteristics better addressed in the introduction. The two seasons are the more contrasted in terms of stratification, ITs activity and temperature structure. Since the aim of the study is to assess how the ITs impact the temperature at seasonal scale, this sustains our choice. These two seasons are also those discussed in Tchilibou et al. (2022). The choice of these two seasons is now explained in the Introduction of the RM (Line 72-97 and Line 128).*

17. L. 383: you reference Fig. 3e-g by itself, in section IV.1, which is different from where you referenced panels (a-d) (namely, III.2). This is a strong indication of either graphical information that should be broken up, or sections that should be consolidated. You could make those three panels a separate figure. While you could see this as a minor detail, Figure 3 is actually barely readable because everything is too small. And this is due to the 3x3 layout you chose, because you tried to show validation info (a-d, referenced in III) together with results info (e-g). Make (a-d) its own 2x2 figure, and make (e-g) another separate, 3x1 figure (recall that the final layout of Ocean Sciences is two-column).

*We agree with the reviewer, that it is better to split figure 3 (3 and 4 in the RM) into two figures. The panel is wider for each figure and get more readable details in the RM.*

18. Fig. 4c-d referenced on l. 389, before Fig. 4a (l. 399). If you feel the need to cite things in the wrong order, it is usually a sign that the overall results presentation needs to be re-thought.

*We agree with the reviewer and rearrange the text and figures citation in a chronological order in the RM text.*

19. L. 396: 'Q_T': isn't it Q_t?

*Corrected in the RM (Line 395 and elsewhere)*

20. L. 410: Fig. 9f doesn't exist.

*Thank you for pointing out this mistake. It's corrected in the RM (Line 409)*

21. L. 429: at this point, I started noticing that your reference temperature for what is a 'cold' isopycnal keeps fluctuating between 27ºC and 27.6ºC (which sounds dreamy, actually) throughout the article. You might want to decide on a choice earlier, in the methods section for example, and ideally use one value throughout. The ripple effect on the overall

presentation might be significant.

*We agree with the reviewer's remark. The term "cold water" now only refers to water < 27.6°C in the RM (Line 113 and elsewhere).*

22. L. 439: did you even define what the 'mixing layer [depth?]' was? NEMO product of dynamics-based?

*We added the mixed-layer definition in the RM (Line 431).*

23. Section IV.3, Vertical structure of the Temperature along A: You mention that ITs deepen the thermocline. But you only show results along A, which is a relatively narrow beam in space… Do I have to imagine the thermocline forming narrow 'trenches' along the beams, or is it a more global result? In other words, if you plotted a map of the termocline depth, would I see large deviations along the beams, or would I see a uniform deepening of the thermocline? If the former is true, plotting that map could be very nice (even better if it correlates with the dissipation pictures that you show later after). If the latter is true, you should mention it, because your choice of showing the results only along A goes against the message that the deepening is uniform.

*We plotted the thermocline depth map (Anomaly Tide – No-Tide) and your first statement is true, we have larger value in ITs region and in the Amazon plume.*

*We decide to add mixed layer depth and thermocline depth map in the RM in order Highlight the impact of ITs on the thermocline and mixed layer depths. See Figures 6 and 7 in the RM.*

24. Ll. 493-494: 'the tidal simulation shows a decrease of the ZDF along the coast'. Decrease compared to what? Or going from which end of the coast to which end?

*It is compared to the previous season. We added this information in the RM (Lines 499)*

25. Ll. 495-496: This is not a true sentence, or not how to use 'While'. Also, 'almost closed' [sic] is redundant. Same remark on l. 532.

*Obviously, it is a mistake, it was removed and we replaced "almost close to zero" with "tends to be null" in the RM(Line 500)*

*The terms 'while was removed (Line 500 and 527)*

26. Section V: I agree with Dr. Vic's opinion that this section should be tightened to highlight key points. In general, I agree with his comments, by the way.

*We also agree with the two reviewers. This Summary section and the abstract have been tightened to highlight the key points of our results in the RM.*

27. L. 728: '(…) of the PhD thesis of Fernand Assene (…)'

*We corrected it in the RM (Line 785)*

28. Fig. 1: Generation site letters A to F are hard to read, especially the black ones

*We increased the figure panel to make these easier to read in the RM.*

29. Fig. 3: fonts are too small, among other things (see comment #17)

*We modified this figure in the RM.*

30. Fig. 6b: did you ever comment about the swirling, filamentary structures you see in the north-west corner of the figure? These are absolutely striking, but I don't remember you commenting on them in the text. If you did, you might want to emphasize it a little better.

*This is also point out by Dr Vic, so we added more discussion about it in the RM in section V.5.*

31. Figs. 6a and (especially) 6b: you mentioned at some point that you don't focus on path B

because it is similar to path A… but these figures, esp. (b), seem to indicate otherwise.

*We add in the text the precision that both paths are similar in vertical structure and there can be some non-noticeable difference in the season AMJ. But in fact, for the second season (ASO), the vertical mixing tends to be null along path B, it is hence useless to show transect along B.*

*Finally, that is what sustains our choice to not show what is going on along B.*

*See in the RM (Lines 429)*

*References*


*Simmons, H.L., Hallberg, R.W., Arbic, B.K., 2004. Internal wave generation in a global baroclinic tide model. Deep Sea Research Part II: Topical Studies in Oceanography, Small and mesoscale processes and their impact on the large scale 51, 3043–3068. https://doi.org/10.1016/j.dsr2.2004.09.015*

*Zilberman, N.V., Merrifield, M.A., Carter, G.S., Luther, D.S., Levine, M.D., Boyd, T.J., 2011. Incoherent Nature of M2 Internal Tides at the Hawaiian Ridge. Journal of Physical Oceanography 41, 2021–2036. https://doi.org/10.1175/JPO-D-10-05009.1*

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