Review on "A New-Generation Internal Tide Model Based on 30 Years of Satellite Sea Surface Height Measurements"

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January 2025

1 General comments

The article and data set describe a model representing the local sea surface height (SSH) signature of the spatio-temporally coherent internal tidal field in terms of a superposition of propagating plane waves (for given tidal constituent and baroclinic mode). Most notably, this new model (ZHAO30yr) incorporates an unprecedented collection of tidal constituents and vertical modes, mapped at higher resolution than in previous work by the author, and includes error estimates. The performance of the model is evaluated using independent altimetry data (using a variance reduction statistics) showing significant improvement over previous work by the author. These major advances were made possible mainly by the refinement of the authors' mapping technique over the years, and the inclusion of input data from more altimetry missions. In other words, ZHAO30yr is the culmination of the author's leading research in mapping internal tides sea surface signature from altimetry data. Yet, at the same time, ZHAO30yr can be seen as a new starting point for the many potential improvements suggested in this study.

One important feature of the article is the description of the spatial variability in a single component of the decomposed multiwave field. The author investigates two decompositions of the multiwave field: (i) by extracting the (locally) five largest propagating plane waves, and (ii) by selecting the largest of the previously extracted waves propagating in a given directional range (generally half circles). While I am convinced that the reconstructed multiwave field is a reasonable representation of the underlying internal tidal SSH field (although all of the five fitted waves might not be significant everywhere), I have more trouble understanding the analysis of individual plane wave components as performed in the present study.

The article claims that such decompositions can be used to disentangle wave interference and further isolate internal tidal beams. This is an apparent contradiction, in that these beams are commonly viewed as an interference pattern resulting from the superposition of waves radiating from distinct sources (Rainville et al., 2010, see in particular their Fig. 9). The confusion is entertained by the claim that individual components of the multiwave decomposed SSH are not affected by multiwave interference (l215 in the article). On the contrary, interference patterns (with nodes and antinodes separated by half a wavelength) are clearly visible in the decomposed product (see Figs. 3 and 5 in Detailed comments). The above apparent inconsistencies make it challenging for me to understand what the spatial variability in a single component of the multiwave decomposed field actually represents (be it the locally largest component or the locally largest one in a directional range).

In any event, I think the article lacks a clear definition of what is considered as "internal tidal beams". Then, the results based on a single component of the decomposed multiwave field (sections 5 and 6) should be analyzed in a way that is consistent with the latter definition.

Another comment is that the general tone of the article is quite enthusiastic (perhaps too much so), somewhat obscuring the limitations of the model. As a result, non-specialist users of the data might end up being mislead. I would like to see the important limitations and shortcomings of the model explicitly stated where fit:

- ZHAO30yr only extracts the spatio-temporally coherent fraction of the ITs. Hence, the decay in amplitude of an observed internal tidal beam with distance from its source region cannot be directly related to dissipation (e.g. Zaron, 2015; Buijsman et al., 2017; Geoffroy & Nycander, 2022).

- ZHAO30yr cannot distinguish between modal ITs originating from barotropic-to-baroclinic conversion and the waves resulting from scattering of different modes (e.g. by interactions with topography).

- Physically, an internal tidal beam is the spatial expression of the interference of multiple waves radiating from distinct sources (Rainville et al., 2010). Thus, one cannot directly "pinpoint" the source locations of an observed beam. Moreover, it is not straightforward to gain information on the generation of the individual baroclinic waves forming a beam.

2 ESSD's review criteria

- *Read the manuscript: are the data and methods presented new? Is there any potential of the data being useful in the future? Are methods and materials described in sufficient detail? Are any references/citations to other data sets or articles missing or inappropriate? Is the article itself appropriate to support the publication of a data set?*

  The methods used in this publication have been used in previous studies (e.g. Zhao et al., 2016, 2019; Zhao, 2021, 2022, 2023, with an evolution throughout the years). I fail to see a significant difference in the methods compared with Zhao (2023), apart from the inclusion of more tidal constituents and vertical modes. Another important advance is the use of more altimetry missions as input data to build the model. The data will very likely be useful in the future. The methods are described in details, and more information can be found in the previously mentioned studies. However, I have troubles understanding a few key points in the methods and presentation of the results (see General comments). These may be misunderstandings, or simple issues with the terminology used in the paper. Nonetheless, I think these points should be clarified before publication. The paper generally does a good job citing appropriate references and other datasets. It could, however, do a better job at pointing at the (important) limitations of the model.

- *Check the data quality: is the data set accessible via the given identifier? Is the data set complete? Are error estimates and sources of errors given (and discussed in the article)? Are the accuracy, calibration, processing, etc. state of the art? Are common standards used for comparison? Is the data set significant – unique, useful, and complete?*

  The data is straightforward to access and use. The data set lacks error estimates for a single plane wave, as well as the mask for regions of large mesoscale variability where the model likely fails (as explained in the article). In particular, each of the fitted plane wave should be compared with the local error estimate for a single wave ; as it is likely that not all of the five fitted waves are significant at any given location. The model, i.e. the sum of the five fitted plane waves for a given tidal constituent and baroclinic mode, is validated using a variance reduction statistics, and it is compared to previous iterations showing significant improvement.

