Nonlin. Processes Geophys. Discuss., 1, C893–C895, 2015
www.nonlin-processes-geophys-discuss.net/1/C893/2015/

Nonlinear Processes in Geophysics
Discussions
Open Access

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

Anonymous Referee #2

Received and published: 3 February 2015

This paper contains a two-dimensional numerical study of large-amplitude internal solitary waves that replicate existing laboratory experiments. The authors adjust the parameters (horizontal and vertical diffusivities and viscosities, and bottom boundary drag coefficients) of MITgcm model to obtain the best agreement between wave properties (shape, fluid velocities, etc) for one laboratory example. They then use the model to explore the relation between wavelength and amplitude (for the same set of laboratory-scale conditions) and then calculate Lagrangian particle motions in the primary model comparison run. The principal conclusions seem to me to be that the MITgcm can reproduce the experiments with somewhat better fidelity than the Bergen Ocean Model, that the wavelength vs amplitude results from the numerical model are not well-captured by the Gardner equation for the largest waves (approaching the conjugate state wave), and that the model gives bottom boundary layer flow reversal in

quantitative agreement with the lab experiment.

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. 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. 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.

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.

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?

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.

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.

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