

## Editor

The manuscript has been significantly improved in the first round of discussion. Nonetheless, there is still room for some improvements, as per the referees' suggestions.

Thank you very much for your time and help!

Dear Author,

The referees have acknowledged the improvements you brought in the revised version and your dedication in addressing their comments. Nonetheless, as you can see, they provide some useful suggestions for a few further adjustments, which I kindly encourage you to consider.

This manuscript was further revised following suggestions from both referees. Please see my point-to-point response.

Concerning the comment by Referee #2 about the number of figures, which they leave to my decision. I would tend to agree on their point that the whole description could be somewhat lengthy for some readers, but on the other hand of they are important for you I am also perfectly fine with them.

In response to Referee #2's concern on the number of figures, I moved Figures 2, 5, and 6 to supplementary materials and dropped Figure 21. Among them, Figure 2 presents the 12 constituents. Figures 5 and 6 show that the new model is better than an old version. Figure 21 shows scaling factors. They are not so important as Figures 7–18 (now Figure 4–15). In the end, the revised manuscript has only 17 figures.

## Referee #1

I thank Dr Zhao for his dedication in answering my numerous comments point-by-point. After reading his response alongside the tracked changes version of the manuscript, I consider he did a great job answering my earlier interrogations. I also think the revised manuscript gained in clarity and does not require another round of review.

Thank you very much for your time and help!

Still, there are three points I would like to raise (I let the author and editor judge the necessity of addressing these):

(A) The first one concerns the denomination of the "apparent" and "real" or "isolated internal tidal beams", i.e. the pattern originating from the interference of multiple internal tidal waves versus a single internal tidal wave originating from a finite-length source. I am unsure referring to the latter as a "beam" is correct (note I am not a native English speaker). Instead, I think a "real internal tidal beam" should simply be called an internal tidal wave. In my opinion, this would greatly improve the clarity of the text and be more consistent with the literature.

Thank you very much for the clarification. I agree that “beam” may have other denominations. The term “internal tidal beam” has long been used since our 2009 paper *New Altimetric Estimates of Mode-1  $M_2$  Internal Tides in the Central North Pacific Ocean* (<https://doi.org/10.1175/2009JPO3922.1>). In this paper, we suggested that multiwave interference must be resolved to correctly interpret in situ observations and satellite altimetry. Later, we used the term in our 2016 paper *Global Observations of Open-Ocean Mode-1  $M_2$  Internal Tides* (<https://doi.org/10.1175/JPO-D-15-0105.1>). It has been cited ~240 times (Google scholar) without public objection to our usage of the word. Since then, I used the term in all my following journal papers. I think that it is better for me to keep consistent. I do not object other researchers to use the term for different meanings, if only those researchers are consistent in their own publications.

Second, as pointed out by the author, my plot (Figure 2) and comments relative to the SSH error were wrong. In the published dataset, the SSH error is given in unit mm while the SSH amplitude is in cm (I assumed everything was in cm). The units are indeed correctly specified in the data, however it requires special attention from the user (at least for users not relying on matlab) that could be easily spared. Thus, I suggest using the same unit for both the SSH amplitude and error in the published data.

Thank you for pointing out that the inconsistency between the units of SSH amplitudes (in centimeters) and amplitude errors (in millimeters) could cause misunderstanding. In the revision, I re-created the model error files. Now, both amplitudes and errors are in centimeters (uploaded to the figshare data server).

Lastly, Dr Zhao made available the error estimates for individual waves as well as the geographic mask corresponding to regions of strong mesoscale activity (<http://doi.org/10.6084/m9.figshare.28559978.v1>). However, this update is not visible from the main dataset page (<http://doi.org/10.6084/m9.figshare.28078523.v1>). Could a reference be added there? (Maybe it is pending, in that case please ignore this comment).

I also noticed that the two datasets (models and errors) do not display side by side at my figshare website, although both Digital Object Identifiers (DOIs) work well. But I cannot manage it, which is likely programmed by the figshare server. To compensate for this inconvenience, I added the hyperlink of the model errors (<http://doi.org/10.6084/m9.figshare.28559978.v3>) or the citation (Zhao 2025) in three places of the revised manuscript. A serious reader will not miss these data files.

## Referee #2

Overall, the paper is improved, and I appreciate the additions that point out limitations with the new model.

Thank you very much for your time and help!

I still have a few comments, the first is in response to a new section (Section 7) on "scaling factors". As the Author Reply notes, this section was originally in his draft paper but then removed in the first submitted version. It has now been added back. I agree with his original thinking, and I question its utility because I don't think it adds much that is useful.

Section 7 was dropped in the revised manuscript. It was used to explain why the  $M_2$ – $N_2$  pair is more similar than the  $M_2$ – $S_2$  pair to address a question brought up in the first-round discussion.

First, the scaling factors are strangely in the form of rational fractions:  $(1/3)$ ,  $(1/5)$ , etc. The barotropic  $Q_1$ – $O_1$  factor is quoted as  $1/5$ . This makes no geophysical sense. The  $Q_1/O_1$  ratio in the astronomical force is  $0.1926 = 1/5.192$ . It is then stated that the internal tides have the same factors, but they are not listed. I question this. E.g., the  $O_1$ – $Q_1$  histogram (Fig 21h) shows a clear bias toward larger  $Q_1$  values (likely from noise), and any kind of orthogonal regression of that data, which accounts for errors in both variables, would give a scale factor different than  $1/5$ . Same for the histograms of  $P_1$ – $K_1$  and  $K_2$ – $S_2$ . The end result is that these factors supposedly imply that "the new internal tide model is trustworthy," but I don't follow the logic of that. My recommendation is that the section be taken back out.

Section 7 shows that the eight internal tide constituents have scaling factors that are consistent with their barotropic counterparts. I rounded the scaling factors to their closest integer ratios for simplicity. In fact, as Referee #2 points out, the  $Q_1/O_1$  ratio is  $1/5.192$  not  $1/5$ . I agree with Referee #2 that this section can be taken out (and left to future dedicated study).

My original review stated that Figures 7–18 were too many figures and most readers would find the description of all of them "tedious". I still think that. Dr Zhao disagrees and thinks all the figures are necessary. I leave it to the editor to decide. Dr Zhao is correct that the journal is no longer cutting down trees to print all these figures, so that is good.

I respect your opinion that this manuscript is somewhat long. In the revised manuscript, I moved Figures 2, 5, and 6 to supplementary materials to make room for Figures 7–18 (now Figures 4–15). Now the revised manuscript has 17 figures (instead of 21 figures).

My final point is in response to his replies to both reviewers regarding interference of wave patterns and the formation of internal tide "beams". It dawns on me that the reviewers and Dr Zhao are using the term "beam" with different meanings. We were using the term to describe a concentrated 2-D focus of wave energy. I think Dr Zhao is using "beam" to describe essentially a wavefront, so he talks about beams even for plane waves (or waves spreading out in a cylindrical pattern). Maybe our objections about "beams" and "interference" would make more sense if this difference in meaning is accounted for.

Thank you for the clarification. Please see my reply to comment (A) of Referee #1. In this manuscript, all my figures (e.g., Figures 16 and 17) clearly illustrate what I mean by an "internal tidal beam", which is consistent with my previous papers.