- *Consider article and data set: are there any inconsistencies within these, implausible assertions or data, or noticeable problems which would suggest the data are erroneous (or worse). If possible, apply tests (e.g. statistics). Unusual formats or other circumstances which impede such tests in your discipline may raise suspicion. Is the data set itself of high quality?*

  As stated above, I think some key points in the methods and presentation of the results should be clarified. Apart from these, the article properly describes the data set.

- *Check the presentation quality: is the data set usable in its current format and size? Are the formal metadata appropriate? Check the publication: is the length of the article appropriate? Is the overall structure of the article well structured and clear? Is the language consistent and precise? Are mathematical formulae, symbols, abbreviations, and units correctly defined and used? Are figures and tables correct and of high quality? Is the data set publication, as submitted, of high quality?*

  The published data set is usable (but still lacks the single wave error estimates and mask for the regions of large mesoscale variability), and the metadata is appropriate. The publication is somewhat long but well written and, overall, of good quality.

- *Finally: By reading the article and downloading the data set, would you be able to understand and (re-)use the data set in the future?*

  Yes, but at this stage only for the reconstructed multiwave field (i.e. the sum of the locally fitted plane waves for a given tidal constituent and baroclinic mode). I don't understand the author's interpretation of the spatial variability in a single wave component.

3 Detailed comments

Introduction

l25-27: It would be fair to cite Colosi and Munk (2006) as well.

l32: To be complete, you could mention semi-analytical methods as well ; especially the recent anisotropic estimates in Pollmann and Nycander (2023) and Geoffroy et al. (2025), which are particularly suited for a comparison with ZHAO30yr.

Paragraph 34-45: Please make it clear that ZHAO30yr only estimates the spatio-temporally coherent ITs.

l51: The “new” mapping procedure is confusing, this seems to be the same procedure as in Zhao (2022).

l56: Same “newly improved” is confusing.

l57-59: This is confusing. Internal tidal beams are generally considered as the expression of the interference pattern between plane waves originating from different sources (Rainville et al., 2010).

l60: ZHAO30yr only sees the coherent fraction of the IT field, thus any information about dissipation would be non-trivial to retrieve (the decay of a beam with propagation distance in ZHAO30yr can also be attributed to a loss of coherence). ZHAO30yr also cannot distinguish between modal internal tides (ITs) generated by the conversion of surface tides and those resulting from the scattering of different modes. This should be clearly stated.

l62: “background internal tides” is unclear, perhaps mention that this will be explained later on.

l63: Errors are lower than 1 mm on a global average.

l67: ”published in Carrere et al. 2021” $\rightarrow$ ”mentioned in Carrere et al. 2021”.

l70: ”are previously masked” $\rightarrow$ ”were previously masked”.

l78: ”Appendix ??”. The appendix appears to be missing (it is not included in the manuscript and supplementary material.) This sentence is unclear at this stage.

l84: ”including all altimetry [. . .]” already written at the beginning of the paragraph.

Section 2.1

l89: ”Direct” $\rightarrow$ ”Directly”

l95: add ”leaked” internal tide signals ?

l98: Taking into account the spatial variations in λ, depending on location one can have λ larger than the cutoff wavelength for the mesoscale field. Hence, this low-pass filtering of the mesoscale estimate does not consistently remove the leaked IT variance (resulting in a biased low IT variance in the results). Applying another (lowpass) filter in frequency might mitigate this, as suggested by Zaron and Ray (2018). In this paper, they also note that the diurnal ITs are more closely entangled with mesoscale dynamics (compared with semidiurnal ITs), so that you may have more leakage from mesoscale activity in your diurnal products.

Section 2.2

l106: Can you demonstrate that these 2 constituent pairs are well separated in ZHAO30yr ?

l113: Do you use the rigid-lid approximation, namely $\Phi(z = 0) = 0$? If not, the problem you solve is not trivial to me. Could you mention the boundary condition you use at the surface and how you normalize the eigenfunctions (and provide a reference) ? One example is Kelly (2016).

l115: I think the equation should read $\lambda_n = \omega c_n / \sqrt{\omega^2 - f^2}$, see for instance Eq. (A5) in Zhao et al. (2016).

l118: What about the temporal variation in λ over seasonal, and inter-annual timescales ? It would be worth comparing this time variability with the bandpass width of the 2D filter.

l123: Can you justify that such close constituents are well separated in ZHAO30yr ?

Section 3

The method extracts 5 independent propagating waves with a target λ and ω. Signatures of other processes than ITs (sharing a similar λ and ω) might be picked up: the mesoscale correction is not perfect, and there may be other waves (e.g. westward propagating tropical instability waves as mentioned in Zhao (2019)). The main assumption here is that only ITs can show the observed spatio-temporal coherent structures over the 30 years of data. Do I get this right ? (If yes, I think this could be made clearer).

l131: Truncating the 2D spectrum (i.e. multiplying it by a rectangular window) will result in ringing, you might want to use a window with a smoother roll-off.

