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

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Nash, J.D., Alford, M.H., Kunze, E., Martini, K., Kelly, S., 2007. Hotspots of deep ocean mixing on the Oregon continental slope. Geophys. Res. Lett. 34. https://doi.org/10.1029/2006GL028170