Nonlin. Processes Geophys. Discuss., 2, C338–C343, 2015
www.nonlin-processes-geophys-discuss.net/2/C338/2015/

Nonlinear Processes in Geophysics
Discussions
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Interactive comment on "Steep unidirectional wave groups – fully nonlinear simulations vs. experiments" *by* L. Shemer and B. K. Ee

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Received and published: 23 August 2015

We thank the reviewer for his general support of our work. We also greatly appreciate the time and the effort invested by him in providing a meaningful review. We have adopted some of the reviewer's suggestions and corrected the errors and misprints noticed by him. Since there are, however, some points of disagreement between us, we welcome his suggestion to debate the differences. In the following we provide the detailed replies to the reviewer's comments. The reviewers' comments are in italics.
1. The restrictive role of periodic conditions in the numerical simulations is drastically overestimated. 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). This seems to be the major

point of disagreement. As it is our desire to address the reviewer's inputs, we would appreciate greater clarification as to what is meant by the statement "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)." We are unaware of any axiom which categorically states the boundary conditions do not matter. Moreover, it is generally known (including from personal experience) that reflections from the far end of the wave flume can change the wave pattern in the whole tank significantly. This is the reason why the wave energy absorbing beach is installed in all wave flumes (including ours), and active control of the wave maker is applied as well to remove secondary reflections in some tanks. In all our experiments, great care is taken to study the spatial evolution of wave trains of finite duration and we limit the measurements to locations that are sufficiently far from the end of the tank to mitigate the effects of secondary reflection, however minor they may be. As for numerical tanks, a similar approach may be adopted. Note that in recent years, numerous numerical wave tanks were based on various versions of the Boundary Elements Method and adopted by a number of research groups. The main motivation for those is to remove the requirement of periodic boundary conditions. One of the earliest attempts in this direction that we are aware of is the paper by Grilli et al. (Engng. Analysis with Boundary Elements 6 (2), 97-107, 1989). At the start of Section 2.5 in that paper it reads: "In the present paper, the Laplace equation is solved in the physical space, since we intend to generate non-periodic waves and to absorb them. The presentation is limited in a 2-D model, but can in principle be extended to 3-D problems". Moreover, regarding all experimental studies reporting on nonlinear unidirectional propagating wave trains published since the late 1970s, beginning from seminal papers by Yuen and Lake, we are unaware of any results indicating that spatially periodic boundary conditions can indeed be observed in experiments. "When there is a mean current, the surface potential is not localized." We wish to stress that the application of the method proposed by Chalikov and Sheinin (2005) or any other method of conformal mapping does provide the velocity potential (not just the surface elevation) in the whole domain of calculation. The Stokes drift current has a vertical velocity profile and it is basically concentrated in a thin upper layer only. The necessity for mass conservation in the experimental wave tank (closed at both ends) results in a slow backward current in the lower layers that practically does not affect waves under deep water conditions. This was also observed in our experimental facility. It is therefore impossible to apply the Galilean transformation (as suggested by the reviewer) to mitigate the effect of the Stokes' drift current. "The numerical simulations presented in the paper are inaccurate . . . due to the rough initial condition (since the bound waves are completely disregarded)," The bound waves are in fact accounted for in our simulations. We appreciate the suggestion by the reviewer to use Dysthe equations to calculate the initial condition. However, a different method was used in our paper. Prior to integration in time, the linear Peregrine Breather solution was modified to satisfy the governing equations; the initial conditions thus effectively include the bound waves as well and were obtained by applying an iterative procedure as described in Chalikov and Sheinin (2005). Also, our computational domain was longer than the effective length of the wave train. 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]. Shemer and Alperovich (2013) [hereafter referred to as SA] demonstrated that there were essential deficiencies in the original paper by Chabchoub et al. (2011). The spectra presented in JGR (2013) do not contain any quantitative data and hardly qualitative information as well, in a sharp contrast to SA. In private discussions with Amin Chabchoub, no evidence was presented by him to counter the points highlighted in SA. For these reasons, these two papers were not cited in our study. As for the paper by Slunyaev et al (2013), it will be cited in the revision of our manuscript although we wish to point out that many of the experimental and numerical results featured in that paper were already published in SA, half a year earlier. 3. The interpretation of analytic solution (6) is wrong. In principle, we agree

