Anonymous Referee #2

I consider this journal focused toward documenting data sets that are useful to the earth science community. As the new 30-year internal tide model is likely to be quite useful, I am happy to see it so thoroughly documented. I have some suggestions about items that appear confused or confusing or might improve the presentation.

Thank you very much for your time and help.

1. I think it is too much to show all 12 derived constituents in large 3-panel figures. This covers Figures 7 through 18, a lot of figures and a lot of page space. Each figure is described in the text and most readers, if like me, will eventually find this tedious. Cannot some of this go into the Supplement? Are they all so important to justify so much page space? "A picture is worth a thousand words" (Line 296), but it can still be too many, unless there is good reason to show all of these.

I have seriously considered the suggestion of moving some figures to the supplementary file. I decided to keep its current arrangement for the following reasons. First, Figures 7–18 present the core product of ZHAO30yr and show new internal tide features that have never been seen before. It is better to keep the completeness. Second, page space is not as critical as before, because papers in this journal (Earth System Science Data) are published online. Both the paper and its supplementary file will be in digital.

2. In each figure there are two panels (b and c) that decompose the wave field into two directions. These are interesting to see, but I am less clear about what they mean and what Dr Zhao thinks they mean. At Line 58 it is claimed that this decomposition "reveals numerous long-range internal tidal beams." But so does the original (not decomposed) wave field. It is my understanding that the apparent beams (horizontal, not vertical!) represent mostly interference patterns (e.g., Rainville & Pinkel, 2006), and this must still be true in all three of the figure panels. I still think the decompositions are useful, because they (presumably) reflect in some way how much wave energy at a location is arriving from which direction. But the justification for these decompositions and the assertions about "beams" needs some more careful thinking.

I thank the Referee for pointing out the differences between “apparent” and real internal tidal beams. Yes. The multiwave interfered internal tide fields give “apparent” internal tidal beams. In contrast, the directionally decomposed fields give real internal tidal beams. This concept may be challenging for new users, because previous internal tide models do not give resolved internal tidal beams.

The multiwave interfered field and the directionally decomposed field seemingly have similar beam patterns. But the latter should be used in the calculation of energy, energy flux, and phase speed. My goal of Figures 7–18 is to demonstrate numerous isolated internal tidal beams previously masked.

I agree with the Referee that, in Figures 7–18, panels (b) and (c) show multiwave interference of some degree. It is partly because the multiwave decomposition is not perfect. Note that the multiwave interference can be reduced by dividing the summed field into 4 components. For example, in one of my previous papers, I have four directionally decomposed components: eastward (directional range is -45°–45°), northward (45°–135°), westward (135°–225°), and southward (225°–315°). In addition, a larger fitting window also helps reduce half-wavelength wiggles.

3. There is some apparent confusion about "sun-synchronous" measurements and how these impact the S2 waves - Lines 169-172. The statement that "signals caused by solar radiance have longer spatial scales" is not the most important point. That is not the problem with sun-synchronous measurements. The problem is that all the altimeter data measure the tide at the same phase, so you are trying to solve for a sine wave when you have measurements at only one phase. I think the reason S2 is recovered here is because there is a lot of CryoSat data. If the fitting were tried with only the sun-synch data, it would be less good and maybe fail, I suspect. Also, (Line 170), Ubelmann et al. solved for S2, but they did not discuss the results, and it is not clear how successful their S2 solution was.

This paragraph was thoroughly rewritten. "There are likely three reasons for why Sun-synchronous missions do not ruin our mapping of S_2 internal tides. (1) Our mapping procedure extracts internal tides not only by their frequencies in time but also by their wavelengths in space. Measurements by Sun-synchronous missions still provide useful spatial information on S_2 internal tides. (2) Our 30-year-long data record itself can significantly reduce model errors. (3) A large fraction of our data is from non-Sun-synchronous missions, which greatly reduces model errors."

One item that does potentially point to small S2 errors (caused by lack of phase sampling in sun-synchronous measurements) can be seen by comparing M2, N2, and S2 results - Figures 7, 9, and 11. Patterns in N2 look more like M2 than do patterns in S2, even though S2 forcing is closer to M2 forcing. For example, look at the southward components in panels (c) in the North Pacific. S2 appears different. I suspect this is because of the problem of so many altimeters that do not sample S2 well enough.

