Answers to Referee:

The manuscript text, related to the first submission is printed in black, to the second submission - in red, to the third submission – in blue, to the fourth submission – in lavender.

It is a pity that the rebuttal gives additional clarifications which were not included in the modified body text. This is a missed chance of this revision.

Answer: The clarifications from the third submission Answers to Referee have been assimilated into the enhanced Introduction.

I read the rebuttal with interest which provides interesting additional information. Still my simple question is not answered. The wind input is evidently not derived from first principle but only made to make self-similar solutions possible. By key remark is that the wind input is not based on observations. This notion is now 'hidden' in the text and should be made more explicit. As it is still artificial, it would be of great interest when some validation against measurements is recommended.

Answer: The explicit statement on ZRP wind input term is added to:

- Introduction, page 4, lines 3-8
- Conclusion, page 22, lines 3-6 and page 23,lines 1-2

The integral characteristics validations against measurements are provided in the section **8 Comparison with the experiments**.

While it is true that the details of the wind input term are not validated, the key point here is that the measured spectral shape is reproduced quite well, whereas, the current form of 3G source terms does not accomplish this. The shape of the spectrum provides a very good surrogate for understading the source term balance.

Comment #4
The detailed description of the numerical scheme is now almost complete. The lines 15-16 on page 8 are a good starting point. It is still not clear when implicit damping is applied in the procedure. It may simply be resolved by adding something like:
1) Enew(f,theta) = Enew(f,theta)+dt*[Swind(f,theta) + Snl4(f,theta)]
2) Overwrite Enew(f,theta) to f-5 tail for f>1.1 Hz

3) Compute Snl4 +Swind over full range of spectrum 4) Etc… Such an addition may enable reproducibility

Answer:

The following pseudo-code has been added on page 11, line 32 and page 12, lines 1-4:

1. Calculate $S_{nl}(\varepsilon(f,\theta))$
2. Overwrite $\varepsilon(f,\theta)$ to f^{-5} for $f > 1.1\,Hz$
3. Update $\varepsilon(f,\theta) = \varepsilon(f,\theta) + dt \cdot S_{nl}$
4. Solve analytically $\frac{\partial \varepsilon(f,\theta)}{\partial t} = S_{wind}$ for time dt
5. Return to step 1

Comment #6
I disagree that the WAM approach for the tail is inferior to the present method. I note that both approaches force a parametric tail to the spectrum from some frequency. The similarity is that for this range the wind source terms do not play a role anymore, whereas both in the DIA and in the WRT the tail is used in the evaluation of the Snl4 term. These properties lead in my interpretation to a 2.5 G model. The differences, however, are related to the assumed physical origin.

The treatment of the tail of the spectrum is required to maintain stability in the S_{nl} integral. Otherwise, the fluxes run up against a “dam” and the energy levels become so large that they create instabilities in the integral. This allows the spectrum to maintain a classic $k^{-2.5}$ form in the equilibrium range.

We concur that both versions of the parametric tail are not in concurrence with a detailed-balance forms for a breaking source term as they are currently formulated.

Comment #17
This section is still short. The rebuttal gives sufficient additional information to better elaborate on the position of this research in the long-term quest to a sound wave prediction model. To name a few: this study is a proof of concept. Which steps are needed to improve on this result, how to solve the issue of insufficient quadruplets, how to solve the strange blobs (Fig. 20), which may be solved by 2D-

wave model computations, validate ZRP wind input against measurements, validity for low and high wind speeds.

The section **9 Conclusions** has been augmented to address Referee concerns on pages 22-24 on the following:

- the note on the study as the proof of the concept
- additional study of sufficient grid resolution which might be exhibited in oscillation of self-similar indices is required
- the spectral blobs appearance, corresponding to the waves running almost orthogonal to the fetch (well-known "smiley" effect), is presumably a numerical artifact connected with the specifics of the studied fetch limited statment. Despite it is shown that their relative contribution doesn't exceed $5%$ of the total wave energy, and they are insignificant for the research purposes, their relevance to the reality can be studied via full 2D limited fetch simulation
- although the integral parameters of the model have been verified agains the experimental observations, more verification of the spectrum details, such as angular speading, is required
- the test of the model invariance with respect to wind speed change from 5 to 10 m/sec has already been done, but further study of the effects of wider range of wind speeds variation on self-similar properties of the model is desirable in the future

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Comment #18
These comments on the 2D application are worth including in the outlook for further work

At the moment of submission of the manuscipt, the main techical obstacle to effective development of new generation of physically based HE models is insufficiently fast calulation of exact nonlinear interaction. The transition to 2D case requires radical increase of the calculations speed. We hope that further such improvements will be made in near future.

