Review
on the revised manuscript
by I.V. Shugan, H.H. Hwung, R.Y. Yang
"Benjamin-Feir instability of waves in the presence of current"
submitted for publication in journal "Nonlinear Processes in Geophysics".

The authors have generally addressed all my questions and remarks to the first version of the manuscript. I believe that their work desires publication, and that the developed model may be useful. On the other hand, the additional explanations in authors' responses help me to have a closer look on Section 4 (simulations with breaking), which I find unsuitable for publication in the present form. I also have important questions concerning the modified Fig. 2. Besides, some less important remarks are given further.

1. Probably the most hurting point is how the wave breaking is introduced in the model (Question 3 of the First Referee). In my opinion, the modified text is poorly written and makes this problem even less clear than before (when details were hidden).

The present discussion of the wave breaking model is to my mind confusing (as far as I understand, the approach by Tulin (1996) and Huang et al (2011) is followed). In line 449 the '*rate of energy loss*' is given, which is '*of fourth order of the wave amplitude*'. I assume that the rate is given at the left-hand-side of the relation, and then it is proportional to η^2. Probably the authors imply, $e \sim \eta^2$, then it is better to say this explicitly.

They say in line 450 that D – is a '*small empirical constant*'. A few lines later they claim $D \sim gD_b|A|^4$ – not a constant (?), D is proportional to the amplitude in power four itself (?). Later, in Eq. (21) and (22) one may find D, which is constant (?) or a function of A (?) (if so, what is A?).

The given in line 453 nonlinear Schrodinger equation at the right-hand-side holds two terms, characterized by D (function or constant?), and a new constant γ appears (its role is not discussed). Besides, the threshold steepness A_s is introduced (its value is not discussed either) to push the losses to a high-steepness domain. I can only guess that the NLS equation is cited from Huang et al (2011) (?), and that the model by I. Shugan et al is modified accordingly adding terms similar to ones which appear at the RHS of the NLS equation (if I understand the approach correctly). Truly speaking I have spent lots of time trying to realize all that.

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.

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 $\sim \sqrt{2}\varepsilon N < 1$), 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 $\sim \sqrt{2}\varepsilon N \approx 3.2$, the corresponding length of perturbation is much longer than the most unstable one (in these variables the most unstable situation corresponds to BFI $\sim \sqrt{2}\varepsilon N = \sqrt{2}$), 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.

<u>Less important</u>

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?

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.

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.

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

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*.

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

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.