In an earlier version of my manuscript, I had one section describing scaling factors. It was dropped before submission. Challenged by this comment, I think it is better to add it back (Section 7 Scaling factors). In this section, I examined the spatial patterns of the barotropic and internal tide constituents using TPXO8 and ZHAO30yr. I found similarities between 4 pairs of internal tide constituents (M_2–N_2, K_1–P_1, S_2–K_2, and Q_1–O_1). M_2 and N_2 have similar spatial pattern with a correlation coefficient of ~0.69. S_2 and K_2 have similar spatial patterns with a correlation coefficient of ~0.57. The similarities stem from their barotropic tide similarities.

4. Related to that, I suspect that another reason Zhao20 is inferior to Zhao30 for S2 is because there was far less CryoSat data in the old solution. This affects the explanation Lines 287-291.

ZHAO20yr-S_2 was developed excluding SSH measurements made by Sun-synchronous altimetry missions. It is poor, because (1) it used much fewer SSH data (1993–2012, excluding Sun-synchronous altimetry missions), and (2) it was mapped using my old mapping procedure.

5. The Abstract needs to acknowledge that the mapping is only the "phase-locked" component. This is clear from the Intro, but it also should be stated in the Abstract.

One sentence was added to the Abstract that "ZHAO30yr only extracts the 30-year phase-locked internal tides, lacking the incoherent component caused by the time-varying ocean environment."

6. Since non-repeat altimeter data is used, it is important in Section 2.1 to state what Mean Sea Surface model was used. Also, Line 86, polar tide should be pole tide.

One sentence was added to specify: "Specifically, the mean sea surface model used in this satellite altimetry product is CNES-CLS15 (Pujol et al., 2018)."

It should be "pole tide." Fixed.

7. Table 1, column "Bandpass width" has units?

A note (b) was added to explain that "bandpass width multiplying local wavenumber K (lon, lat) yields bandpass cut-off wavenumbers."

8. Line 123. I think this statement should be removed unless it can be backed up with evidence.

This statement was removed. A new sentence was added that "… therefore, it is challenging to separate these two constituents. In this paper, I extract reasonable K_1 and P_1 internal tides empirically using 30 years of altimetry data."

9. Line 139: For plane-wave fitting, perhaps Ray and Cartwright (2001, GRL) should be mentioned here?

A sentence was added here. It reads "Plane wave analysis evolves from the two-dimensional plane-wave fit method (Ray and Cartwright, 2001), but plane wave analysis can determine multiple waves in different propagation directions and thus resolving multiwave interference (Zhao and Alford, 2009)."

10. Section 4.1 on model errors describes an interesting approach to determine errors, with interesting results. Also, I notice that mode-2 errors are larger than mode-1 errors, even in absolute terms, not just relative. I like this section.

Figure 2 shows that mode-1 and mode-2 errors are overall on the same level. The latter is slightly larger than the former by 0.1 mm.

11. In Figure 3 caption, please state what the numbers in upper left mean. In Figure 4 caption, please state units (yes, the figure gives mm^2, but it is tiny in upper right and easy to miss). In Figure 8 caption, what is "too many circles"?

The captions of Figures 3, 4 and 8 were changed/fixed as suggested.

12. Line 437: Answering WHAT question?

This paragraph was rewritten. One sentence was added "This feature raises the question of what special topographic and tidal conditions combined induce these isolated beams."

13. Figure 19 caption. I do not understand the sentence "Note that the largest..." Also, fix apparent typo on "flurier".

This sentence was rewritten. It reads "Note that maximum SSH amplitudes along all beams do not appear on the Amazon shelf, but about one wavelength away from their source. It is an artificial feature caused by the large spatial windows used in plane wave fitting and Fourier bandpass filtering."

Typo was fixed. It should be “Fourier.”

More typos:

78 - ??

Fixed. There once was an Appendix in the manuscript but was dropped in later revision.

Figure 19h, "moe"

Typo was fixed.

504 - should "model" be "data"?

Changed. Now this sentence reads “For comparison, the S_2, N_2, and K_2 beams disappear sooner, likely because their lower amplitudes are masked by the still large errors in our models.”

728 - "using"

Typo was fixed.