Reply to RC1

This paper looks at internal tides in the Vitoria-Trindade region using both satellite altimetry and a high-resolution ocean model. The approach is straightforward and the results seem reasonable to me. I recommend some minor changes before publication.

Firstly, we would like to thank you for your valuable comments and remarks, which helped us refine the analysis conducted in this study. We think your inputs improved the overall quality of the paper. We took them into account for the new version of the paper. We remain available for any additional information for the following steps.

I was especially intrigued by the propagation study that examined how a large mesoscale eddy impacted internal tide fluxes -- Figures 12 and 13. The authors emphasized the change in propagation direction (clear in the cartoon of Fig 13, less clear to me in the actual data of Fig 12). But even more striking to me was the nearly complete disappearance in panel (b) [Feb 24] of the energy fluxes south of the eddy. This seems more significant than the small perturbations to direction. But it also raises a number of questions. Most of this paper focuses on M2 alone. Is Fig 12 showing just M2, or is it a general "semidiurnal" flux? If just M2, then I'd like more details about how M2 could be isolated from all the other constituents in their model. If all tides are shown, is the lack of energy in panel (b) just the spring-neap cycle, and thus of minor significance? Perhaps the authors could fill in more details behind these diagrams. (They could also improve the aesthetics of Figure 12, since some fonts are too tiny to see. Maybe some tiny text is not needed?)

We thank you very much for your interest in this Figure. As stated in the paper, we focused solely on the M2 tidal frequency, even for Figure 12. We specified it in the updated Figure 12. After verification, while every panel refers to the day with the highest spring tide cycle, it was not the case for panel (b). Therefore, we decided to update the Figure by taking the closest highest spring tide to the 24th of February. In the new Figure 12, panel (b) displays the baroclinic flux corresponding to the 27th of February. On this new panel (b), the direction of the flux remains the same, with a deviation angle of 25° towards the east. We specified the deviation angles in the section in the new version of the paper. This deviation process has notably been recently documented in Goret et al. (2026) and Kouogang et al. (2025). Considering that every panel of the Figure are now in a similar tidal regime, it is true that the baroclinic flux might be dampened by the anticyclonic eddy, able to trap the baroclinic energy. Since we display only one single descriptive case of interactions between an eddy and the internal tides flux, we preferred to remain qualitative in our analysis. However, Figure 11 highlighted a peak of energy dissipation at the latitude of the eddy and during the November to April period, which can be a first indicator of energy redistribution caused by the eddy. Thanks to your recommendations, we also improved the aesthetic of the Figure, especially by increasing the font size. You can find the updated Figure in the new version of the paper. We also specified that the M2 tidal frequency was retrieved through an online harmonic analysis in the model in the section 2.2.

I also had some concerns with Fig 5 that compared model and altimetry. The altimetry has mostly smaller amplitudes. I agree that this could stem from the altimetry being an average over 20+ years. But the altimetry is also somewhat noisy and one naturally wonders about error bars. It would be good to see error estimates here, which should be easily derivable from their altimeter analysis.

Thank you for your remark on this intercomparison between altimetry and model outputs. We agree that the discrepancy is especially high where the internal tide signal is most energetic. However, as shown in our Figure 2, the filtering method employed in this study does not introduce excessive smoothing when compared to the HRET interpolated product.

On one side, the altimetric dataset that we used in this study is the output of a first preprocessing step performing a harmonic analysis on the full SSH time series available in this region. As a result, the dataset only provides the amplitude and the phase of the chosen tidal frequency along the satellite track only, without any temporal dimension. This hampers any direct estimation of the temporal variability of the M2 baroclinic signal displayed on Figure 5. This is the reason why we underlined the fact that the M2 baroclinic signal is derived from a 27-year time series, which can lead to a smoothing of the amplitude of high-variability signals.

On the other side, the uncertainty associated with instrumental noise is expected to be small (of the order of 0,1 cm approximately) due to the large number of overpasses of the region in more than 20 years of data used to conduct the harmonic analysis. This appears negligible compared to the amplitude of the M2 contribution to the SSH elevation in this region, being of a few centimetres.

