Review
on the manuscript
by L. Shemer and B. K. Ee
“Steep unidirectional wave groups – fully nonlinear simulations vs. experiments”
submitted for publication in journal “Nonlinear Processes in Geophysics”.

Numerical and laboratory simulations of a self-modulating wave train, initially prescribed by the Peregrine solution of the nonlinear Schrodinger equation, are discussed in the paper in comparative manner. Special efforts have been made to enable such a comparison between the laboratory measurements and the fully nonlinear potential numerical simulations. One of the main objectives of the study is the kinematic breaking criterion. The authors suggest that waves start to break when the fluid particles on the top of the maximum crest overtake the wave crest. The paper is well-organized and well-written. It contains significant study; in particular, it represents probably the most thorough comparison between the laboratory and numerical simulations of the Peregrine breather. I assume it may be published in the journal. At the same time I see several serious drawbacks, and thus I would like to debate with the authors and to request a revision of the paper. In particular, I find questionable some conclusions of the paper.

1. The restrictive role of periodic conditions in the numerical simulations is drastically overestimated.

page 1170: *"...it should be stressed that periodic boundary conditions prescribed by the computational model imply that the mean value of the horizontal velocity is zero, and the values of the horizontal velocity at the boundaries of the computational domain vanish. This actually means that Stokes drift cannot be reproduced in the present numerical simulations. Note that significant Stokes drift was indeed documented in experiments by Shemer and Liberzon (2014)."*

page 1171: *"As stressed above, in the present computations the Stokes drift is absent as a result of the prescribed periodicity of the boundary conditions."*

page 1175: *"...inaccuracy is the lack of the Stokes drift in the computational results due to the imposed periodicity of the boundary conditions."*

page 1176: *"...stem from less than perfect accuracy of the model."*

page 1179: *"The prescribed spatial periodicity of the velocity potential effectively eliminates appearance of the 2nd order Stokes drift current, thus resulting in an inaccurate horizontal velocity at the liquid surface."*

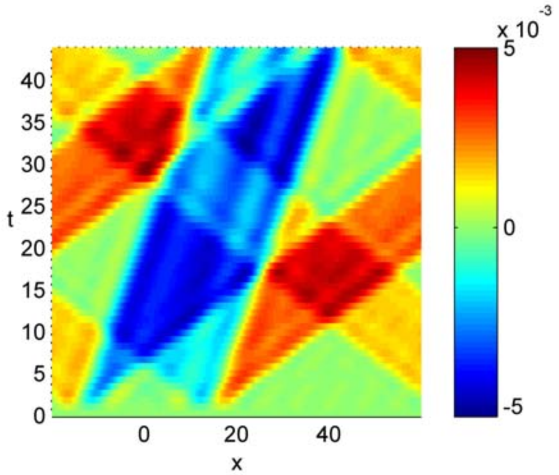
page 1180: *"...while the periodicity in the time domain is possible for propagating unidirectional waves, they are, strictly speaking, aperiodic in space. This point adds an additional aspect to essential differences that exist between the spatial and temporal formulations of the wave evolution problem, as discussed above. All nonlinear solutions that are based on spatially periodic boundary conditions, as in the method adopted here, as well as in a variety of alternative methods that employ spatial discrete Fourier decomposition, therefore contain intrinsic inaccuracy already at the 2nd order in the nonlinearity parameter ε. These numerical solutions thus can only provide approximate results and require careful experiments to verify their validity."*

The authors seem to get confused. It seems to be an axiom that if waves are well localized in the domain of consideration, then the boundary conditions do not matter (whether they are periodic as in the numerical tank, reflective as in a laboratory, etc). 'Waves' in the context of the employed by the authors numerical approach means 'surface elevation' and 'surface potential'. When there is a mean current, the surface potential is not localized. Thus, indeed, a mean current should be taken into account otherwise, for example, using the frames co-moving with the mean current. This is a trivial mathematical procedure which solves the problem. On the other hand, it is natural to consider a basin ***without*** mean current (at least the authors did not pose an objective to study waves on currents).

