

Reply on 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" baroclinic 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 $\pm 180^\circ$ jumps. Changing the y-axis to some other range would show the data better.

We acknowledge that the phase representation in the $[-180^\circ, 180^\circ]$ range introduces apparent discontinuities that can make the Figure less straightforward to read. Conventional phase ranges are usually $[-180^\circ, 180^\circ]$ and $[0^\circ, 360^\circ]$, 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.

Reply on RC2

Below is a review of “modulation of internal tides properties off the Vitoria-Trindade Ridge during contrasted seasons from altimetry and a regional ocean model” by P. Bauchot and co-authors.

This paper uses an ocean model and historical altimeter data to demonstrate that mesoscale variability and seasonal stratification act jointly to modulate the coherence and energy pathways of internal tides.

I enjoyed this paper and found it interesting. I think the conclusions are reasonable based on the data and analyses presented and I have no major comments to offer in that regard. The main comments are either editorial in nature, or connected to a concept where you are assuming the reader understands what is being discussed.

I recommend accept subject to minor revisions. I would gladly re-read the paper post edits to ensure my comments were satisfactorily addressed.

My comments are enumerated below.

We thank the reviewer for reading our paper carefully and highlighting the points of improvement on which we have worked in the new version. Thank you for your positive feedback, which encouraged us to submit a paper of higher quality. Below you will find a more detailed answer to each of your comments.

- Lines 12-13 – “reflection beams” – what exactly are they? They are never defined in the manuscript. I’ve thought about internal tides for a while and it’s a term I’m not familiar with. This term is used in other places in the manuscript where you assume the reader understands. I looked the term up in Google and I think I know what it is but I’m not quite sure. EGU’s readers are smart but not all specialists. They should get an explanation of what reflection beams are and how to see them in the results you are presenting.

Thank you for your remark. We agree that the term “reflection beam” was not clearly defined in the original manuscript. In the context of internal tides, a reflection beam refers to a beam of internal tide energy that results from the reflection of a primary (incident) beam on ocean boundaries, such as the surface or topography. After reflection, the beam propagates away from the reflection point following the direction imposed by the internal wave dispersion relation.

In our analysis, these reflection beams are identified as coherent structures in the SSH signal, corresponding to regions where baroclinic tidal energy remains concentrated along preferential directions. We have now included this definition at the first occurrence of the term (Section 3.1) to improve clarity for the reader.

- Line 26 – “in the climate regulation” – doesn’t read well, either “the regulation of climate” or removing “the” reads better

We updated this phrasing both in the introduction and in the conclusion.

- Line 32 – “one of the main responsible” – doesn’t read well, perhaps inserting “mechanisms” between “main” and “responsible”?

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

- Line 113 – “is in adequacy” – doesn’t read well. I was going to suggest a rewrite but I’m not completely sure of what you’re trying to convey.

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

- Line 132 – “ $2,5 \cdot 10^{-3}$ ” – I’m not used to scientific notation being presented this way. Can this be rewritten in more standard notation (e.g. .0025)?

We have replaced the scientific notation by a more classical one.

- Line 156 – “The temperature and salinity profiles of the first 500 metres of the water column of TAPIOCA-36 appear really close to the ones reported by the WOA, which

therefore make our model reliable” – this is at best a qualitative argument to claim “reliability”. Can you present a more quantitative case to justify this statement?

We agree that this comparison is qualitative and does not constitute a full validation of the model. We have clarified this point in the revised manuscript and explicitly refer to Giachini et al. (2024) for a more comprehensive validation of TAPIOCA-36.

- Figure 3 – right panel. “temperature and salinity profiles of tapioca-36 against WOA data”. What am I looking at? A single location, a mean across all profiles? If you are presenting this as T-S validation of TAPIOCA-36, you should give more details. For example, numbers of profiles and mean and rms error statistics.

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 this information have been updated in the associated paragraph and in the Figure legend for enhanced clarity.

- Line 173 – “box n° 3” – how about calling it box 3 – as is done in the figure?

Based on your comment, we corrected it in the new version of the paper.

- Figure 5 – green and red line, at least in my printout appear muted and hard to differentiate. Please consider a better version with better colors see. Also, “signal barocline” in the label of the left panel should be changed to match the legend.

We are sorry that the colors of this Figure confused you. However, we didn’t display any element of the Figure in the green color. The indications about the direction of propagation of internal tides as well as the location of the VTR on the graph have been put in blue instead of red. We hope it will be better for the reader’s comfort. Thank you for pointing out the language mistake in the label. It has been corrected in the new version of the Figure.

- Starting at line 195 (as well as Figure 5) - There is a discussion of beams of reflection. I didn’t fully understand the concept because it was never defined by the writers. It made this discussion nearly impossible to appreciate. My line 12 comments hold here too. Don’t assume the readers understand a term, define it so they can appreciate what you’re discussing. The readership of EGU sphere are smart nonspecialists.

As mentioned in our previous comment, we took your feedback into account and defined the concept of “reflection beams” in this specific paragraph. We hope that it facilitates the understanding of our analysis.

- Line 207 – “we retrieved that almost 45% of the dissipation occurs locally” – are you trying to say that “we find that almost 45%”

Indeed, thank you for your suggestion, we updated it in the new version of the paper.

- Line 233 – As the conversion rate C – I assume you mean barotropic to baroclinic? If so, please specify.

Indeed, the conversion rate always refer to the amount of barotropic energy converted into baroclinic energy. We specified it in this paragraph, as well as in Section 3.1.

- Figure 10 – never mentioned in the text

There was a mistake in the referencing of the Figures, which now has been fixed. Thank you for pointing that out.

- Figure 11 – what are the lines near -21 and -22? Should define in legend.

The dotted lines represent the assumed reflection beams in the dissipation signal. This is now defined in the legend.

- Line 263 – “After three periods of internal tides”. This confused me. Are you talking about 3 mode-1 wavelengths, which would be a distance or some sort of time interval? This is confusing and should be expressed more clearly.

Line 264 – First period of reflection – not defined, so I have no guidance to interpret what this means.

Indeed, we acknowledge that it was not very clearly written. In the original version, “three periods of internal tides” was intended to refer to a spatial scale corresponding approximately to three mode-1 wavelengths, rather than a temporal period. Similarly, the expression “first period of reflection” was meant to describe the first reflection beam generated after the interaction of the internal tide with the surface.

To avoid this confusion, we have revised the text and now consistently refer to “reflection beams”, which more clearly describes the spatial structure of the internal tide propagation and its variability.

- Figure 12 – Panel labels are way too small, they need to be increased in size. Also, you have a mix of (presumably) French and English, please be consistent (e.g., isopycne).

Thank you for your comment. We reworked the Figure to fix these issues.

- Line 294 – “period” – I think you meant “periods”

Indeed, thank you for underlining this typo, we corrected it.

- Line 334 – “six beams of reflections” – commented on earlier.

As it has been more broadly introduced earlier in the paper, we rephrased it by speaking of “reflection beams”. We hope that the reader will understand at this point of the paper to what we are referring to. Thank you for your valuable remarks.

- Line 335-336 – “Altimetry allows to retrieve the wavelength of each mode of propagation: 140 km and 65 km for the first and second mode of propagation, respectively” – reads awkward, how about “Altimetry allows the retrieval of the ...”

Thank you for your comment. We rephrased this sentence for a better reading experience.