Section 3.1

Could you mention how do you bin the altimetry data in time ? What tolerance do you use to consider two measures collocated in time ? What would a typical snapshot look like in terms of point density within a fitting window ?

Eq. (2): If I am not mistaking, this equation represents waves emanating from an infinitely long line source. This might be a good representation far away from the actual sources (of finite length), but not so much close to generation sites (where I would expect the waves to spread cylindrically). See Fig 1 for an example mode-1 M2 plane wave (in a correspondingly sized fitting window) using values from Table 1 in the article. Could the extent of the spatial window (smaller than a wavelength here) negatively impact the fit ?

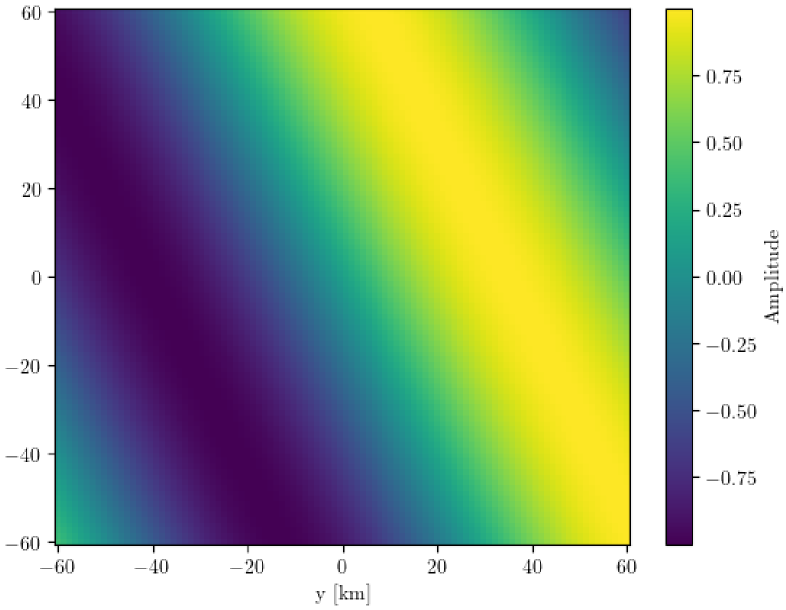

Figure 1: Mode-1 M2 plane wave as per Eq. (2) for a single m in a 120 km fitting window using the mean parameters from Table 1.

l148-154: It took me several reads to understand this procedure, this could be rewritten to make it clearer. The wording is confusing: "target internal tidal wave" is a sum of "5 internal tidal waves", perhaps the latter could be simply called plane waves. Also, you could explicitly state at what stage A_m, ϕ_m, and θ_m are obtained.

l150: Is the least squares fit performed in each compass direction ? If yes, it should be mentioned in the following sentence for the sake of clarity.

Each plane wave fit should be compared with the estimated error (using ”background” ITs or some other error estimate). Fits resulting in an amplitude below the estimated error should be discarded (as in Zhao et al. (2016)). Also, the error associated with a single plane wave should be included in the dataset.

Section 3.2

l159: ”in one overlapping 850 by 850 km” → ”in overlapping 850 by 850 km windows” ?

l161: ”is nontidal errors’ → ” is considered as noise” ?

l162: Is truncation done with a rectangular window ? Does this introduce ringing ? If yes, did you evaluate/mitigate it ?

The truncated spectra may still contain some background noise, not only the tidal peaks. Perhaps this background noise level could be estimated from the variance just outside the theoretical wavenumber range.

Section 3.3

l168: ”S2 has an tidal aliasing” → ”S2 has an aliasing issue”

l172: ”wavenumber-frequency filtering”: I understand that you only filter in wavenumber space (but you select the frequency during the least square fit).

l181: Could you point to the section where these results are presented ?

l184-185: I see that bandpass width times λ overlaps for mode-1 and mode-2 waves. But it is not obvious to me why mode 1 can affect mode 2 but not the other way around. Could you elaborate ?

Section 3.4

l193: define major/minor constituents.

l200: This light blue color code for amplitudes lower than 1 mm does not reflect the spatial variability of the error and may be misleading. Can you think of a better representation including the geographical variability of the error ? I realize discarding the results where the amplitude is smaller than the error may not be a very good solution neither since a large fraction of the global field would be masked (see Fig. 2). Perhaps you could discard data based on the variance reduction statistics computed in section 4.2 ?