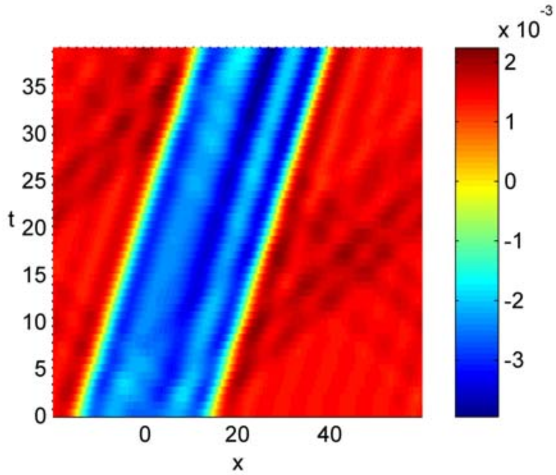
Meanwhile, if long-scale currents are generated in the course of wave evolution, this fact should be taken into account in view of the finite size of the domain, since long waves propagate quickly and may return to the area of interest. Thus, the simulation domain should be large enough, or the radiated long waves should be damped somehow. This circumstance occurs in numerical and laboratory basins both.

The numerical simulations presented in the paper are inaccurate not due to the periodic condition by itself, but due to the rough initial condition (since the bound waves are completely disregarded), and due to inappropriate size of the computational domain. The Stokes drift occurs at once as soon as the wave train starts to move. Since the initial condition does not take the induced current into account, long waves are radiated. In insufficiently large domain they interact with other waves and make the situation complicated. The induced long-scale current is even more important in the intermediate depth and definitively can influence the outcomes of the study in respect of the wave kinematics. Fig. 11, 12 with spectra do not show the long-wave range, though it is relevant for this discussion.

The correction according to the Dysthe theory can significantly reduce the level of radiated long waves. I give below pictures of the long-scale velocities (only very long harmonics are retained after spectral filtering) for two computer experiments similar to shown in Fig. 2 (fully nonlinear numerical simulations, similar but not equivalent conditions of the experiments). The left panel shows the case when the initial condition is given by the NLSE solution (like in the paper), while the right panel – when the Dysthe corrections are taken into account (induced flow, second and third harmonic). The modulated train is initially located within interval from $x = -17$ to $x = 17$ and then propagates rightwards; it induces negative current, which convoys the train. One may see that emerged long waves contaminate the left picture (the size of the computational domain is not sufficient, and the long waves interact with the train), while there are no significant artefacts in the right picture. The mean current is zero in both cases.



(NLS initial condition)



(Dysthe initial condition)

Thus, the source of errors was not identified correctly in the manuscript. There is no deficiency in the periodic boundary conditions by themselves. The comparison between the computer and laboratory simulations is possible, though the effects should be properly understood and controlled.

2. The bibliographic review is lopsided.

The Peregrine breather and many other analytic solutions have been reproduced in laboratory experiments many times by A. Chabchoub with colleagues [Chabchoub, A., Hoffmann, N., Akhmediev, N. Rogue wave observation in a water wave tank. Phys. Rev. Lett. 106, 204502, 2011 and subsequent papers]. The spectrum of the Peregrine breather was discussed in these works as well [Chabchoub, A., Neumann, S., Hoffmann, N., Akhmediev, N. Spectral properties of the Peregrine soliton observed in a water wave tank. J. Geophys. Res. 117, C00J03, 2012]. The comparison between laboratory simulations of the breather solutions and fully nonlinear simulations was given in [Slunyaev, A., Pelinovsky, E., Sergeeva, A., Chabchoub, A., Hoffmann, N., Onorato, M., Akhmediev, N. Super rogue waves in simulations based on weakly nonlinear and fully nonlinear hydrodynamic equations. Phys. Rev. E. 88, 012909, 2013]. The breather solutions have been already suggested for sea-keeping tests [Onorato, M., Proment, D., Clauss, G., Klein, M. Rogue waves: From nonlinear Schrodinger breather solutions to sea-keeping test. PLOS One 8, e54629, 2013]. It is unfair to overshadow these publications. They should be cited properly (pages 1161-1163, 1169 and so on).

