REVIEW

of the manuscript
"On the generation and evolution of internal solitary waves in the Andaman Sea" by Yujun Yu, Jinhu Wang, Shuya Wang, Qun Li, Xu Chen, Jing Meng, Kexiao Lu and Guixia Wang

The paper focuses on the problem of generation and evolution of internal waves in the Andaman Sea, specifically, their origin due to interaction of tidal flow with a ridge in the Nicobar Archipelago and successive evolution of generated waves in the surrounding areas. The problem is addressed by considering the model results obtained based on two-dimensional numerical modelling using the MITgcm. An interesting element of this paper is a contribution of two small ridges to the wave fields generated by the main ridge located between them. Some sensitivity runs on the temporal variability of the wave characteristics over the neap-spring cycle, as well as due to seasonal variations of the stratification were conducted. The influence of the variation of the bathymetry (different transects across the ridge) was also in the focus.

There are some interesting elements in the paper that deserve to be considered, although I personally have some doubts concerning the robustness of the applied method. It is quite difficult to judge the paper if you are not convinced in plausibility of the reported results. So, in my opinion the paper should not be accepted for the publication in the form it is submitted now. I would recommend the authors to conduct a much more detailed analysis taking some recommendations onboard which could be as follows.

A) I see three principal methodological flaws that concern the model setting. My first concern is about the two-dimensional replication of the internal tides generation in the Andaman Sea. In terms tidal energy conversion, the area of Andaman and Nicobar Islands is a cluster of point sources of generation in which waves radiate from every particular source radially. This is clearly seen in SAR images depicted in Fig.1 Even far away from the generation points the fronts of internal solitary waves are still curved presenting their three-dimensional origin. In terms of modelling, an attempt to replicate such a complex three-dimensional generation and evolution using a two-dimensional approach leads to a great overestimate of the amplitudes/energy of propagating waves.

B) Of course, the model setting can be tuned, and model predictions can be corrected based on the model validation which assumes comparison the model predicted wave characteristics with observational data. This is not the case of the present paper, and this is my second concern. In many cases the consistency of the observational data with the model output gives a confidence that the predicted wave characteristics reflect elements of the real

wave dynamics. However, I did not find in the text any attempt to justify the modelling results comparing them with observations. The paper is built on the “trust me” basis.

C) The lack of model validation put a great question mark on the robustness of the predicted wave characteristics. Specifically, with such a coarse grid taken in model setting, Δz=500m horizontal step, and H_p~90m thickness of the surface layer, Fig. 4c, the leptic ratio $\Delta z/ H_p$=~5.5 which is >> 1. In such conditions I expect predominance of the numerical dispersion over physical, and all results related to internal solitary waves shown in Figs. 8, 10, 13, 15 can be artifacts generated by the model with great numerical dispersion, but not actual physical phenomenon. Much finer model resolution is required for making numerical dispersion at least at the level or much below physical dispersion.