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Interactive Comment

Interactive comment on "Features of fluid flows in strongly nonlinear internal solitary waves" *by* S. Semin et al.

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We are sincerely thankful to the reviewer for all the comments to our paper. The remarks and authors' responses are given below.

1. None of these results are particularly new or novel. There are papers that have compared laboratory experiments to numerical solutions and the authors should reference some of these (e.g papers by M. Carr and colleagues). Some of these papers use much better numerical modeling approaches.

Authors' Response: Nowadays many problems of fluid mechanics are being solved numerically in the framework of models for Euler or Navier – Stokes equations with the use of special software. There exists a variety of such models, all of them are quite complex, and it is not trivial to use and configure them to solve specific problems. Therefore it is necessary to have universal test problems (benchmarks) for each class of phenomena or processes modeled to help making comparison of accuracy for different models. Of course, modeling of an internal solitary wave with the help of numerical model of Navier – Stokes equations is not a new result itself. However, the results of simulations in the frameworks of different models (based on different numerical algorithms and realized with the help of different technologies) for the same equations have quite strong variations even for simplest tests. It should also be noted that there is not so many tests for the modeling of internal waves, and they are not universal for different models. That's why a comparative numerical study (comparison of the results from two models and from laboratory experiment) can be considered being of independent interest in the field of computational fluid dynamics. Moreover, the new results contain analysis of the reverse flow under the solitary wave and the investigation of trajectories of fluid particles in different vertical levels – they have a potential for studies of sediment transport in the near-bottom layer and transport properties of internal solitons with regard to passive impurities. We plan to include the obtained results into the set of benchmarks for MITgcm.

2. Indeed, in two dimensions it is possible to do DNS at these lab scales (at least of the propagation stage, if not the lock-release phase which be turbulent and 3D), and three-dimensional DNS is not that far out of the question. So I'm not sure of the value of tuning the MITgcm to the laboratory wave.

Authors' Response: Our aim was to verify the numerical model on some reliable and relatively simple internal wave solution, which is not easily available for scales of natural basins.

3. One then wonders how the other runs (i.e., the length vs amplitude results) depend on the tuning. Furthermore, details of how the tuning was done are not given other than to say it was done by comparison with the experiment.

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Authors' Response: The tuning was done in such a way to reproduce the width of generated solitary wave without losing accuracy in its amplitude, to accurately describe its propagation and reproduce the structure of the reverse flow behind the solitary wave.

4. The agreement between the MITgcm model and the experiment (wave amplitude (fig 5), wave width (fig 8) and horizontal velocities (fig 6a)), is not very good. I suspect that the disagreement between the two models and the laboratory result from the respective tuning (viscosity, etc and possibly resolution) of the two models and the fact that the modeling does not account directly for the three-dimensional aspects of the experiments, specifically the turbulent, dissipative lock-release and possibly lateral dissipation on the tank walls. The calculations used, approximately, the same lock conditions as the experiments, but do not account for the details of the lock-release, thus the tuned values for the diffusivity and viscosity must surely reflect these missing processes. It then becomes difficult to generalize the results.

Authors' Response: We agree that the 3-dimensional effects cannot be reproduced in a 2-dimensional model, but it is generally accepted that the effect of turbulence and dissipation are weak for long internal waves, and this is a general approach to describe such laboratory experiments with the use of 2-dimensional numerical models (Thiem et al., 2011; Hsieh et al., 2014) with good agreement on the stages of generation and propagation of internal solitary wave.

5. As for the comparison with the Gardner equation, the fact that this equation does not give quantitatively accurate results does not come as a surprise, especially in this case where the ratio of the upper to lower layer depths is relatively small (0.25), pushing the Gardner equation outside its range of quantitative validity. I think that the authors could have easily computed the same relations with the full-physics (nonlinearity, dispersion and continuous stratification) Dubriel-Jacotin-Long model and probably achieved better results. It would also give the limiting wave properties for the experimental stratification and may explain the deviation of the Turner and Vanden-Broeck two-layer model. Why is a similar comparison between phase speed and amplitude not shown?

Authors' Response: The Gardner equation is the simplest and most transparent model here, and it reproduces quite reasonable guess for the amplitude (phase speed) and width of solitary waves, though the case is formally outside the range of quantitative validity for this equation. The comparison with other listed models is also possible, but is out of scope of this paper due to its limited size.

6. The change in background stratification in the lee of the wave (end of p.7) attributed to "nonlinear effects" might result from the large vertical diffusion coefficient used in the MITgcm model, or sampling the model results in the dispersive tail. To simply point to nonlinear effects without justification is not enough..

Authors' Response: We agree with the reviewer, and changed correspondingly the text.

7. The bottom boundary layer flow reversal and, especially, the Lagrangian trajectory calculations have the potential to be interesting, but with only one case shown, it is difficult to say that much has been learned. I suggest that the authors look more closely at these aspects..

Authors' Response: This section is now extended to give the results from several numerical experiments with different amplitudes of initial box-like disturbance of both negative and positive polarities.

Interactive comment on Nonlin. Processes Geophys. Discuss., 1, 1919, 2014.