

REVIEWER 1

Spatial and temporal variability of mode-1 and mode-2 internal solitary waves from MODIS/TERRA sun glint off the Amazon shelf.

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We thank the reviewer for taking the time to review our manuscript and especially for the valuable comments regarding the ISW inter-packet distance variation according to the tidal cycle. In the following, our responses to the reviewer’s comments are in bold black colors.

The paper has improved significantly. The writing is still a bit cumbersome and difficult to read in places.

Comments

1. In the first sentence of section 2.1 it is stated that 140 images were used. I suggest replacing all statements that ‘more than a hundred’ images were used with ‘140 images’ throughout the manuscript. ‘More than 100’ is not precise enough. More than 100 could mean 101, 10,000 or even more.

ANSWER:

The total number of images was replaced throughout the whole manuscript, as suggested by the reviewer.

2. The period MAMJJ includes spring and summer months but is referred to as 'spring' while ASOND includes summer and fall months and is referred to as summer/fall. Seems inconsistent not to refer to MAMJJ as spring/summer.

ANSWER:

The authors agree with the reviewer and the terms boreal spring and boreal summer/fall were removed from the manuscript.

3. Line 15. "play a role in refracting the waves towards the northeast". Or "bending the waves ...". I'd delete the "which gives them an extra offshore acceleration". The meaning of this is not very clear.

ANSWER:

The "which gives them an extra offshore acceleration" was deleted, as suggested.

4. Lines 29–30. Delete 'internal solitary waves' and just use the acronym as the acronym was defined on line 23.

ANSWER:

"Internal solitary waves" was deleted.

5. Line 33. Delete 'generate'. The hotspots aren't generated. Internal waves are generated.

ANSWER:

The word "generated" was removed from the manuscript, as suggested.

6. In the results section sub-patches are mentioned but not defined. Where in figure 2 are the subpatches? How were the distances between them, given in table 1, calculated?

ANSWER:

The location of the sub-patches and their middle point used in the calculation of the distances were added in Figure 2-(b). More explanation is presented on page 7, lines 155-158.

7. Mention is made of wavelengths and velocities estimated from the images. The wording often implies that the propagation velocity is estimated independently of the wavelength (e.g., "mean propagation velocity/wavelength varies ..." on line 238) but in reality the velocity is simply the distance between consecutive wave packets divided by the M2 tidal period. So if the wavelength increases by 10% then the velocity will as well. That the velocity is computed this way was not clear enough to me. It would help if the wording was changed to something like "... the wavelength increased by x% implying a corresponding increase in the propagation speed".

Also, I think using the term 'wavelength' instead of 'inter-packet distance' is not a great idea.

ANSWER:

Taking into account the linearity of the ITs, the ISW inter-packet distances can be used as a proxy for the IT wavelengths (this information was added on page 5, lines 119-120). However, we agree with the reviewer, so the term wavelength when referring to ISWs was replaced by inter-packed distance in the whole manuscript, as suggested by the reviewer. Furthermore, as suggested, when referring to the propagation velocity, we add the term “corresponding velocity”.

8. Line 177. What does “calling for mode-1 waves” mean?

ANSWER:

We changed to: “the group with higher inter-packet distance associated with mode-1 IT”. Please, see page 8, line 183.

9. Line 182. What is the simulated mode-1 mean propagation velocity?

ANSWER:

We changed it to: “the calculated mode-1 mean propagation velocity is [...]”. Please, see page 8, line 188.

10. Lines 184–187. This seems a bit irrelevant and begs for an explanation of why the phase speed is proportional to the surface wave elevation and why it explains the higher underestimation of the mode-1 waves.

ANSWER:

As suggested by the reviewer, this explanation was removed from the manuscript.

11. Lines 197. Here it is stated that the mode-1 velocities are underestimated by 22%. What about the mode-2 velocities?

ANSWER:

This information was added to the text: “The TGE allows a relevant prediction of the propagation velocity/wavelength distribution of mode-1 and mode-2 waves, with mode-1 velocities being underestimated by 22% and mode-2 by 3.7%” (page 11, line 205).

12. Line 201. What is meant by ‘joining area 2’? I do not see a green rectangle in Figure 7(b).

ANSWER:

The green rectangle in Figure 7-(b) was missing. We corrected that.

13. Line 227. Simplify: “... waves travel in a more eastward direction”

ANSWER:

The sentence was changed as suggested by the reviewer.

14. Line 238. Why are higher wavelengths associated with neap tides rather than spring tides where the stronger tides would suggest larger ITs being generated with larger propagation speeds.

ANSWER:

We agree with the reviewer that ITs with higher velocities are expected during stronger tides, i.e., near spring tides. Indeed, our results show higher inter-packet distances (and, consequently, corresponding higher velocities) associated with the wave signatures found near neap tide. We argue that maybe this result could be associated with our unbalanced data set, which may hamper our analysis. This argumentation was inserted in the text, please, see page 14, lines 246-247.

15. Line 344. Should 'Area A' be 'Area 2'?

ANSWER:

Yes, "area A" is actually "area 2". We have corrected it.

16. Lines 355–356. What is meant by 'refracting the waves northeast'. It seems like everything is propagating roughly northeast. Do you mean more eastward or more northward?

ANSWER:

Indeed, we mean to say more eastward. This was changed in the manuscript according to the reviewer's comment.

17. Lines 373–374. Mention is a northwest branch is made but I can't see anything propagating northwest in any of the Figures.

ANSWER:

The authors agree with the reviewer. So, the text was changed to "Furthermore, during ASOND the flux coming from the IT site D (contained in area 2) divides into two, creating a more westward branching. In Figure 14-(b) the branching is visible in the ISW satellite measurements near latitude 4°N" (see pages 22-23, lines 383-385).

18. Line 376. Mode-2 waves coming from D are mentioned here but in Figure 17 there is nothing coming from D.

ANSWER:

We have added a green rectangle in Figure 7-(b) showing the waves that probably are coming from the D site. This information was added in the text as well. Please, see page 23, lines 286-287.

REVIEWER 2

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We thank the reviewer for taking the time to review our manuscript and especially for the important remark regarding the values of the diffusivities. In the following, our response to the reviewer's comment is in bold black color.

The authors have addressed most of my comments and I think the paper is suitable for publication. My only remark concerns the values of the diffusivities, I think molecular diffusivities are not appropriate for geophysical flow it is really nearly equivalent to consider an inviscid flow. So putting such a low diffusivity does not really make sense it would be more straightforward to state that they consider inviscid solutions of the TG solution. If they really want to get an estimate of the diffusivities impact on TG solution they should consider eddy diffusivities which will be orders of magnitude larger.

ANSWER:

The authors agree with the reviewer's comment. We changed the manuscript to state that we have considered inviscid solutions of the TGE. The appendix containing the

method to solve the viscous TGE was excluded and all important information about the TGE is summarized in section 2.3: Theoretical calculation of IT velocities.