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Interactive comment on "A comparison of the measured North Sea Andrea rogue wave with numerical simulations" *by* E. M. Bitner-Gregersen et al.

Anonymous Referee #1

Received and published: 9 December 2013

The paper presents a numerical investigation of the Andrea rogue wave event. The approach is based on the coupling of the ECMWF hindcast data (corresponding to the output of the WAM model), together with a Higher Order Spectral Method (HOSM). The procedure presented here is interesting, and should be published in the Natural Hazards and Earth System Sciences. However, several points are not clear, sometimes contradictory, and should be explained.

First of all, the paper concludes that the results obtained through this procedure agree satisfactorily with the data recorded at the Ekofisk field. This statement is far from being clear from the text, for the following reasons.

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Indeed, I found no mention in the text of data from the Ekofisk field (except those in table 1). As far as I understood, data plotted on figures 2, 3, 4, and 5 are obtained from the WAM model (the text is not clear on that point). The origin of figure 6 is not clear neither. Is it an output of the WAM model, or an ensemble average of the simulations with HOSM, initiated with the corresponding WAM data? In any case, I could not understand how to compare the results with any data from the field.

Furthermore, a sentence at the end of section 4 states that the location used in simulations is not the same as location of the Ekofisk field. Another, just before, states that the exact time of the event is not covered by the simulation. These arguments are used to explain differences with the occurrence of the Andrea rogue wave. Are they statistically relevant, or not? And thus, why were these choices made for?

Then, results of the HOSM model are plotted on figures 7, 8 and 9. The time scale presented is really different from the time scale of the WAM model. It is found that skewness, kurtosis, and normalized wave height vary significantly within this time scale. However, it is not clear how this result can be interpreted.

Finally, in abstract, it is claimed that wave profiles comparisons are made with the Ekofisk records, but I could not find such a figure in the document.

Secondly, concerning the procedure of coupling itself, and the model used, some details are needed. The equations presented in section 4 suggest that the HOSM model used is a 2D version. The initialization of such models, however, is not straight forward, especially in two dimensions of propagations. Indeed, the conversion of linear spectrum into nonlinear surface requires a detailed procedure, and several of these procedures exist (see G. Ducrozet, F. Bonnefoy, D. Le Touzé, and P. Ferrant, "3-D HOS simulations of extreme waves in open seas", Nat. Haz. Earth Sys. Sci., 7, 109-122, 2007 and references therein). What was the choice of the authors?

Finally, concerning the form,

- Figures are small, and not easily readable. Is it possible to enlarge them? At

least, the axis fonts should be enlarged.

- Some of the references cited in the text did not appear in the reference list (Magnusson Donelan 2013, Bitner-Gregersen and Toffoli 2012, Krogstadt et al. 2008)

- In section 2, is 'a low' referring to a low pressure cell?

Interactive comment on Nat. Hazards Earth Syst. Sci. Discuss., 1, 5033, 2013.

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