

Interactive comment on “Rogue waves in a wave tank: experiments and modeling” by A. Lechuga

Anonymous Referee #2

Received and published: 29 August 2013

In the revision the author made an obvious and honest effort to address my critical comments. The paper has been improved significantly. However, there are still numerous inaccuracies and inconsistencies that should be dealt with before final acceptance. Since the titles of the axes in the figures are now provided, it becomes possible to address the quantitative aspects of this manuscript. It is clear from the text that the experimental facility mainly serves for modeling field conditions. The structure visible in Fig. 1 simulates a dike with scaling of 1:39. While this scaling is irrelevant in the context of the present study, it is nevertheless used selectively in presentation of the data. Such selective scaling only leads to confusion. The wave frequencies and the wave heights in the figures and the Table do not represent actually measured quantities. The paper should specify real parameters in appropriate units. The author claims that deep water waves are studied, but there is no data on the actual wave lengths. The location along the tank of the unrelated to this study structure is specified, but no

C999

quantitative data is given on the coordinates of wave sensors. The claim that no substantial change between the wave shapes measured by the sensors is observed is not supported by presented data. Since neither the dominant wave length nor the sensors' locations are known, it is unclear whether the constant shape is a result of the short distance between the sensors, or, as the author states, it can be attributed to the lack of instability (p. 4, line 15). In the latter case, it is unclear why deterministic waves are “outside of modulation instability mechanism.” Even the simplest deterministic Stokes wave is unstable due to Benjamin-Feir instability! The input signal is only characterized by its power spectrum, no data on the actual wave shape is given. Table 1 contains the scaled wave heights; the temporal information is missing. The author claims that there is “an evident energy concentration”. It seems, however, that no such concentration actually occurs as a result of wave train evolution; the appearance of a steep wave is prescribed by the wavemaker input signal. It is unclear how the dependence of the surface elevation on the longitudinal coordinate was obtained in the experiments (see, e.g. Fig. 8). With only 3 wave gauges, this cannot be done without adopting some additional assumptions. It would be better to present the measured by the sensors temporal dependence of the surface elevation. The GL equation solutions can also be given as a function of time at the wave sensor location. The physical meaning and the actual values of the coefficients in the GL equation (1) remain not clarified. It is claimed that ε represents “the wave peaked-ness”. This quantity should be defined. From the scaling given in (3) and (4) it seems that ε represents in fact the small parameter of the problem characterizing wave steepness. If indeed so, the equation apparently contains terms of different orders and is thus apparently inconsistent. It is specified (p. 5, last line) that $\sigma=1$ and $\varepsilon=0.5$ (hardly a small number!). It is later stated that the experimental conditions correspond to $\varepsilon=0.085$ and $\sigma=0.029$ m². If I understand correctly, the 1st set of σ and ε pertains to the dimensionless version of eq. (1). What are the value and the dimension of h? If ε is dimensionless, then the units of σ should be m⁻². How the dimensional GL equation looks like? In the form given by (1) the equation is not dimensionally consistent. I hope that the author can provide constructive replies to

these queries to make the manuscript acceptable for publication in NHES.

Interactive comment on Nat. Hazards Earth Syst. Sci. Discuss., 1, 3201, 2013.

C1001