

## ***Interactive comment on “Benjamin–Feir instability of waves in the presence of current” by I. V. Shugan et al.***

**Anonymous Referee #3**

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In this manuscript the authors discuss the modulational instability in the presence of a non-homogeneous current. They develop a model to predict the modulations of a monochromatic wave and the maximum amplitude reached during the evolution. The authors include also a model of dissipation due to wave breaking.

From a physical point of view the results are not new: the fact that when waves encounter an opposing current, the modulational instability can be triggered is known in the literature. However, the model presented by the authors is different from most of the models in the literature (mostly based on the NLS equation). Therefore, I believe that the present results should be considered for publication.

My major objection to the manuscript is that the results of the comparison between the model proposed and the experiments are rather inconclusive. Apparently their model

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should be superior to all the others; however, for example in Fig. 2a, the model does not seem to fit well the experiments. The reason of this discrepancy is not explained in the text. Also in Fig 2b, the first two experimental points are well off the curve obtained from the model. It is quite difficult to state safely that the developed model is better than the others.

I would suggest to discuss more in deep the comparison between experiments and the model.

Why do the authors name the model as “resonant”? The modulational instability in one horizontal direction is not a resonant process but a quasi-resonance. Does the presence of a current make it exactly resonant?

Paragraph (iv page 181) is taken entirely from Ruban (2012); this is not a major problem, but I just do not understand the reason of this choice.

I have appreciated the fact that the list of references in the introduction is complete.

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Interactive comment on Nonlin. Processes Geophys. Discuss., 1, 1803, 2014.