We are deeply touched by the careful review and so detailed comments made by the referee two. Such kind of discussion greatly helps us to clarify the results and to make the text of paper more readable.

1. Probably the most hurting point is how the wave breaking is introduced in the model (Question 3 of the First Referee).

I believe that Section 4 should look like follows. Firstly, it makes sense to emphasize that the wave breaking effect will be taken into account by virtue of extra terms, which are artificially added to the already obtained equations for dispersion relation and wave energy. Then the wave breaking parameterization by Tulin (1996) and Huang et al (2011) may be cited with the NLS equation (with clear reference), briefly discussing the role of all the non-classic terms. Why all the terms should be taken into account? Isn't it superfluous, could not it be simpler? At this stage (or before) it seems necessary to discuss why the dispersion relation needs correction jointly with the wave energy laws, and of what sort. Finally, the way how the NLS terms are adapted to the three-wave system should be discussed.

Text of the dissipative model introduction is rewritten:

Dual non-conservative evolution equations for wave energy density $E = 1/2g\eta^2$ and wave momentum $M = E/c = E/(\omega/\mathrm{k})$, or, correspondingly, for energy E and celerity c were rigorously derived (Tulin and Li, 1999) using the variational approach: a modified Hamiltonian principle involving the modulating wave Lagrangian plus a Work Function representing the nonconservative effects of wave breaking. It was also shown that these dual equations correspond to the complex NLS equation, as modified by the non-conservative effects, i.e. to energy and dispersion equations. Wave breaking effects were characterized by the energy dissipation rate D_b and momentum loss rate M_b.

An analysis of fetch laws parameterized by Tulin (1996) reveals that the rate of energy loss D_b due to breaking is of fourth order of the wave amplitude:

$$D_b / E = \omega D \eta^2 k^2,$$

and $D = O(10^{-1})$ is a small empirical constant. Momentum-loss rate M_b was quantified in terms of energy dissipation rate D_b and parameterized in such a way that

$$cM_b = (1+\gamma)D_b,$$

where γ is empirical coefficient, which is varied in the range $\gamma = 0.4 - 0.7$. Strong plunging breaking corresponds to $\gamma = 0.4$ and weak to $\gamma = 0.7$. In all our numerical simulations was chosen $\gamma = 0.4, D = 0.1$.

Tulin & Waseda (1999) through consideration of a multi-modal wave system evolving from a carrier wave and two side bands, showed that energy downshifting during breaking is determined by the balance between momentum and dissipation losses, suitably parameterized by the parameter γ:

$$\frac{\partial}{\partial t}\left(E_0 - E_2\right) = \gamma D_b / (\delta\omega/\omega),$$

where E_0, E_2 - energy densities of the sub and super harmonics, respectively. The parametric value γ was found to be positive $\gamma > 0$ and providing the long term downshifting.