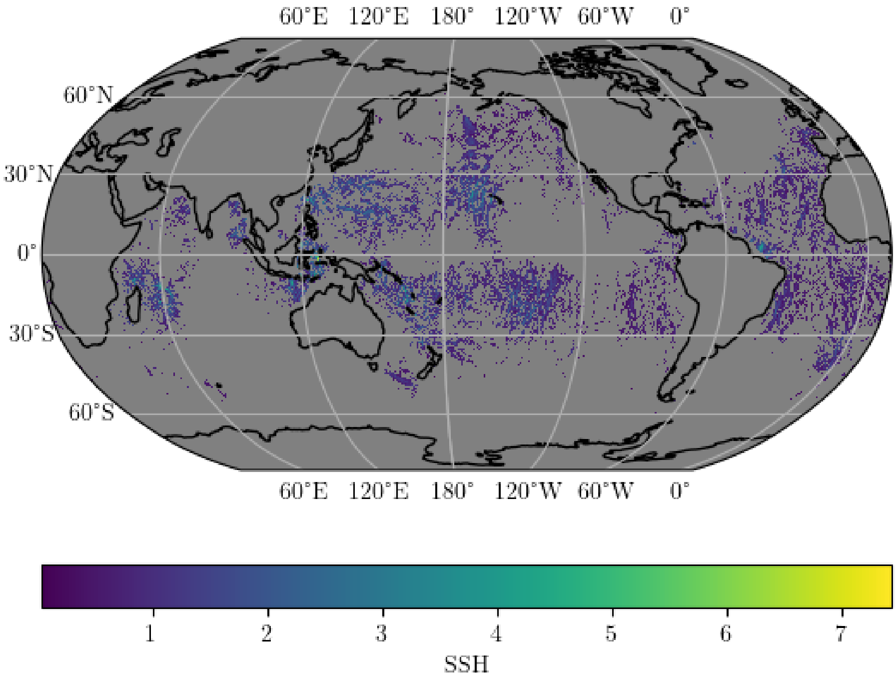

Figure 2: Mode-1 M2 internal tide amplitude (sum of the 5 fitted plane waves), masked where the amplitude is smaller than the provided error for mode-1 semidiurnal ITs.

l201: Consider publishing your mask excluding the regions of large mesoscale variability alongside the data.

l202: "have largest amplitudes" → "have the largest amplitudes"

l203: same for "lowest"

Section 3.5

l210: "5 waves of arbitrary directions" → "arbitrary" may not be adapted here. Perhaps rewrite in something like "the 5 most prominent plane waves with empirically determined directions".

l215: I disagree, interference patterns with half-wavelength fluctuations are clearly visible in the decomposed wave field. In Fig. 3, I attach detail maps of an area between Hawaii and the Aleutian Islands displaying a marked interference pattern in the North-South direction in the mode-1 M2 first (largest) plane wave (IW1). This can also be seen in your maps in supplementary material for mode-1 M2, N2 (and S2 to a lesser extent). Diurnal constituents seem affected as well (in particular, IW1 in mode-1 K1 west of India shows similar fluctuations in the East-West direction, see Fig. 5).

