Referee #2

This manuscript describes the results of using a two-stage plane-wave-fitting technique to estimate the mode-1 baroclinic N2 tide from satellite altimeter data. The N2 tide is only about 20% as large as the M2 tide, so it represents a relatively small signal to be estimated, but, nonetheless, potentially significant in the interpretation of SWOT data in some regions.

The paper is a well-organized description of the author's methodology and results. Because the frequency of the N2 tide is close to that of M2, one largely expects the N2 fields to be very similar to those of M2, which are born out by the results. One novel aspect of this work is the author's technique for estimating the error in the harmonic constants by performing an analysis at nearby non-tidal frequencies where the expected results should consist entirely of noise. This technique has been used previously in other tidal studies, e.g., Ray and Susanto, 2016, and their work should be cited (search for "false" tides in that work). I think the author's use of the terminology "background tides" is confusing and he should find a better way to explain his error estimation procedure.

Thank you very much for your time and help!

In the revised manuscript, we cite two papers that employ similar techniques to estimate tidal errors: Ray and Susanto 2016; Zaron et al., 2023. Section 2.5 is heavily revised to better present our estimation procedure and resulting model errors.

Except for a few minor comments, below, I have no objection to publishing this manuscript in Ocean Science. I do think the results will be of limited interest in their current form, though, and with a few minor additions the interest and impact of this paper could be increased.

- The author computes an error estimate, but the estimate does not seem to be used in any way. The areas delimiting mesoscale contamination and western boundary currents seem to be taken from his previous work. Shouldn't the masking or delimiting areas of significance be based on the present error estimate?

In the revised Figures 3, 4 and 6, we delete the black contours taken from our previous papers.

Figure 6 is revised. It shows that the performance of the N_2 model can be related to the difference between the N_2 variance and 1.5 times the error variance. The factor of 1.5 is chosen, because it gives the best spatial correction between the variance difference (Figure 6c) and the N_2 variance reduction (Figure 6a). Figure 6d gives the mask indicating the region where the N_2 variance is greater than 1.5 times the error variance. The N_2 model performs better in the masked region.

- For comparisons with other estimates of the N2 tide which are likely to be produced by other groups, it would be useful if some quantitative summary statistics were presented, such as area-average potential or total energy of N2, and global average explained variance.

We have computed the globally integrated N_2 and M_2 energies following the method described in Zhao et al (2016 JPO). They are about 1.8 and 30.9 PJ, respectively. Their ratio is about 5.8%, larger than the theoretical value of 4%, because N_2 contains relatively more error variance (25%) than M_2 does (1%).

- The author's assertions that the tidal predictions will be useful for correcting SWOT data would have more impact if they were quantitative. For example, what is the expected explained variance averaged along the SWOT 1-day repeat tracks?

Along the SWOT daily repeat swaths, we have computed the variance reductions caused by the N_2 and M_2 internal tide models (ref. to Figures 6a and 6b). We have also computed the variance reductions in the masked region (N_2 variance > 1.5 error variance). The results show that both models perform better in the masked region. Please see our detailed description in Section 2.6.

- Personally, I thought much of Section 3 could be deleted or considerably shortened since the results are very much as would be expected based on our knowledge of M2.

We agree with the referee that "the results are very much as would be expected based on our knowledge of M_2." However, we can only expect the general features of N_2 internal tides from pre-known M_2 internal tides, due to our limited knowledge. More details should be provided by observations or numerical simulations. Figure 7a shows that the correlation coefficient between the N_2 and M_2 amplitudes is only 0.69, which suggests that we cannot accurately predict one tidal constituent from the other.

Itemized comments:

l25: Agreed that the N2 tide could cause variation in internal tide driven mixing; however, it might be worthwhile to point out how small this is expected to be in comparison with M2 (e.g., 4% if the mixing is a quadratic function of amplitude or 0.1% if the mixing is cubic). These variations are likely fall smaller than the uncertainty in the mixing caused by the larger components.

One sentence is added here: "*Theoretically, N_2 may modulate M_2 internal tides by ±20% in amplitude, and by ±40% in energy. On average, N_2 will enhance the M_2-induced ocean mixing by 4%.*"

l28: "provide" --> "could provide"

Changed.

l33: "it is" --> "it will be"

Changed as suggested.

l79: Since it was described in detail in previous studies, it should not be referred to as a "new" method in the abstract, etc.

Throughout the manuscript, 'new' is removed/changed in several places.

l102: Is there a reason why the rigid lid boundary condition is used? Doesn't this lead to errors of a few percent? (Wunsch and Chelton)

Changed. Our WOA18 phase speeds are computed using free-surface and rigid-bottom boundary conditions. In fact, our mapping procedure is not sensitive to the different boundary conditions.

l139-l146: This is a nice test for crosstalk.

Thanks.

l149: "in the our"

Fixed.

l151: "that"

Fixed.

l155: Is it fair to say that the rms error of the N2 tide is estimated to be 50%, or 25% error variance?

It is true. One sentence is added here: "O*n global average, the error variance is about 25% of the N_2 variance and 1% of the M_2 variance."*

l159: Can you move the explanation of the term "background internal tides" to the start of the paragraph so the meaning is clearer?

