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 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: (i) lines 75-76, the waveguide concept is not used in the manuscript so it should not be mentioned; (ii) line 145, the coherence vs incoherence of internal tides is not affecting the results on surface layers cooling; (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.
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.
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.

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.
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).
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.

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.
2. Line 59. Remove "but with higher vertical velocities"
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.
4. Line 62. Wrong use of "i.e."
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.
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)?
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".
8. Line 151. Remove "linear non-hydrostatic".
9. Line 155. Wrong reference to Munk and Wunsch (1998), please remove.
10. Lines 167-169. This has been mentioned previously and should be removed.
11. Lines 207-210, starting by "Several configurations…" I do not think this is relevant, it could be removed.
12. Line 225. "assumed" should be replaced by "prescribed" or "enforced"?
13. Line 248. Could you develop what the Kelly et al. (2010) method consist of?
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)
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.
16. Line 277. "allows the propagation pathways" is unclear.
17. Line 284, equation (6). "z" should be a subscript of "K" (vertical mixing coefficient).
18. Line 286. Replace "space" with "spatial"

19. Line 292. What is the "sum of the numerical diffusion"?
20. Line 304 and elsewhere. There should not be "the" before letters attributed to variables. ("Q_t", not "the Q_t", etc)
21. Line 314. The agreement is not shown, please remove.
22. Line 316. Wrong use of "inland". Shoreward?
23. Line 317. "This is in terms…" is wobbly. What do you mean exactly? Is that a known bias of models? Please amend.
24. Line 327. "explains" -> no s
25. Line 328. "The critical slope for the M2…" is misleading. Is 1.2 the criticality parameter? It should be properly introduced and discussed.
26. Line 335. "signs" -> has a footprint/signature?
27. Line 349. Are you using the same model configuration as Tchilibou et al. (2022)? Please clarify.
28. Line 364. Remove the minus sign.
29. Lines 386-387. It is not clear from Figure 3. Could you show the difference?
30. Line 401. "as"?
31. Line 410. There is no Figure 9 in the manuscript.
32. Line 423. Remove "the"
33. Lines 438-440. "During […] open ocean" is that supporting any conclusion? Try to avoid statements that are essential to the narrative.
34. Line 445. "following the propagation paths of the IT energy flow"? please clarify.
35. Line 469. Density is not shown. Please amend.
36. Line 479. Section title is not grammatically correct.
37. Line 488 and elsewhere. The rate of change of temperature is huge, I wonder if there could be a missing scaling factor?
38. Line 495. "closed" -> close
39. Line 528. Remove "the vertical gradient" (stratification already refers to a vertical gradient)
40. Line 532. "closed" -> close
41. Line 533. Remove "(and thus stratification)"
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.
43. Line 541. "weak extreme"? -> weak extrema?
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).
45. Line 576. "extreme values are shifted" is not clear visually.
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.
47. Line 595. "more pronounced" is not clear visually.
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.
49. Line 629. "reflection"? I don't think it is a reflection. Do you mean through a few wavelengths?

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.
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.
52. Line 691. “recovered”?
53. Line 704. “anchorage”? mooring?
54. Line 705. Wrong date in reference, should be 1999 I think.
55. Figure 1. Remove points C,D,E ? they are not discussed.
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.
57. Figure 3. Subplot titles are not consistent, e.g., TMI SST vs SST_TMI, etc. Also in panel (d) title : “mensual” -> monthly

References

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