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Nonlinear Processes in Geophysics
Discussions
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# *Interactive comment on* "On ZRP wind input term consistency in Hasselmann equation" *by* V. Zakharov et al.

**Anonymous Referee #2**

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This manuscript considers a new paradigm of the source term balance in deep water in spectral wave models. Key features are the role of non-linear four-wave interactions, a new concept of wave dissipation and a wind input closure term to arrive at self-similar solutions of the Hasselmann equations. This is interesting work deserving attention, but not in its present form. As outlined below that are too many issues that hamper publication. Therefore, I regret to advise a reject.

It seems that the present manuscript was put together much too hastily; insufficient attention has been paid to a proper description of the numerical results, there is hardly a comparison with observations, the representativeness of the chosen set of experiments is poor, and wishful thinking as expressed by a much too qualitative assessment of result.



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Also, a large part of this manuscript is not new. The basic concept of a ZRP wind source term in relation to self-similar solutions has already been presented in Zakharov et al. (2012), and Pushkarev and Zakharov (2016, Ocean Modelling, 103). The fetch-limited tests have already been presented in PZ 2016. This applies to most figures related to fetch-limited wave growth. The only new results are related to checking the consistency of the new paradigm for duration limited wave growth. This in itself is too limited for publication in NPG.

It is noted that physical basis of the new ZRP wind input is missing. It is constructed as a closure term to enable self-similar solutions. Still, it is interesting that some features of wave growth are represented, of which the typical spectral shape deserves more attention.

There are hardly any comparisons against measurements, and the ones shown already appeared in PZ 2016. It is a shortcoming that no attempt has been done to compare the typical spectral shapes of Figure 7 and 17 with field observations.

The discussion of the results is poor, especially in section 4. Many figures are just mentioned with hardly any discussion. This also holds for the flow of the body text.

The number of numerical simulations is too limited to draw firm conclusions and the results shown are not convincing. Just consider Figure 11 where only 4 symbols should provide evidence of this set of source terms, or Figure 21 with only 7 symbols which do not even coincide with theoretical results. Details of the numerical procedure to handle the implicit damping are missing. Also section 4.1 in PZ2016 does not give implementation details. This makes it practically impossible to reproduce findings.

The range of applicability for other than academic 1D-cases is not discussed at all. As mentioned in PZ2016 the next step should be to test the applicability in 2D- field cases, but nothing is said about this.

The title is not appropriate. The focus is not on wind only, but on the whole concept of

the new set of source terms.

The quality of various fits or numerical results is poor, and the assessment of the goodness of fit is much too qualitative. An example is figure 2 where the fitted line is still far away from the solid line. Other examples are those in the figures 6 and 13, where the numerical results are still far away from the theoretical result.

The figures 10 and 20 hardly provide added value to the present study.

Many legends to figures are incomplete or cryptic.

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