This paragraph is rearranged to better present our estimation method.

l160: Why did you only use one of these estimates? Why not take mean amplitude across all the frequencies? Oh, I see, Figure 5 suggests that you only computed the error estimate at all frequencies within the regional box. Is that correct?

It is true. We run it in the central Pacific using 13 frequencies that evenly distribute between N_2 and M_2, and show that they give similar results. We then pick only one frequency (period is N_2 minus 3 minutes) for a global run (because it is time-consuming).

l217: "no shown"

Fixed.

l248: How was the calculation method "confirmed"?

Changed. Now it reads "*The same calculation method has also been derived by Geoffroy and Nycander (2022).*"

l253: Repetitive: "model results freely available"

This sentence is removed to avoid repetition.

Section 3: Overall, I thought that this section could be shortened considerably. I think it suffices to say that the N2 and M2 tides are similar, as expected. As for their differences, I am not sure much can be said, due to the small size and noise of the N2 maps. I do like the focus on regional maps, which are more intelligible than the global maps.

We agree with the referee that Section 3 is too long for the referee and readers who are familiar with this field (tide experts). However, our readers may have different research backgrounds. We want to show the detailed regional and global maps to readers who are not very familiar with this topic and hope to see details to better understand our conclusions (intermediate researchers or graduate students or from related research fields).

Fig 6: You previously computed an error estimate in Figure 4. Would it be useful to mask off the explained variance estimate so that it excludes locations where the standard error is larger than, say, 50%, of the predicted amplitude? As-is, it appears that you have a lot of regions where the explained variance is either negative or unexpectedly large (e.g., in the WBC regions). It would be interesting to see how correlated are the explained variance and the predicted variance.

Figure 6 is revised. Figure 6c shows the difference between the N_2 variance and the error variance (times 1.5). Here a factor of 1.5 is chosen, because it gives the best spatial correlation between panels (a) and (c). Figure 6 suggests that the N_2 model performs well in the region where the N_2 variance is greater than 1.5 times the error variance. The masked region is shown in Figure 6d.

Referee #3

The revised manuscript now provides detailed descriptions of the method to extract mode-1 N2 internal tides and the characteristic features of the obtained internal tides. I appreciate the author's efforts to thoroughly revise the manuscript, which makes it easier for readers to see that the method is overall plausible. Although I still do not find any novelty in terms of the internal wave dynamics, the presented results seem reasonable and will make a solid contribution to the processing of the satellite altimetry data. Hence, I recommend publication of this manuscript after minor revisions.

Thank you very much for your time and help! Observing internal tides is the same important as exploring dynamics. Observations may indirectly improve our understanding of internal tide dynamics.

Minor Comments:

Response to Comment 3 of Reviewer #3: The author responded that the mode-2 N2 internal tides can be easily separated from the mode-1 N2 internal tides because they have significantly different wavelength. This is true, but I question why the author ignores the existence of mode-2 and higher modes even near the generation regions of internal tides, where higher modes may have comparatively large amplitude with mode-1. Note that the ultimate goal of this study is to remove all signals of internal tides from the satellite altimeter data for the study of mesoscale and sub-mesoscale processes.

We have tried mapping mode-2 N_2 internal tides around the Hawaiian Ridge (185–205ºE, 18–28ºN). However, the resulting model is noisy, as expected. In this region, the mean amplitude of mode-1 N_2 internal tides is about 2.5 mm. The mean mode-2 N_2 amplitude would be 1 mm, using a ratio of ~2.5 from mode-1 and -2 M_2 internal tides. The 1-mm mode-2 N_2 signal cannot overcome the ~0.7-mm error. This discussion is added to the summary section.

Response to Comment 4 of Reviewer #3: If M2 internal tides are randomly and weakly Doppler-shifted by time-varying background fields, the resulting internal wave frequency will spread centered around the M2 tidal frequency. Therefore, Fig. 5, which does not show such spreading in the frequency space, may be considered evidence that the Doppler-shifting effects are not very strong (at least in this particular region). However, I do not understand why the results described in Lines 139–146 can be regarded as evidence to show that the Doppler-shift is weak. If M2 internal tides are Doppler-shifted by time-varying background fields, they no longer have M2 tidal frequency even in a fixed frame of reference.

Our analysis described in Lines 139–146 is to show that "the N_2 and M_2 internal tides do not crosstalk in our mapping method." It is not to examine the Doppler effect of background currents on internal tides.

Section 2.3: Why does the author need to perform the plane wave analysis twice in the data processing (steps 1 and 3)? Does the obtained tidal field change significantly if one skips step 1? If so, why?

The first-round plane wave analysis is to map internal tides from along-track altimetry data to horizontal fields at a regular longitude-latitude grid. It cannot be skipped. The second-round plane wave analysis is to decompose internal tides into five waves of arbitrary directions at each grid point. It cannot be skipped either.

Lines 295–296: Barotropic-to-baroclinic energy conversion is not examined in this manuscript.

This sentence is repetitive and inaccurate, thus removed in revision.