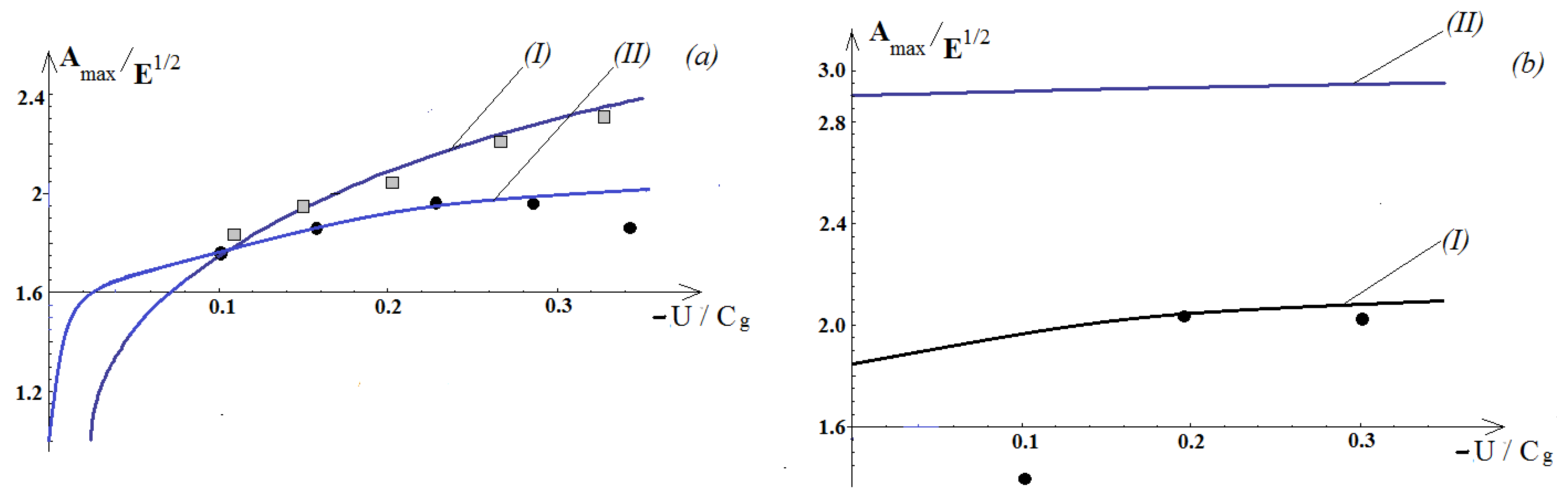
The sink of energy D_b and momentum M_b due to wave breaking leads to additional terms at the right sides of the wave energy equations (20) and dispersive equations (16) for each of the waves. Tulin (1996) suggested using sink terms along the entire path of wave interaction with the wind. The wave dissipation function for the adjusted model (Huang et al., 2011) includes also the wave steepness threshold function

$$H\left[\frac{\left|\varepsilon\sum\sigma_i\phi_i k_i\right|}{A_S}-1\right],$$

where H is the Heaviside unit step function and A_S is the threshold value of the combined wave steepness $\varepsilon\sum\sigma_i\phi_i k_i$, is applied to calculate energy and momentum losses in a high steepness zones. In our computations the threshold was chosen $A_S = 0.32$.

2. My second remark concerns the results of numerical simulations given in Fig. 2 and compared with available laboratory measurements. On the one hand the new curves agree the laboratory measurements noticeably better, as far as understand, due to a more accurate description of the current variation with distance. On the other hand, the difference between the old and new curves is so striking that I cannot understand how this difference could happen. Most important, I cannot accept the new curves due to physical reasoning.
Fig. 2a. In the experimental runs by Toffoli et al (2013) the initial condition (in calm water) corresponds to the case when the BF instability does not act (BFI ~ $\sqrt{2}\varepsilon N$ <1), thus $A_{\max}/E^{1/2}=1$ when U = 0. Why does the resonance system by I. Shugan et al give another value larger than one (i.e., modulation occurs)?
Fig. 2b. I do understand why the analytic forecast from Toffoli et al (2013) overestimates the laboratory observation. The analytic model considers the given length of modulation, and ends up with estimation for this particular length of perturbation. In reality other (shorter) perturbations may have larger growth rate, and thus develop quicker resulting in eventually smaller wave amplification. In case T11 of Ma et al (2013) BFI ~ $\sqrt{2}\varepsilon N$ ~ 3.2, the corresponding length of perturbation is much longer than the most unstable one (in these variables the most unstable situation corresponds to (BFI ~ $\sqrt{2}\varepsilon N$ ~ $\sqrt{2}$), and the growth rate is much smaller. The model used by I. Shugan et al fixes the harmonics under consideration (only one mode of instability) and thus is unable to capture the discussed effect. Therefore the almost perfect agreement between the laboratory and simulated data in Fig. 2b surprises me very much.

Initial conditions in Toffoli et al. (2013) experiments have the value of BFI ~ $\sqrt{2}\varepsilon N$ ~ 0.98 which is really close to the threshold of instability. Even very small adverse current can triggered the instability. Our model have the correct limiting value $A_{max}/E^{1/2}=1$ when $U = 0$, but we have not draw the graph of amplification up to it following to the small restriction of Toffoli et al. (2013) estimation (it cannot be prolong up to exactly point $U=0$). The graph of amplification at Figure 2a is now extended up to limiting value $U=0$.