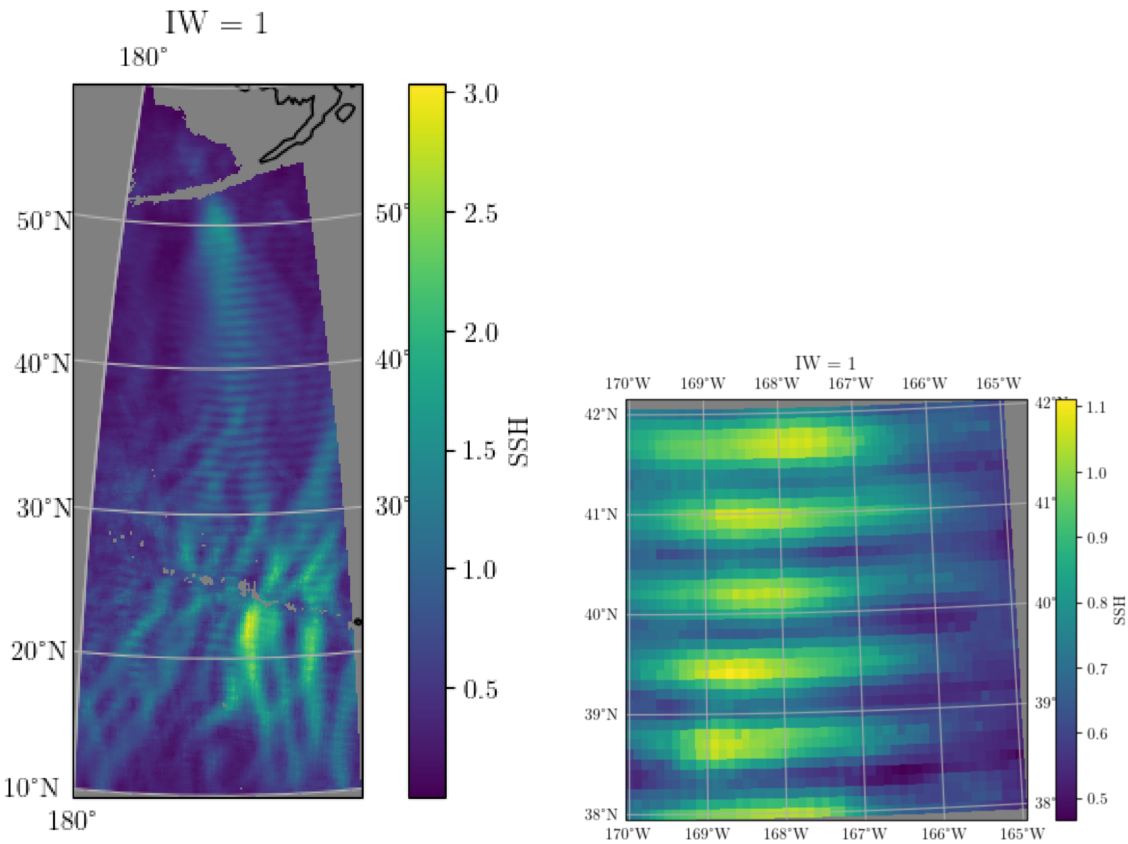

Figure 3: Left panel: Amplitude of the first plane wave for the mode-1 M2 internal tide, an interference pattern is clearly visible (horizontal darker and lighter rays with wavelength of about $\lambda/2$; Right panel: Zoom in the interference pattern.

l216: "new features that are previously masked" → "new features that were previously masked"

l215-224: This is confusing. Internal tidal beams are an interference pattern, I don't understand how you could locally represent a beam as a single plane wave as defined in Eq. (2) (for a single m, see Fig. 1).

l222: Do you mean the decomposed multiwave field ?

l223: Yes, looking at a decomposition by propagation direction might be a more consistent approach. Still, I don't quite understand how you could assimilate the amplitude of the largest plane wave in a directional range to a beam.

figure 2: Hard to read, panels are too small.

Section 4.1

l238: The corresponding mask is not included with the published data.

l240: "reliably" might be an overstatement, especially at the global scale (as you discuss in section 4.2).

Figure 3: The statistics seem to be computed including regions of large mesoscale variability, discarding these regions (as stated in the text) will improve the results.

Section 4.2

l244: "(amplitudes and phases)" → "(amplitude and phase of each constituent)"

l246-247: should read T_n, $f_n(t)$, $u_n(t)$, $A_n(x, y)$, and $phi_n(x, y)$.

l249: delete "Here" or → "Here,"

l254: "is the variance difference" → "is the difference in variance computed"

l256-257: confusing, rephrase.

l262: "real internal tides" → "predicted internal tides"

Section 4.3

figure 4: Hard to read, panels are too small.

l292: internal tidal beams are presents in all products, rephrase.

Section 5.1

l306-307: as well as in studies based on in situ observations, it would be fair to mention a few of these as well.

l310: ”But” → ”Note”

l312-313: These single-wave error estimates are not published with the data.

l316-318: unclear, rephrase.

l325: ”Section 6.1 has shown” → ”Section 6.1 shows”

Figure 8: delete ”[too many circles]” in caption

l329: This is unclear. The detection of mode-2 M2 ITs with altimetry might be strongly influenced by the stratification structure, but not so much the underlying true wave field (other parameters set the generation, e.g. the topographic wavelength (Llewellyn Smith & Young, 2002)). You should also mention that mode-2 waves tend to be more incoherent.

l342: ”The mode-1 and mode-2 beams” → ”The detected mode-1 and mode-2 beams”. Item (1), and perhaps item (2), can be partly explained by mode-2 waves being more incoherent than mode-1 waves (e.g. Rainville & Pinkel, 2006). Also, a possible explanation for item (2) could be that the interference of the shorter mode-2 wavelengths results in narrower beams compared with mode 1.

l346: item (3), linear theory is instructive here: it predicts very small conversion where the topographic length scale does not match the wavelength of the wave (Llewellyn Smith & Young, 2002). In other words, surface tide conversion into mode 2 is maximized at topographic features with length scales close to the shorter mode-2 wavelength.

l347: Again this is confusing, the beams are not generated at a point location, they are the result of the interference of multiple (line) sources.

l349: This is wrong, mid-ocean ridges also generate significant mode-2 M2 ITs. See global map of the M2 mode-2 generation from linear theory in Geoffroy et al. (2024).

l350-351: The model also have important limitations that should be clearly mentioned (only coherent ITs, interference pattern present in the amplitude of the individual fitted plane waves, separation of barotropic conversion and scattering processes).

Section 5.2

l361: ”; however,” → ”, however,”

l365: Could (1) be explained by weaker S2 barotropic tide as in (2) ?

Section 5.3

l382: "summed filed" → "summed field"

l382: "beams are masked by multiwave interference", rewrite.

Section 5.5

l410-411: eastern/western hemisphere relative to 120 deg W ? (State reference longitude to avoid confusion.)

l422: "Figure 13b, circle", which circle ?

l423: "Figure 13b, circle", which circles ? (Use different colors to distinguish from the Mona Passage.)

l435: "pinpoint" is confusing.

l437: Unclear, rewrite.

l438: Already stated above, delete sentence (and typo in "K2").

Fig. 14, 15, 16: mention blue circles in caption ?

