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Natural Hazards and Earth System Sciences
Discussions

Interactive comment on "On kinematics of very steep waves" *by* L. Shemer

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I certainly agree with the reviewer that the wave field parameters selected in this study are not applicable to real ocean conditions where the waves are random and have considerable directional spreading. This does not, however, necessarily mean that the conclusions of the study are irrelevant to ocean waves. The purpose of this note was to investigate the applicability of various breaking criteria based on different physical argumentation for well-defined, controllable and repeatable deterministic wave parameters. In particular, it is demonstrated that the dynamic condition of Phillips cannot be satisfied for a wide-spectrum wave train at the location and instant when the phases of all spectral components are identical. In this respect I want to clarify the issue of the wave steepness stressed by the reviewer. He is of course absolutely correct stating that wave breaking will eventually occur in the course of evolution of a unidirectional

random wave train with the initial steepness of about 0.12. The wave steepness for a random wave field, however, is routinely defined as a product of the wave number of the dominant wave, k0, by the characteristic wave amplitude represented by the rms value of the surface elevation variations. It is clearly stated that for the deterministic wave considered in the present study, the characteristic steepness is defined on the basis of the maximum crest height at the leading order, ζ0k0 (see paragraph following eq. 7). The rms definition of the wave amplitude is meaningless in this case, as only a single finite-duration wave train is excited in every experimental run. The wide wave train with a complicated envelope generated by the wavemaker (see Shemer et al. 2007 for relevant records) focusses at the prescribed location due to dispersion and nonlinearity, to form a short envelope with a Gaussian shape. Beyond that point, if its evolution is followed for a sufficient time and distance, it would gradually disintegrate due to wave dispersion. Contrary to the statement of the reviewer, if such a deterministic wave train does not undergo breaking when its crest's height attains maximum, it will never break. The exact definition of wave breaking is in fact not necessary in the framework of the present study that is based on experimental observations. It is stated in the manuscript that for ζ0k0=0.3 and T0=2.8 s the water surface in the experiment at the focusing location remained smooth (and the measured temporal and spatial evolution of the surface elevation was in a very good agreement with simulations based on the spatial Zakharov equation, see Shemer et al. 2007). For a longer wave with T0=4.34 s and the same steepness, plunging breaker was observed. Both these observations were video recorded in 2003; these video clips were presented at numerous international meetings and can be provided if requested. Decrease of ζ0 by 10% for the dominant wave period of T0=4.34 s eliminated this breaking; very good agreement again was observed for wave shape evolution along the tank for this value of T0 and ζ0k0=0.27. The theoretical analysis of the kinematic wave parameters at the crest is thus performed for two wave trains with identical maximum steepness of ζ0k0=0.3: no breaking was actually observed in experiments for a shorter wave (T0=2.8 s), while a plunging breaker was clearly visible for a longer wave. Although there is no doubt that the conformal transform method for solution of the problem of evolution of a unidirectional water wave field that is periodic in space often has important advantages as stressed by the reviewer, I cannot accept his statement that the expansion in orders and certainly the Fourier analysis are used just because of tradition. There are compelling reasons for co-existence of different approaches for the analysis of water waves; the discussion of these issues is however beyond the scope of this document. As implied above, I accept the comment by the reviewer about the possibility to apply alternative methods to attack the problem theoretically. The Introduction of the manuscript was therefore extended substantially in this revision and some other ways to carry out the analysis are briefly reviewed. The appropriate references are added. The reasoning for selecting the Zakharov equation as the theoretical model is also presented. It should be stressed in this context that in our previous combined experimental and theoretical studies of wave trains with different spectral shapes and widths it was demonstrated that the Zakharov model describes adequately the evolution of the surface elevation shape of steep and essentially nonlinear gravity wave trains, with the difference between the computed and the measured surface elevation variation in time and space remaining within the experimental error. The results of the present study, however, seem to show that this is not the case when wave kinematics is concerned, and more accurate methods are needed, as is now acknowledged in the Conclusions. I hope that in view of these clarifications and the numerous modifications that were introduced in the revision the reviewer will find the manuscript acceptable for publication in NHESS.

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