To provide an empirical estimate of the uncertainty linked to our method in the region, we propose to conduct a cross-over analysis using independent TOPEX/Poseidon – Jason 1/2/3 tracks intersecting the track 137. At locations where tracks are crossing each other, we compute the differences between the M2 baroclinic tide amplitudes. The standard deviation of these differences provides an idea of the uncertainty linked both to our filtering method and to the temporal variability of the internal tide signal. Only 4 tracks of the TOPEX/Poseidon – Jason 1/2/3 mission intersect the track 137 in the surrounding of the VTR. Based on these crossing tracks, we estimate that the uncertainty linked to our method is about 0,3 cm. This quantification has been added in the paragraph 2.1 introducing the Lancsoz filtering method.

Taking this uncertainty into account on Figure 5 allows us to state that the values of the model are within the altimetry range, except near the ridge where the error is a bit higher due to a higher variability. In particular, the highest difference occurs at the cross-over point with track 100. This cross-over point is located at the VTR latitude, where the M2 baroclinic amplitude is also higher. At this latitude, we note a difference of 0.7 cm between the signal retrieved with track 100 and with the one observed by track 137.

Some other minor points (with line numbers):

- The instructions to OS say the Abstract should be "short, clear, concise." It is written clearly, but it does seem somewhat long.

  The abstract has been shortened in the new version of the paper.

- 32 - main -> main mechanism? missing word?

  Thank you for pointing it out. The word "driver" has been inserted into the sentence.

- 38 - the reference to Carter et al. does not seem appropriate, since that work was focused on the Hawaiian Ridge, not the global energetics.

  Indeed, you are right, we removed this reference which was not adequately placed.

- 43 - "to a day" - It can be many days, as shown by some waves crossing ocean basins.

  It is true. We corrected this mistake by adding "to a few days".

- 88 - The altimeter data extend past 2020, even if not all of it is used here.

  The period on which altimetric data are available and the period that we used for our analysis has been clarified in the new version of the paper.

- 90 - "without aliasing" is not correct. The tidal signals are still aliased, no matter how long the time series.

  Indeed, thank you for pointing it out. We rephrased this paragraph for the sake of correctness.

- 97 - "contribution of baroclinic tides" is part of the noise?? Perhaps the authors mean "incoherent" baroclininc tides.

  Exactly, thank you for noticing this inconsistency, which has been corrected.

- 99 - Arbic reference not needed for the frequency of M2, surely.

  This reference has been removed.

- 101 - I think Step 2 is actually a part of Step 3 and can be removed. It is just a part of any filtering step. There are really only two important steps here.

  It is true, this paragraph has been clarified in the new version of the paper.

- 113 - I did not understand what is "in adequacy"

  This is a language mistake, "in adequacy" has been replaced by "in reasonable agreement".

- Fig 5 panels for barotropic tide. The phase plot is not informative as it just shows big ±180° jumps. Changing the y-axis to some other range would show the data better.

We acknowledge that the phase representation in the [-180°, 180°] range introduces apparent discontinuities that can make the Figure less straightforward to read. Conventional phase ranges are usually [-180°, 180°] and [0°, 360°], which would also include big jumps in the signal. Even if we could have centred the range on another value than 0° or 180° to avoid these jumps, the main goal of this Figure is to analyse how the barotropic signal phase compares with that of the FES2014 reference model. As such, changing the range would not significantly improve the interpretation. We therefore chose to keep this range to align with the range of the baroclinic phase shifts of the left panel of the Figure. To avoid any misunderstanding, we now clarify in the revised manuscript that these phase discontinuities should not be interpreted as strong physical variability.

- 136 "boundaries" might be better than "frontiers"

  “frontiers” has been replaced by “edges” for a better fit while avoiding repeating “boundary” twice in the same sentence.

- 140 - Was FES2014 model used for forcing only on the open boundaries?

  Yes, this has been specified for increased clarity.

- 144 - I think it would be useful to say how barotropic and baroclinic signals were separated. This can be sometimes a source of confusion, so it is good to state what was done.

