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Interactive comment on "Collisions of two breathers at the surface of deep water" *by* A. I. Dyachenko et al.

Anonymous Referee #2

The authors present numerical solutions of the new Zakharov-type equation, "improved" in one-dimensional case by an additional canonical transformation, which explicitly utilizes the well-known but somewhat mysterious fact that the coefficients of resonant four-wave interaction are identically zero for one-dimensional gravity water waves. Considering the collision of two breathers numerically, the authors report that the collision appears to be non-elastic (although a large number of collisions is needed to see this), and interpret their result as a numerical proof of non-integrability of the new Zakharov equation.

In principle, the result is clear and of interest, and the paper should be published. My concerns are about the presentation of the results. It seems to me that the authors do not make sufficient effort to make the text accessible to a wider audience. They

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should clearly explain why they expected integrability (or maybe non-integrability - it is not quite clear from the text) in this case. Besides that, although the canonical transformation has been published earlier, the present text should also be self-contained. For example, it is quite hard to understand why the "projection operator" is introduced, without referring to earlier papers. Also, since in the paper the numerical results are effectively used a proof of analytical concepts, the numerical method employed should be presented very clearly, and its properties should be discussed. Currently it is not the case. In fact, Eq. (10) is just introduced without any reference, and even the notation is not explained (what do the angular brackets mean?). The authors should clearly explain why the eventual (at very large time) emergence of something that looks like small numerical noise can lead to theoretical conclusions. At the moment, they only mention the fact that the energy is conserved with very high accuracy. This may not be sufficient, since in some cases numerical errors may appear without breaking the energy conservation; the authors should explain why it is not so in this case. Finally, it seems to me that the scale of some figures may be reconsidered (e.g. it is hard to compare figures 7 and 9 in this scale), and figure captions should be expanded.

Interactive comment on Nat. Hazards Earth Syst. Sci. Discuss., 1, 3023, 2013.