with the reviewer's comment and will replace the term "asymmetric in space" to "aperiodic in space" to stress the point that the analytical solution of the Peregrine Breather evolving along the tank lacks spatial periodicity. 4. The discussion of wave kinematics in page 1175-1176 is questionable. . . . one may assume quite the opposite – that the laboratory measurements were probably not accurately enough to claim the correctness of the breaking criterion. We believe that the comment is biased for the following reasons. In both Shemer and Liberzon (2014) and in the present manuscript, state of the art experimental methods were applied and we are curious as to what led the reviewer to question the accuracy of our results and the validity of conclusions. While it seems that the experimental findings by Chabchoub in PRL 2011 and later papers regarding the observability of breathers are accepted unconditionally and presented as truth, the reviewer questions the accuracy of our results without any substantiation. We would like to stress that the validity of the suggested breaking criteria was demonstrated in two independent studies with different experimental methods used. In Shemer and Liberzon, accurate Particle Tracking Velocimetry (PTV) measurements of Lagrangian velocity and acceleration were performed (ref. to Figures 4, 7, and 9 for velocity and Figure 10 for acceleration). The crest velocities at the inception of breaking were measured using digital video image processing. Moreover, the steepest crest propagation velocities proved to be in very good agreement with analytical solutions based on the Peregrine Breather. In that paper, the video clips were also presented as a supplement, providing visual evidence of strong acceleration of fluid particles at the inception of breaking. In the present study, the focus was made on detailed comparisons of fully nonlinear solutions in space with the time histories of the surface elevation measured at numerous locations along the tank. The instantaneous steepest crest velocities were obtained from data acquired by closely located probes. We do not see any reason to question the accuracy of our experimental results especially since the same level of scrutiny is not applied to other experiments. The possible reasons for minor discrepancies between experimental and numerical results are candidly discussed in the manuscript. 5. Remarks to section 'Discussion and conclusions'. In the present manuscript, we do not generate the boundary condition at the wavemaker. We just discuss the method to generate the appropriate initial wavemaker driving signal. As such, the reference suggested by the reviewer does not seem to be directly relevant. We see this Section as an integral part of the paper where the total body of results presented before are discussed from a common view point and without unnecessary repetitions. The truncation of an infinite wave train is indeed a common approach and was applied effectively in all studies of wave group propagation. We wish to clarify that we do not claim any element of novelty here and simply aim to provide a detailed description of the experimental accuracy. Shemer et al. have applied a similar approach since the late 1980s, as have many others. The Dysthe equation was indeed not discussed in this paper. An appropriate reference to SA will be given here in the revision. 6. It is not clear, was the breaking observed in the numerical simulations or not, how the accuracy of numerical simulations was controlled. The accuracy of numerical simulations was tested using the standard procedure of reducing the integration steps, as specified in the manuscript. We thank the reviewer for highlighting the apparent inconsistency regarding the late stages of the evolution. In our simulations the horizontal fluid velocity appears to be discontinuous at a t/T0 approximately 62. This also seems to fit with what is shown in Fig. 9 as the max crest height exceeds 3. We recall that these 2 plots are for the numerical simulations only of the temporal evolutions of spatially distributed functions. We clarify that this does not necessarily mean that the computation has to break down at t/T0 $\approx$62. The numerical computation breaks down when it just blows up. One of the goals of the paper is to determine up to what time do we want to set the applicability of the simulation results. Beyond that time, we say that the result is not useful. So since the simulation of the surface elevation ran till around 74T0, we used the data up to that time. However, as can be seen in Fig. 4 (surface elevations variation with time for fixed x values), using the data up to t/T0 = 62 does not affect the results shown in Fig. 10 - which aims to directly compare between experiments and the relevant post-processed results from the simulations. Less significant remarks: 7.1. Abstract - We will change the phrase "A method was developed…" to "A

method was applied…" 7.1. - We accept the remark and the manuscript will be modified to include intermediate depth. 7.2. - We question the observation since significant spectral broadening associated with the focusing of the Peregrine Breather seems to be irreversible. 7.3. – We accept the comment and will change the first sentence to "Shemer and Alperovich (2013) demonstrated that the modified nonlinear Schrödinger (MNLS, or Dysthe) equation was advantageous in describing the PB evolution along a laboratory tank as compared to the NLS equation (Dysthe, 1979)." 7.4. – The sentence will be changed "Shemer and Liberzon (2014) noticed that the spectral widening, being an essentially nonlinear process, occurs at slow spatial and temporal scales. Hence, it was found that the wave train behavior with background steepness of about 0.1 is still described by the PB solution of the NLS equation with reasonable accuracy, as long as the surface elevation spectrum remains sufficiently narrow and the maximum wave height in the train remained below approximately twice that of the background." 7.5. – The notation is correct. On page 1164, we refer to physical variables whereas on page 1168, the axes are defined according the conformal mapped space. 7.6. – We accept the comment, the typo will be corrected. 7.7. – We accept the comment and the year will be revised accordingly. 7.8. – We accept the comment and the figure will be corrected.

Interactive comment on Nonlin. Processes Geophys. Discuss., 2, 1159, 2015.