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Interactive comment on “Asymptotes of the nonlinear transfer and wave spectrum in the frame of the kinetic equation solution” *by* Vladislav G. Polnikov et al.

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The letter of reply on the comments by anonymous reviewer #1 on manuscript “Asymptotes of the nonlinear transfer and wave spectrum in the frame of the kinetic equation solution” by Vladislav G. Polnikov, Fangli Qiao and Yong Teng

This review can be sheared in two parts.

In the first part (the first half of review), the reviewer has made remarks about several details of the paper. But nearly all of them do not hold water. Indeed, 1) In his remark about the non-locality, the reviewer did not notice that we have used the spectra with

the same power-falling tail but different peak parameters. Herewith, the power-falling law of the NL-transfer tail is different. This is not a trivial result. This is not a simple influence of the initial condition. This is the non-locality firstly described here. It seems that the reviewer has not noticed this idea.

2) The remark about “large time asymptote for peak frequency “ was commented in the text of our paper. The small difference of powers in Eqs. (29) and (34) is acceptable for numerical calculations, as it is well covered by the statistical scattering for these powers.

3) The remark about Table 2 is simply the draw-away words. We did not state the non-locality from Table 2 results. It was done rather earlier in the text.

4) The only remark about the missing reference (Badilin and Zakharov, 2017) in the list of references is fully accepted. Though, this is the negligible misprint with could be easily improved.

Thus, we may conclude that this part of remarks is not principal for a judging the text merits.

The second part of remarks is more principal one. Here the reviewer has tried to argue the authors approach for this study.

5) The reviewer did state “From the theoretical viewpoint the Hasselmann equation without forcing and dissipation is well known to conserve wave action, but not energy, which slowly leaks to small scales (Badulin et al 2005, referenced in the text)”. Yes, we know this paper, which is one among others by the Zkahrov’s group, where this result was stated. In our text we fixed this results as the “paradox fact”, which takes place in the conservative system (the conservatively of the potential-wave system was proved by Zakharov, 1968). From paper by Zakharov (2017) we know that this leakage of energy (in the solution of KE) is due to low convergence of the kinetic integral for slow-falling spectra appearing in this solution. This fact is also mentioned in our paper

as the defect of the present approximation for KE. To overcome this defect of the KE, we accepted two versions of the KE-solutions: with preserved wave action, and with preserved wave energy.

6) The reviewer said “I do not think this is a good idea”. In opposite: this is a very good idea. It permits us to check the point of applicability of the Kolmogorov-turbulence treating the results of the KE solutions. This idea has resulted in the unexpected and amazing result. Indeed, in the case of preservation of total wave energy, the long-term asymptote of the KE-solution has the same falling law as in the case of preserving wave action. In this regard, the reviewer makes a comment: ”Actually, by imposing energy conservation, the authors effectively pump some energy into the system at each time step, therefore replacing the original problem with a different one. Apparently, this "forcing" is too weak to form the "-11/3" spectral slope typical of the inverse cascade”. By these words, in fact, the reviewer has agreed that in the solution of the pure KE, the spectrum with the tail-faling power "-11/3" is not established; despite of the fact that the downward wave-action flux takes place. Thus, the Kolmogorov treating has no place. That is what we want! The reviewer does support our main result.

7) Herewith, the reviewer has finalized his comments by the listing of our conclusions, stating that they are “(a) not new (b) incorrect (c) senseless”. These reviewer’s statements are not furnished with proof, because the listing of our conclusions, following after these statements, is not a proof. In opposite, these conclusions are new, correct, and have a very important physical sense.

As there are no other principal remarks, we can state that, taking into account the fact of supporting our results by the reviewer (see the end of remark 6), we have a positive result of this discussion.

Thus, the positive final of our discussion with the reviewer permits us to end our reply and hope on the publishing our paper (despite of the negative reviewer’s conclusion, which we consider as the homage to the Zakharov’s group results).

Some details of our replies to the certain reviewer’s remarks are pasted into the text of Review #1, given as the supplement to this our reply.

On behalf of the co- authors, Dr. Vladislav Polnikov. 18.09.2018

Please also note the supplement to this comment:
https://www.nonlin-processes-geophys-discuss.net/npg-2018-35/npg-2018-35-AC3-supplement.pdf

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