

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

By Carina Regina de Macedo et al

The paper is focused on the characterization of ISWs off the Amazon shelf from MODIS satellite imagery. The most important result is the characterization of recurrent mode 2 ISWs and the impact of seasonality/circulation on wavelength and phase speed propagation directions. Regarding mode-1 ISWs most of the results confirm the analysis of Marghales et al (2016) based on SAR data; more interesting is the detection of mode 2 ISWs which are more impacted by seasonality and circulation than mode-1 waves.

I believe the paper could be accepted for publication after moderate/minor revisions

In general the analysis is convincing. I think yet that the background conditions obtained from the model for the circulation and IT generation could be added to further evidence the impact of circulation on the figures. I also think that a ray tracing computation for mode-1 and mode-2 following Rainville 2006 would be a nice addition to further illustrate the impact of refraction by the circulation.

Another point is that I do not really agree with the fact that the authors rule out KdV arguing it does only apply for flat bottom, it is also the case for their modal decomposition! I believe some KdV estimate of the phase speed would probably mostly fill the gap between the phase speed they observe from satellite and the linear modal phase speed. The problem is more than they can't estimate the amplitude of the ISWs to get the KdV phase speed. All in all despite the systematic underestimation of phase speed/wavelength by the linear model, the difference of characteristics between mode-1 and mode-2 and the bimodal distribution of ISWs characteristics is enough to convince the reader that both ISW amode-1 and mode-2 are observed.

IN the abstract and the conclusion, the authors suggest at generation of ISW at the point of IT beam reflection, I guess this corresponds to the local generation mechanism described by Gerkema, if so he should be cited and this mechanism more thoroughly discussed.

The abstract refers to very specific labels/features of the figure and is mostly impossible to understand without seeing the figure. Although it is fine to put some quantitative results in the abstract I would suggest to delete too much specific reference to fig labels etc...

I have several more specific comments/questions below.

Abstract L14-15 seems contradictory with previous sentence

L61 shift what? the depth of max amplitude of displacement i-e velocity node?

L146-147 how these diffusivities and viscosities are estimated, what is their impact on the phase speed?

L187 On figure 2 A and B are between isobath 500 and 2000 m not 200 m

L194-195 cloud cover could definitely be as strong biased isn't it possible to get the distribution for clear sky images, or even simply the mean crest length and stdv for clear sky images?

L202 why normalized ?

L203-205 It seems really difficult to conclude anything on the seasonality of the number of signatures, if we restrict to months with at least 5-10 images I don't see any significant differences.

L209 I don't get this sentence, you find mode 1 every year according to Fig 4 b and mode 2 every year except 2009, what do you mean by "the annual probability of finding a mode-1 signature is more than 4 times the probability of finding a mode-2 one"

Paragraph starting at L219 seems to me to describe the method to separate mode 1 and mode 2 wave and should therefore be introduced before Fig.5

L284 how do you define near spring? +/- 7 days after peak spring?

L296-297 difference of phase speed between spring and neap tide is typically a result of amplitude/nonlinearity so it can't be reproduced

L363 I think the authors describe the generation of ISW at the point of IT beam reflection, I guess this corresponds to the local generation mechanism described by Gerkema, if so he should be cited I think.