Relevant comments have been added to the section 8 Conclusions.

Comment #22
Just add an explicit reference to Resio and Long (2007), which clearly illustrates the existence of a bump in the spectrum. It would be great when the authors may provide additional references.

The corresponding references have been added in relevant places.

Comment #24
The comments in the rebuttal about the origin of the wiggles (insufficient quadruplets) should be included in the body text. It is relevant information for a reader and also for reproducibility.

The origin of the wiggles have been reformulated to more understandable language.

The number of quadruplets is connected with the grid resolution. The issue of limited quadruplets number is, in fact, the finite grid resolution issue, which exhibits itself, in particular, in the indices oscillations in the following way: the spectral peak is down-shifting in the process of the evolution, and most of the times its location is “in-between” neighboring grid point, while the self-similarity theory deals with continuous Fourier space. When the spectral peak coinsides with the grid node, its value jumps. That should be the reason of the observed indices oscillations.

We shifted from the quadruplet language explanation to grid resolution explanation (discretness) one, since it is more understandable to the reader.

Relevant comments have been added to the figures explanations as well as in the Conclusion.

Comment #27
Just give explicit reference to Resio and Long (2007), and/or others.

The corresponding references have been added in relevant places.

Comment #29
I agree that removing this figure (Fig 10). It is not very illuminating and it is not a proper way to illustrate a directional broadening. Please take care in renumbering all remaining figures.

We desided to leave the figures of frequncy-angular distribution and added the additional figure showing the portion of total energy, containing in every angle. This figure shows that the “smiley” effect, being presumably the numerical artifact, is not significant for our purposes.

Comment #31
In the rebuttal a reasoning is given related to '… limited number of quadruplets …' I do not exactly understand what is meant which this phrase. I can only speculate that the present computations were carried out with a too coarse model to evaluate the nonlinear interactions. If true, it degrades the soundness of the present results. This issue should be clarified and shared with the reader.

See the answers under ***Comment #24***

Comment #34 See previous comment

See the answers under ***Comment #24***

Comment #36 The authors are a bit cheating here in explaining the fact that Swind=0 for f>1.1Hz. This behavior conflicts with the Eqs. 41-45, where a continuous function is presented without a frequency cut-off. So, wind input is artificially set to zero as part of the numerical procedure.

The Authors don't see any cheating in here. Let us explain, why.

The wind input is not only artificially set to zero above f_d=1.1 Hz as the part of the numerical procedure – it is zero on the stage of the continuous model formulation.

The cheating, in our opinion, is in the construction of the WAM-like source terms, when the supposed-to-be physically based Snyder wind input term is superimposed with dubious spectral maximum dissipation function, producing as the result of their summation some sign-indefinite source function.

In our approach, every frequency range has to be occupied by inidividual physically based source term -- that's why the wind input stretches only up to f_d=1.1 Hz. As far as concerns discontinuity of the wind input function, it is chopped off on the stage of transition from WTT approach to the phase space confined model, corresponding to the reality (see the modified Introduction)

Now suppose the realization of the "non-cheating" case – that we desided to continue the wind input function up to the highest frequency. Anyhow, it has to be overlapped with some dissipation function at high frequencies, strong enough to suppress the wind input above f_d=1.1 Hz and stabilize the model from non-physical energy "build-up" at the highest frequency due to the "damb" effect (insufficient ability of the highest frequency bound to "leak" the KZ energy flux), which would have some unpredictable reverberations at high frequencies, including energy flux reflection from frequency domain upper bound and numerical instabilities.

As far as concerns discontinuity of the source terms at f_d=1.1 Hz, it does not consitute any problem whatsoever, since the integral equations, in the contrary to the differential ones, exhibit solutions smootheness even for discontinuous source terms.

Comment #38
The explanation offered by the authors to explain the spectral blobs at angles of +-85 degrees is in my opinion wrong. In addition, Figure 20 does not show directional broadening, unless the authors consider the blobs at either end of the spectrum as directional broadening. In case the authors stick to a physical explanation, then proper references should be given. In my opinion the blobs are spurious artefacts arising from the numerical procedure to evaluate fetch-limited wave growth using a simple 1d-wave model. For angles close to +-90 degrees the cos(theta) (see 46) is close to zero leading a strong growth of energy. This is similar to an infinite fetch perpendicular to the wind direction. This feature is known as the smiley effect for decades. Moreover, it manifests itself mainly close to shore and vanishes in 2D- wave model computations.

We agree that this is the numerical artifact. See the answer to the **Comment 17 and 29.** Relevant explanation is also included at the section in the relevant figure comment and section **8 Conclusions**.