Section 5.6

l441: "but that the K1" delete "that" ?

l446: "Another similar feature" add "with mode-1 K1".

Section 6.1

l494: The beams are actually clearly visible from the summed plane wave product (see Fig. 4). The directional decomposition offers a cleaner view of the beams.

Figure 19: Title of (h), typo in "mode-2 S2".

Last line of caption, "flurier 2D filtering" → "Fourier 2D filtering".

Last line of caption: On the contrary, this last observation seems consistent with the literature: The maximum in SSH occurring at some distance away from the generation site(s) has been observed and modeled in previous studies. This has been related to the first surface bouncing of the tidal

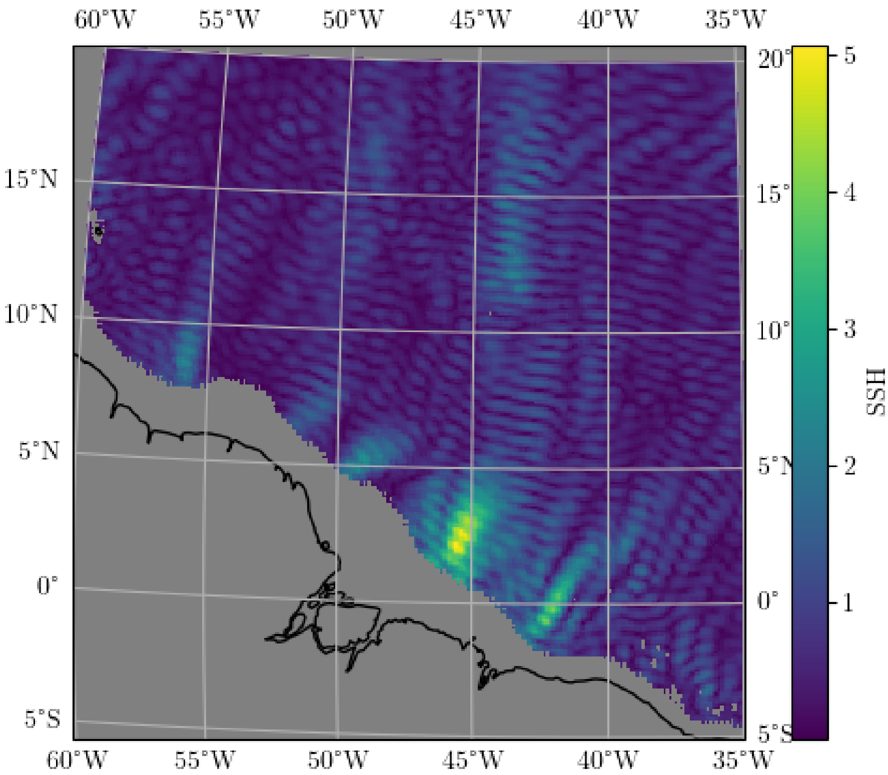

Figure 4: Mode-1 M2 internal tide amplitude (sum of the 5 fitted plane waves) close the Amazon mouth.

beam (Merrifield & Holloway, 2002; Carter et al., 2008). This could also be interpreted as the expression of interference between multiple plane waves (Rainville et al., 2010)).

l501: Could you explain ? Is this because of the superposition with waves (reflecting or originating) from the Mid-Atlantic Ridge ?

l505: It would be good to remind that the decay in amplitude of a spotted beam with propagation distance is partly due to the growing fraction of incoherent ITs (not being taken into account in your model).

l514: Since you mention possible comparisons, the semi-analytical estimates from Pollmann and Nycander (2023), and Geoffroy et al. (2025) are also particularly suited for a comparison with ZHAO30yr.

l515: "temporal variation of semidiurnal ITs", unclear. do you refere to the beat originating from the sum of the constituents close to the M2 frequency ?

Section 6.2

l528: Typo "the the four diurnal". Also, you rather show the two first modes of the two diurnal constituents O1 and K1.

l533-535: This beam is actually solely due to IW1 in the plane wave decomposed field (see Fig. 5). Furthermore, an interference pattern with half-wavelength fluctuations is clearly visible within the area of the beam. Hence, Fig. 20 a is challenging to interpret. Nonetheless, the evolution of phase and amplitude along the putative trajectory look convincing.