  Thank you for your remark, it is true that this information was missing in the initial manuscript. We follow the method used in Kelly et al. (2010) to separate barotropic and baroclinic tide constituents. This separation is directly performed at each time step by the model during the simulation. We hope that this clarifies our method. This information has been added in Section 2.2.

- Fig 3, right panels: Are these profiles a MEAN over the whole region? And over what time range?

  These profiles come from the average of all density profiles available in 2008 in a box located over the internal tides region, which boundaries are from 20.5°S to 29°S in latitude and from 40°W to 33°W in longitude. The chosen box has been added to Figure 3 and these details have been updated in the associated paragraph and in the Figure legend for clarity.

- Fig 3, left panels: Is this the "barotropic" SSH for the model, or the "full" SSH? I suspect it is the full external+internal tide, which would explain the phase jitters. Also, what is contour interval for phase lines?

  Indeed, the TAPIOCA-36 map displays the M2 contribution to the SSH of both barotropic and baroclinic tides, which is specified in the text. We updated the legend of the colorbar *of the Figure by removing "barotropic". We added it in the legend of the Figure as well. In the legend, it is specified that black contour lines refer to the tidal phase. We made it clearer by adding some reference values on the maps directly.*

- 163 - why "global"?

  This has been rephrased in the new version of the paper.

- Fig 4. Box 1 is mostly red, boxes 3-6 mostly blue. Why are all boxes negative in the bar chart? It seems Box 1 should be positive.

  It is true that the results obtained in Box 1 differ from the one obtained in the other boxes, certainly due to topographical effects. On the conversion map, every negative value corresponds to the amount of barotropic energy converted into baroclinic energy, whereas positive values generally arise from phase delays between the barotropic tide and the baroclinic tide preventing an accurate quantification of the conversion rate. This leads to less reliable local estimates of the conversion rate. This has been explained in the section 3.1 and we refer the reader to Carter et al. for more details.

  Although Box 1 appears predominantly positive in the map, negative values are still present. The bar chart, however, represents the spatially integrated conversion rates over each box, computed as the sum of all grid point values weighted by their respective cell areas. As a result, a limited number of negative values with larger magnitude can outweigh more widespread but weaker positive values. This explains why the integrated conversion rate in Box 1 remains negative despite its overall appearance as positive in the map.

- Fig 5 and corresponding text, for wavelength determined from altimetry. The peak in the wavenumber spectrum is used. Was this corrected for the orientation of the T/P track? The track crosses the main beams at an angle. (This same issue arises in the Conclusions, too -- line 336.)

  To compare altimetry with TAPIOCA-36 outputs, we selected the model grid points located under the altimetric track. Therefore, we compare signals with the same orientation. As the wavelengths retrieved for the mode-1 and 2 of propagation aligned with previous studies (including Paiva et al. (2018)), it is true that we did not account for the orientation of the altimetric. Now, for a more precise result and based on your relevant comment, we added the correction of the angle on the wavelengths of propagation in the updated version of the paper. We estimated an angle of approximately 20° between the internal tides flux and the altimetric track, which leads to a corrected wavelength of 131 km for the mode-1 and of 61 km for the mode-2.

- Fig 10 - aside from a small change in wavelength, summer versus winter, more pronounced is the differences in the energy level. Summer peak is much reduced in magnitude.

We thank you for your remark on the matter. Indeed, this is not mentioned in the corresponding text but this result is commented in the Discussion section, in regard to the model outputs and to other studies on the seasonal variability of internal tides. Based on your comment, we added a descriptive sentence in the paragraph quoting this Figure in the first place.

- Fig 11 - seems rather cramped and hard to see. Would a log scale for dissipation be better?

  Thank you for your remark. We took it into account to adapt the scale of the x-axis. As harmonic dissipation rates are negative, we now display the absolute values of dissipation rates to allow the use of a log scale here. We would like to thank you for your suggestion which improved the clarity of our Figure.

- 474 - The reference is wrong. This cites it as a OS Discussion paper. The actual publication was in OS in 2022.

  Thank you for pointing it out. The correct reference is now mentioned in the updated version of the paper.