3. The interpretation of analytic solution (6) is wrong.

page 1171: "*...the mean crest propagation velocity is somewhat higher than c_p due to ... the presence of the exponential term in Eq. (6),*"

page 1177: "*The solution (6) of the spatial form of the nonlinear Schrödinger equation (2) is asymmetric in space due to the presence of an exponential term. Similarly, the temporal form of the NLS equation (Lo and Mei, 1985; Shemer and Dorfman, 2008) yields PB that has an asymmetry in time.*"

The exponential term in (6) with imaginary phase is solely due to the difference between the group velocity and phase velocity. Since the NLSE is written in references moving with c_g, the wave phase varies with coordinate. If the fraction in the square brackets in (6) is put equal to zero, then (6) gives the plane wave solution. Thus, this term has no relation to the nonlinear velocity correction or to any kind of asymmetry.

4. The discussion of wave kinematics in page 1175-1176 is questionable.

The authors claim that due to the imperfectness of the numerical simulations the gap between u_h^{max} and v_{cr} (3% of c_p) may be shrunken. Both the velocities are computed in the same system of references, and therefore a common flow most likely will modify them similarly.

Looking at Fig. 10, one may assume quite the opposite – that the laboratory measurements were probably not accurately enough to claim the correctness of the breaking criterion. It would be great to have an estimate of accuracy of these measurements.

In contrast to laboratory measurements, the numerical simulations are almost exact. As I have already discussed, the 'problem' with induced mean flow may be easily overpassed. It is difficult to accept the suggested kinematic breaking condition if it not confirmed in numerical simulations.

5. Remarks to section 'Discussion and conclusions'.

page 1177, line 19: the boundary condition for a wave maker was generated in fully nonlinear simulations in [Slunyaev, A., Clauss, G.F., Klein, M., Onorato, M. Simulations and experiments of short intense envelope solitons of surface water waves. Phys. Fluids 25, 067105, 2013].

page 1178: "*This dependence that is very different from the analytical solution given by PB...*" – this point was not discussed in the paper.

page 1178, line 18-19: "*…to follow the experimental approach of Shemer and Alperovich (2013) and Shemer and Liberzon (2014). The theoretical solution given by Eq. (6) was truncated and tapered before being used to determine the initial condition at the wavemaker*." – I believe, this is a common approach since an infinite wave train cannot be reproduced in a finite basin. At least this approach was applied in [Chabchoub, A., Hoffmann, N., Akhmediev, N. Rogue wave observation in a water wave tank. Phys. Rev. Lett. 106, 204502, 2011] and consequent publications.

page 1180: "*The present study shows that the fully nonlinear solution, although flawed, yields better agreement with experiments than the application of the limited to the 3rd order spatial version of the modified nonlinear Schrödinger (Dysthe) equation that does not require spatial periodicity*." – the Dysthe model was not discussed in the paper. The sentence should be removed.

6. It is not clear, was the breaking observed in the numerical simulations or not, how the accuracy of numerical simulations was controlled.

page 1170: "*...simulations at larger times may become inaccurate*."

page 1173: "*...apparent breakdown of computations at* $t/T_0 \approx 62$."

At the same time in Fig. 2 the simulation continues up to 74 T_0. Thus, I am absolutely puzzled.

Less significant remarks:

7.1 Abstract: "*A method was developed to carry out detailed qualitative comparison of fully nonlinear computations with the measurements of unidirectional wave groups*". It is somewhat difficult for me to accept that the **method was developed** in the paper. The approach seems to be straightforward, and has been already used, for example, in the cited paper by Shemer & Dorman (2008), and also in [Slunyaev, A., Clauss, G.F., Klein, M., Onorato, M. Simulations and experiments of short intense envelope solitons of surface water waves. Phys. Fluids 25, 067105, 2013].

7.1 page 1161, line 7: Not only deep water wave groups are described by the NLSE.

7.2 page 1161, last paragraph: the discussion is not sufficiently accurate, since the **return** of the Peregrine breather has been observed in many experiments, but it was not the **full return**.

7.3 page 1162, first sentence: The MNLS equation cannot be a better model for the solution of the NLSE, it may be a better model for the physical effect. Please re-phrase.

7.4 page 1162, lines 11-12: the limit of applicability of the solution depends on the wave steepness, which is not given.

7.5 page 1164, line 27, page 1168, lines 7,8: notations for the surface elevation and vertical coordinate are probably not correct, please check.

7.6. page 1179, line 13: word 'crest' is used twice.

7.7 page 1182: wrong year in reference Perić et al (2015).

7.8. Fig. 8: I suggest using scaled by T_0 times for the figure.