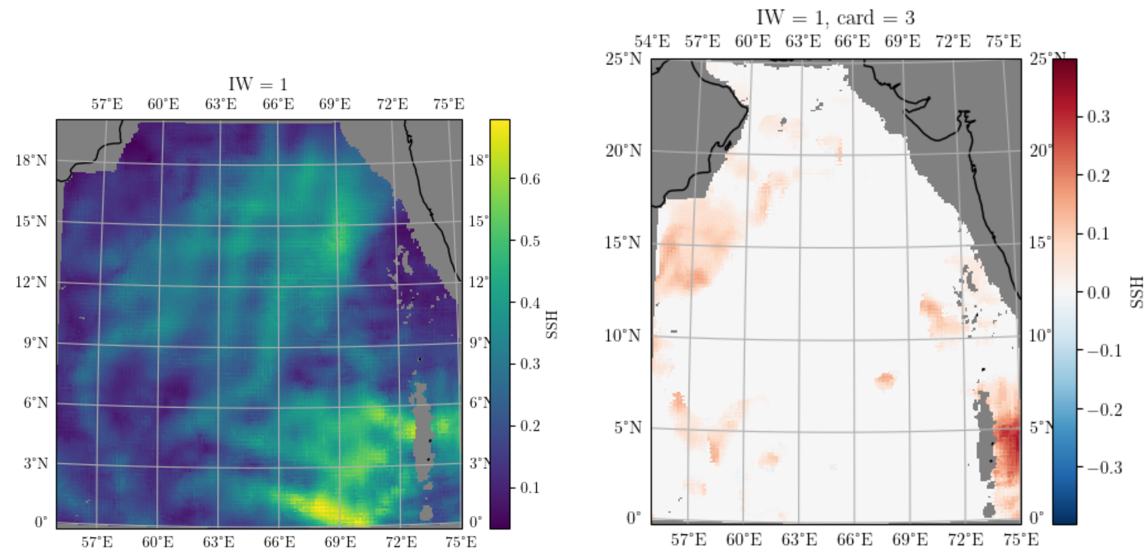

Figure 5: Left panel: Amplitude of the first plane wave for the mode-1 K1 internal tide (IW1), an interference pattern is clearly visible (both vertical and horizontal darker and lighter rays with wavelength of about $\lambda/2$; Right panel: IW1 minus the westward component of the directionally decomposed internal tidal field.

l535: "repeat cycles", unclear. Do you refer to a satellite mission in particular ?

l537: "beams [...] composed of narrow beams" is unclear, rephrase.

l541: That's the 2 first modes of 2 constituents.

l545: "pinpoint", again, not exactly.

Summary

l549: delete ”new”, it has been published already.

l554: ”down to lower than 1mm” add ”on a global average”.

l559: ”the internal tidal field into” $\rightarrow$ ”the internal tidal field is decomposed into”

l560: I would be more nuanced on the results of the multiwave decomposition (specifically for the presence of interference patterns in a single plane wave field).

l564: reference for ZHAO20yr ?

l569: To be nuanced: the beam generation does not occur at a single source point (Rainville et al., 2010). Moerover, incoherence acts to decrease the detected beams' amplitude with propagation distance, this is not straightforward to disentangle from dissipation.

l571: ”off the Amazon shelf recognized” $\rightarrow$ ”off the Amazon shelf have been recognized”

l573: ”For mode-2 constituents” $\rightarrow$ ”For M2 and S2 mode-2 internal tides”

l575-577: The mode-1 K1 beam you identified west of India corresponds to the first (largest) fitted plane waves in this region. This is inconsistent with the commonly accepted view of a beam resulting from the interference between multiple plane waves emanating from distinct generations sites.

l579: The beams have already been identified in previous studies. Also, you already stated this in point 4.

References

Buijsman, M. C., Arbic, B. K., Richman, J. G., Shriver, J. F., Wallcraft, A. J., & Zamudio, L. (2017). Semidiurnal internal tide incoherence in the equatorial pacific. *Journal of Geophysical Research: Oceans*, *122*(7), 5286-5305. Retrieved from `https://agupubs.onlinelibrary.wiley.com/doi/abs/10.1002/2016JC012590` doi: https://doi.org/10.1002/2016JC012590

Carter, G. S., Merrifield, M. A., Becker, J. M., Katsumata, K., Gregg, M. C., Luther, D. S., ... Firing, Y. L. (2008). Energetics of m2 barotropic-to-baroclinic tidal conversion at the hawaiian islands. *Journal of Physical Oceanography*, *38*(10), 2205 - 2223. Retrieved from https://journals.ametsoc.org/view/journals/phoc/38/10/2008jpo3860.1.xml doi: 10.1175/2008JPO3860.1

Colosi, J. A., & Munk, W. (2006). Tales of the venerable honolulu tide gauge. *Journal of Physical Oceanography*, *36*(6), 967 - 996. Retrieved from https://journals.ametsoc.org/view/journals/phoc/36/6/jpo2876.1.xml doi: 10.1175/JPO2876.1

Geoffroy, G., Kelly, S. M., & Nycander, J. (2025). Tidal conversion into vertical normal modes by continental margins. *Geophysical Research Letters*, *52*(1), e2024GL112865. Retrieved from https://agupubs.onlinelibrary.wiley.com/doi/abs/10.1029/2024GL112865 (e2024GL112865 2024GL112865) doi: https://doi.org/10.1029/2024GL112865

Geoffroy, G., & Nycander, J. (2022). Global mapping of the nonstationary semidiurnal internal tide using argo data. *Journal of Geophysical Research: Oceans*, *127*(4), e2021JC018283. doi: 10.1029/2021JC018283

Geoffroy, G., Pollmann, F., & Nycander, J. (2024). Tidal conversion into vertical normal modes by near-critical topography. *Journal of Physical Oceanography*, *54*(9), 1949 - 1970. doi: 10.1175/JPO-D-23-0255.1

