Response to the Anonymous Referee #1

We would like to thank the Referee #1 for the valuable comments.

COMMENT BY THE REFEREE
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 wasthe exact Stokes wave). Evidently, the use of the linear harmonic mode as a carrying wave cannot 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.

ANSWER BY THE AUTHORS
We thank the Referee for this comment. Our simulations consider linear harmonics as initial conditions only. Within less than a wave period, the Euler equations automatically build up the Stokes' contribution. This is to say, the model actually provide the evolution of a Stoke's wave, in total agreement with theory. Note also that formation of B.-F.-like instability has been verified by starting the simulation with a simple monochromatic waves seeded by 2 side bands. In the amended version of the manuscript, we have added a discussion concerning the formation of Stokes' waves to clarify this issue.

COMMENT BY THE REFEREE
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 behavior 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.

ANSWER BY THE AUTHORS
For deep water and collinear perturbations, simulations show a monotonic (and recurrent) growth of the amplitude in agreement with theory (cf. Janssen 2003). We agree with the Referee that the behaviour recorded under the effect of oblique perturbations is a bit more erratic, showing an oscillation of the maximum amplitude after about 100 periods. Note however that the simulations clearly indicate that there is still a monotonic growth within 100-150 periods, which leads to a clear amplification of the wave amplitude of about two times the initial value. No more recurrence is observed under this configuration, though. In the revised version of the paper, the erratic behaviour observed for oblique perturbation has been discussed.

Regarding the slow development, it is important to mention that the obtained time-scales are consistent with the generation of rogue waves in the laboratory. Simulations with HOSM performed by Toffoli et al. (2010), in this respect, show that the evolution of the wave field as predicted by the HOSM is in very good agreement with experimental observations. It should be mentioned, however, that directionality and limitations on water depth are responsible for slowing down the instability process, further delaying the formation of extreme waves. In the amended version of the manuscript, reference to agreement with laboratory experiments is added to clarify the reliability of the numerical simulations.

COMMENT BY THE REFEREE

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 and McLean instability theories, however, these works do not pretend to be the investigations of the freak wave phenomenon.

ANSWER BY THE AUTHORS

We agree with the Referee. The intention of our research is indeed to demonstrate that rogue waves are generated by the presence of 4 oblique unstable modes located ad-hoc in the instability region (see Figs. 1 and 2 in the manuscript). The fact that other modes may arise during the evolution of the wave packet is not directly related to the formation of freak waves. It is likely that this concept was not well presented in the manuscript, leading to a misinterpretation of our work. In the revised version, the idea and aim driving our research has been carefully reviewed to avoid confusions and misinterpretations.

COMMENT BY THE REFEREE

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 idealized conditions investigated in the paper.

This is why, I believe, mentioning of 'rogue wave' in a title of the paper is sort of misleading.

ANSWER BY THE AUTHORS

We agree with the Referee. This configuration always exists in nature due to the broadness of the ocean wave spectrum. Our research has only considered 4 of the most unstable modes to verify the survivability of modulational instability and the concurrent formation of extreme waves in water of finite depth ($kh < 1.36$). Note that we do not differentiate between the term freak or rogue in our manuscript. The presence of additional modes in a more realistic spectrum may affect our results either enhancing or decreasing the intensity of the modulation-induced effects. The full understanding of the role of modulational instability in broad directional sea states in water of finite depth has to be investigated further to be fully understood. This has been highlighted in the conclusion.

COMMENT BY THE REFEREE

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.

ANSWER BY THE AUTHORS
We thank the Referee for this comment. Our manuscript, as many others (see Osborne et al. 2010 and references therein), points out that modulational instability is a plausible mechanism for explaining the formation of extreme wave. This claim is here supported by numerical simulations, which show how an initial regular wave doubles its amplitude when perturbed by unstable modes. In our opinion, the title reflects our results.

Our main result, furthermore, is that a significant amplification can be achieved also for relatively shallow water with $kh < 1.36$, provided the perturbations are properly selected within the instability region. This is also in agreement with other experimental and numerical studies (Yue and Lake 1982; Trulsen and Dysthe 1996; Trulsen et al. 1999; Tulin and Waseda 1999; Gramstad and Trulsen 2011; Toffoli et al. 2013). We agree that wave amplification in finite water depth is rather small and that the term rogue waves can be misleading. In the revised version of the manuscript we therefore have modified the title that now reads: “Modulational instability and wave amplification in finite water depth”.

We would also like to mention that modulational instability needs some time to force the evolution of a wave packet into a sea state with ‘extreme waves’. This time scale depends on the wave steepness and it is on the order of few tens of periods and hence in agreement with numerical and experimental results (see Socquet-Juglard et al. 2005; Onorato et al. 2009; Waseda et al 2009; Toffoli et al. 2010; Osborne 2010; Toffoli et al. 2013, among many other). Note also that processes related to wave instability slow down with decreasing water depth (e.g. Janssen and Onorato 2007), as reported in the present manuscript.

In the amended version of the manuscript we have stressed more clearly the robustness of our results.

COMMENT BY THE REFEREE
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.

ANSWER BY THE AUTHORS
Figs. 6-7 have been obtained by selecting the maximum wave amplification in all simulations. Note that they summarize all configurations that we tested, while only a few cases are reported in Fig. 3. In the revised version of the manuscript, explanation of Figs. 6 and 7 has been improved.

COMMENT BY THE REFEREE
I am wondering why the authors prefer to use a dimensional form of the presentation. The equations are self-similar while the non-dimensional form is more general.

ANSWER BY THE AUTHORS
We thank the Referee for this comment. The present model used dimensional equation for convenience. Knowing that the process is self-similar, results are presented in a non-dimensional form.

COMMENT BY THE REFEREE
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.

ANSWER BY THE AUTHORS
We thank the Referee for this comment. The main message is indeed covered by Figures 3 and 6 and does not really need extra emphasis. Figs. 4 and 5 have therefore been eliminated from the amended version of the manuscript.

COMMENT BY THE REFEREE
Reference to Zakharov et al (2002) paper is irrelevant, since those authors use the one-dimensional approach which was known long before.

ANSWER BY THE AUTHORS
We thank the referee for this comment. Reference to Zakharov et al. (2002) can be and has been removed from the manuscript.

COMMENT BY THE REFEREE
A choice of the specific configuration of perturbations is not explained.

ANSWER BY THE AUTHORS
We thank the referee for this comment. A justification for the selection of the sided bands has been added to the revised manuscript.

COMMENT BY THE REFEREE
It is possible that at different configuration the result might be different.

ANSWER BY THE AUTHORS
Whatever the combination of side band is, the result is expected to be the same if perturbations are selected within the instability region. Note, however, that the process can slow down significantly if side bands come from the marginal zones of the instability region. If perturbations are selected outside the instability region, modulational instability does not develop and this amplification will not be observed.

COMMENT BY THE REFEREE

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.

ANSWER BY THE AUTHORS

We thank the Referee for this critical comment. Limitations of the HOSM are now more clearly mentioned in the revised version of the manuscript.