The prediction for the maximum wave amplitude obeys the following equation (Toffoli et al., 2013):

$$\frac{A_{max}}{\sqrt{E}}=1+2\sqrt{1-\left[\frac{\exp(U_0/c_g)}{\sqrt{2}\varepsilon N}\right]^2}$$

It looks like the initial value of BFI ~ $\sqrt{2}\varepsilon N$ in this formula formally can be chosen freely inside the instability region $\sqrt{2}\varepsilon N>1$. Experimental results (Toffoli et al., 2013) show that this estimation is workable for the initial BFI~1. For much bigger initial BFI values we can image the idea of this formula is to estimate to amplitude amplification for very special rogue wave events (may be really for some special initial values of BFI in dependence from current conditions). It means, that if rogue wave already take a place, probably its amplification will correspond to the presented formula. To receive in experiments regular rogue waves from the initial 3-wave system for different values of adverse current seems to be weakly solvable problem. Experiments of Ma et al. (2013) confirm such a conclusion: we don't see any freak waves in the experiments and probably that is why analytical forecast of Toffoli et al. (2013) strongly overestimate the observed amplitude amplification.

Our 3- wave system in principle cannot reproduce freak waves events only due to redistribution of wave energy (it need at least five discrete waves with almost equal amplitudes and coinciding phases) even under the impact of adverse current. Nevertheless amplitude amplification up to factor two can be described by our system and we see satisfactory correspondence to the laboratory observations.

3. *Maybe the present title is not perfect since it does not distinguish the paper from the remaining bulk of publications. I*

suggest the authors to consider more particular title highlighting the peculiarity. May be "Dynamical resonance model for BF instability of waves in the presence of current" would be suitable?

Suggested title looks more corresponding to the content of paper. Title of paper is changed

4. The answer on Question 1 of the Fourth Referee (and corresponding extension of the introduction) does not sound too much convincing for me. It is difficult to argue with the general statement that when only three (ω, k) components are taken into account, they provide only limited description. The wave blocking effect prevents propagation of small scale waves upstream, but the waves still can propagate in the other direction. In the case of wave breaking small-scale components will lose energy, and then the model should include empirical assumptions, thus does not follow from the primitive water equations. I would suggest to soften the authors' statement in the Introduction.

The corresponding statement in the Introduction is softened:

"Nevertheless the experimental results of Chavla and Kirby (2002) and Ma et al. (2010) on the modulation instability under the influence of adverse current show that energy spectrum is mostly concentrated in the main triad of waves and high frequency discretized energy spreading is depressed due to the short wave blocking by the strong enough adverse current. Higher side band modes have also prevailed energy loss during wave breaking (Tulin and Waseda, 1999). That is why we hope that our simplified 3-wave dynamical model still have potentiality to adequately describe some prominent features of wave dynamics on adverse current."

5. A few lines of description of the linear modulation model by Gargett & Hughes (1972), Lewis et al (1974), which mentioned at line 339, are necessary.

Text added:

Wave refraction by current is acting simultaneously with nonlinear wave interactions. The linear modulation model (Gargett & Hughes, 1972; Lewis et al., 1974) assumes the independent variations of harmonics with current and gives much larger maximum amplitude of the carrier wave (IV). It just adsorbs energy from the adverse current.

6. Is the breaking parameterization (including constants) the same for results shown in Fig. 3, 4, 5?

The breaking parameterization is the same for all numerical simulations including constants ($D = 0.1, \gamma = 0.4$). All the values are clarified in the revised text.

7. *I cannot agree with the statement in Conclusion "The steepness nonlinear wave on adverse current is much less than that of a linear refraction model". In Fig. 1 such a comparison is provided for the surface displacement amplitude of interacting wave harmonics, but not for the steepness of harmonic superposition.*

We agree with this comment. The property of carrier is tangled with waves train behavior. The phrase "The steepness nonlinear wave on adverse current is much less than that of a linear refraction model” is removed.

8. *I suggest removing the word ‘explosive’ in the last sentence of Conclusion to avoid possible misunderstanding.*

The word “explosive” is really “provocative” and removed.

9. *I am not sure that the representation of expressions with braces in Eq. (15), and especially in Eq. (21), (22) is the best, and find it difficult for reading and interpretation. I suggest to rewrite the expressions in a simpler form.*

Equations (15) are rewritten in a more compact form and (21), (22) (dissipative terms) are simplified with acceptable accuracy (in accordance with the comment of referee one also)