Kelly, S. M. (2016). The vertical mode decomposition of surface and internal tides in the presence of a free surface and arbitrary topography. *Journal of Physical Oceanography*, *46*(12), 3777 - 3788. Retrieved from https://journals.ametsoc.org/view/journals/phoc/46/12/jpo-d-16-0131.1.xml doi: 10.1175/JPO-D-16-0131.1

Llewellyn Smith, S. G., & Young, W. R. (2002). Conversion of the barotropic tide. *Journal of Physical Oceanography*, *32*(5), 1554 - 1566. doi: 10.1175/1520-0485(2002)032¡1554:COTBT¿2.0.CO;2

Merrifield, M. A., & Holloway, P. E. (2002). Model estimates of m2 internal tide energetics at the hawaiian ridge. *Journal of Geophysical Research: Oceans*, *107*(C8), 5-1-5-12. doi: 10.1029/2001JC000996

Pollmann, F., & Nycander, J. (2023). Resolving the horizontal direction of internal tide generation: global application for the m2 tide's first mode. *J. Phys. Oceanogr.*, *53*(5), 1251 - 1267. doi: 10.1175/JPO-D-22-0144.1

Rainville, L., Johnston, T. M. S., Carter, G. S., Merrifield, M. A., Pinkel, R., Worcester, P. F., & Dushaw, B. D. (2010). Interference pattern and propagation of the m2 internal tide south of the hawaiian ridge. *Journal of Physical Oceanography*, *40*(2), 311 - 325. Retrieved from `https://journals.ametsoc.org/view/journals/phoc/40/2/2009jpo4256.1.xml` doi: 10.1175/2009JPO4256.1

Rainville, L., & Pinkel, R. (2006). Propagation of low-mode internal waves through the ocean. *Journal of Physical Oceanography*, *36*(6), 1220 - 1236. Retrieved from `https://journals.ametsoc.org/view/journals/phoc/36/6/jpo2889.1.xml` doi: 10.1175/JPO2889.1

Zaron, E. D. (2015). Nonstationary internal tides observed using dual-satellite altimetry. *Journal of Physical Oceanography*, *45*(9), 2239 - 2246. Retrieved from `https://journals.ametsoc.org/view/journals/phoc/45/9/jpo-d-15-0020.1.xml` doi: 10.1175/JPO-D-15-0020.1

Zaron, E. D., & Ray, R. D. (2018). Aliased tidal variability in mesoscale sea level anomaly maps. *Journal of Atmospheric and Oceanic Technology*, *35*(12), 2421 - 2435. Retrieved from `https://journals.ametsoc.org/view/journals/atot/35/12/jtech-d-18-0089.1.xml` doi: 10.1175/JTECH-D-18-0089.1

Zhao, Z. (2019). Mapping internal tides from satellite altimetry without blind directions. *Journal of Geophysical Research: Oceans*, *124*(12), 8605-8625. Retrieved from `https://agupubs.onlinelibrary.wiley.com/doi/abs/10.1029/2019JC015507` doi: https://doi.org/10.1029/2019JC015507

Zhao, Z. (2021). Seasonal mode-1 m2 internal tides from satellite altimetry. *Journal of Physical Oceanography*, *51*(9), 3015 - 3035. Retrieved from `https://journals.ametsoc.org/view/journals/phoc/51/9/JPO-D-21-0001.1.xml` doi: 10.1175/JPO-D-21-0001.1

Zhao, Z. (2022). Satellite estimates of mode-1 m2 internal tides using nonrepeat altimetry missions. *Journal of Physical Oceanography*, *52*(12), 3065 - 3076. Retrieved from `https://journals.ametsoc.org/view/journals/phoc/52/12/JPO-D-21-0287.1.xml` doi: 10.1175/JPO-D-21-0287.1

Zhao, Z. (2023). Mode-1 n_2 internal tides observed by satellite altimetry. *Ocean Science*, *19*(4), 1067–1082. Retrieved from https://os.copernicus.org/articles/19/1067/2023/ doi: 10.5194/os-19-1067-2023

Zhao, Z., Alford, M. H., Girton, J. B., Rainville, L., & Simmons, H. L. (2016). Global observations of open-ocean mode-1 m2 internal tides. *Journal of Physical Oceanography*, *46*(6), 1657 - 1684. Retrieved from https://journals.ametsoc.org/view/journals/phoc/46/6/jpo-d-15-0105.1.xml doi: 10.1175/JPO-D-15-0105.1

Zhao, Z., Wang, J., Menemenlis, D., Fu, L.-L., Chen, S., & Qiu, B. (2019). Decomposition of the multimodal multidirectional m2 internal tide field. *Journal of Atmospheric and Oceanic Technology*, *36*(6), 1157 - 1173. Retrieved from https://journals.ametsoc.org/view/journals/atot/36/6/jtech-d-19-0022.1.xml doi: 10.1175/JTECH-D-19-0022.1