

Referee's report on the revised manuscript "Ocean swell within the kinetic equation for water waves" by S. Badulin and V. E. Zakharov

The authors consider long-term evolution of swell within the Hasselmann kinetic equation. The major challenge faced by the authors was that the Hasselmann equation is derived under the assumption of spatial homogeneity, although the effects of spatial divergence are known to play an important role for the swell evolution. At the same time, wave-wave interactions, which are modelled by the equation, are of primary importance for the swell evolution as well, and the authors had to find a rather difficult balance between the features of the evolution they could and could not consider in the study. In general, I think that in the revised version of the manuscript this balance was found successfully. The paper contains interesting results and can be published. A few minor points are listed below.

1. Page 6/5: *"This implies that the only one (or very few) of an infinite series of eigenfunctions... contributes to wave spectra evolution... This treatment of the heavily nonlinear boundary problem in terms of a composition of eigenfunctions is possible in this case as demonstrated by Zakharov and Pushkarev (1999)"*. I think a slightly more detailed explanation is needed here. I guess the authors mean the eigenfunctions of the diffusion problem that can be derived from the Hasselmann equation, but it is not clear which boundary problem they are referring to, why it is "heavily nonlinear", and Zakharov & Pushkarev (1999), while discussing the diffusion approximation, make no mention of eigenfunctions.

2. Page 6/10: *"Their appearance within the kinetic equation approach is generally associated with wind generation..."*. This expression (there are quite a few such cases in the text) is confusing; apparently, the authors mean "generation by wind". Also, I'm not sure about the relevance of both Bottema & van Vledder references here. The title of Pushkarev et al. (2003) reference is not quite correct.

3. Page 6/20: *"This invariance does not suppose a point-by-point coincidence of properly normalized spectral shapes"*. If it does not require point-by-point similarity, then in what sense we can speak about the invariance? Below, the authors go on to discuss only integral parameters of spectra, so may be not clear to the reader why the invariance was mentioned.

4. Page 9/25: what is the initial steepness? For the final values $H_s = 2.8m$, $T_p = 11.4s$ I get the significant wave steepness $\frac{1}{2}H_s k_p = 0.043$, which corresponds to $\mu = 0.022$ in full agreement with the authors (since μ in Eq.(15) is half the significant steepness). But for the initial $H_s = 4.8m$ and $T_p = 3s$ I cannot get anything like $\mu = 0.15$ as stated by the authors (which is, by the way, quite high already). Instead, I obtain a value beyond all physically reasonable limits. It would not be an issue for long-term simulations, since the steepness in simulations quickly drops anyway, and the exact form of the initial conditions is irrelevant. However, since the authors put a lot of emphasis

on the near-field attenuation in the manuscript, one may think that the fast initial attenuation may be due to unphysical initial conditions.

5. In figure 2, the labelling of panels is missing, and the notation of wave momentum does not correspond to the caption.

6. Page 10/25: *“This relaxation generally occurs at essentially shorter scales than ones of wind pumping and wave dissipation”*. Apparently, the authors mean “shorter time scales” here.