***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.*

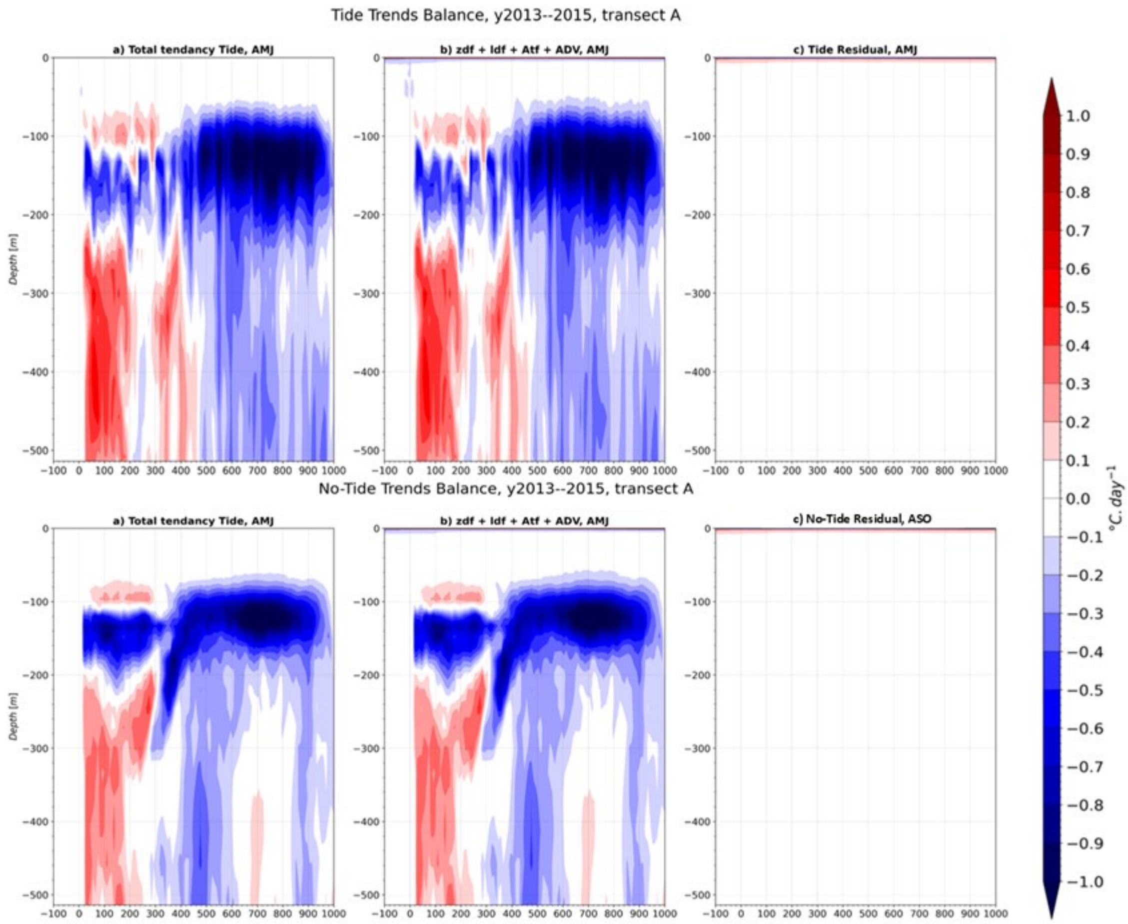
For the figure below :

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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