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Natural Hazards and Earth System Sciences
Discussions

Interactive comment on "Modulational instability and rogue waves in finite water depth" *by* L. Fernandez et al.

Anonymous Referee #1

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In this work the fifth order spectral HOSM model was used for investigation of a rate of energy exchange between a carrying wave and disturbances. Two configurations of disturbing modes was considered, i.e., oblique and collinear. The main results are given in Fig. 6, 7 and 8. It is shown that the amplification rate for oblique disturbances is larger than that for collinear disturbances. At small depth the instability is blocked completely.

General comment. I am not a great expert in the instability theory, but I was always wondering about the obvious contradiction between different works devoted to the modulation instability and analytical B.-F. and semi-analytical McLean theories. Both theories investigate growth of the side modes interacting with the Stokes wave (in B.F. theory the second order Stokes wave was considered, while in Mc-Lean theory it was

the exact Stokes wave). Evidently, the use of the linear harmonic mode as a carrying wave can not show the instability predicted by both theories.

It would be very interesting to see why the instability does develop in the numerical investigation case when a linear carrying wave is considered.

The paper considers growth of disturbances with time as a result of a nonlinear energy exchange between modes. The evolution of energy presented in Fig. 3 does not show that amplitudes of the disturbances grow monotonically, i.e., on the contrary, they show rather a chaotic behaviour after t=100-150. Expecting of 'a rogue wave' in such cases seems to be unjustified. Who knows what is going to happen then? Such a slow development of the amplitudes of the side modes contradicts to all known data confirming that an extreme wave develops very quickly. The results presented in the paper show that additional modes can increase, but it is unclear in what way such effect is connected with the rogue wave phenomenon. Development of new modes was also observed in B-F an McLean instability theories, however, these works do not pretend to be the investigations of the freak wave phenomenon. In reality, the configurations investigated in the work always exist in the real sea with a developed spectrum, so, according to the schemes based on the modulation theory, freak waves do exist permanently while a rogue wave is quite a rare phenomenon which manifests itself as development of an extremely large wave over a very short period of time. It is quite possible that the development of the instabilities in a wave field with a rich spectrum differs from such development at the idealised conditions investigated in the paper. This is why, I believe, mentioning of 'rogue wave' in a title of the paper is sort of misleading. Otherwise, the Benjamin, Feir paper and McLean et al and all the papers on modulation instabilities can be also referred to as the papers investigating the rogue waves. I do not think it is correct. All the observational data show that a rogue wave appears suddenly without any prehistory, while the paper demonstrates a very slow and irregular growth of energy with a vague result.

I would recommend the authors to rename the paper or give a solid explanation why the slow and irregular growth of the side modes may result in generation of an extreme wave. In the current form the paper rather rejects the modulation instability process as a possible mechanism of the freak wave generation.

Specific comments

1. It is not quite clear, how the curves shown in Fig. 3 could give the results shown in Figs 6-7. The method of calculation of the amplification factor should be explained. 2. I am wondering why the authors prefer to use a dimensional form of the presentation. The equations are self-similar while the nondimensional form is more general. 3. Reference to Zakharov et al (2002) paper is irrelevant, since those authors use the one-dimensional approach which was known long before. 4. A choice of the specific configuration of perturbations is not explained. It is possible that at different configuration the result might be different. 5. Fig 4 is large, complicated and not informative, especially for the collinear modes. The fact that they are not growing is shown in Fig. 3. 6. The result in Fig. 9 demonstrates a large difference between 3rd and 5th orders. It proves that the HOSM model is inaccurate, even for an idealistic wave field. For simulation of the high and sharp freak waves the HOSM model should have a far higher order of nonlinearity. Unfortunately, the high orders in the HOSM scheme introduce a high risk of the numerical instability. A high order HOSM model works well for the cases of the narrow spectrum, low total steepness and absence of high local inclination. The HOSM model of a low order creates artificial viscosity suppressing the high-wavenumber modes and providing robustness of the model. Probably, this is why this model is so popular.

I would recommend revision of the paper.

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