Interactive comment on "Ocean swell within the kinetic equation for water waves" by Sergei Badulin and Vladimir Zakharov

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Answers to referee #1

Authors are grateful to the referee for attentive reading of the manuscript and valuable comments and suggestions. The authors took all these comments into account when preparing the revised version. Many changes are made in the text, almost all the figures have been re-drawn, additional numerical runs have been carried out as recommended by the referee for longer duration and with higher directional resolution. Ten new references appeared in the paper bibliography. Finally, the paper becomes three pages longer. A native speaker who checked the text has made very few suggestions in English style and grammar.

Our answers follow the reviewer's report (given in bold).

Major remarks:

1. **A major assumption, of which the consequences are not yet clear, is that swell evolution is treated as duration-limited evolution in an infinitely homogeneous ocean, which effectively reduces the action balance equation to dN/dt=Snl4. This assumption neglects dispersion and spatial divergence of wave energy. This mismatch makes it difficult to compare the results of this study with observations. The consequences of this assumption in relation to the true evolution of swells on the ocean surface need to be clarified**

   The authors realizes severe limitations of the duration-limited setup in the problem of ocean swell. Nevertheless, even this extremely restrictive model shows quite rich physics: self-similarity of swell evolution, universality of spectral shaping, bi-modality of directional spreading. Limitations of the duration-limited setup are now emphasized in many parts of the text (e.g. 3/17-25 Page/Line). Everywhere in the text we stress robustness of the effects of wave-wave interactions and present prospective plans for more realistic models of swell evolution in time and space where wave dispersion and spatial divergence play important roles. We also note concistency of our results with previous numerical and experimental findings (e.g. Banner & Young, 1994; Ewans *et al.*, 2004);

2. **The second point concern the different phases of swell decay in the form of near- field and far-field. The authors argue that there is an initial strong swell decay. The relatively strong decay of the near-field is still hypothetical, as no comparison with field observations could be made. Whether such measurements do not exist, or whether the authors have**

not searched for such measurements, is unclear. Looking at the results in the Figures 8 and 10 I conclude that the comparison against field data is made on the basis of the sw330 case. This seems a bad choice for 2 reasons. Firstly, sw330 is not comparable to field data in terms of directional spreadings. Secondly, the results in Figure 8 show for the initial phases a strong flux for the higher frequencies, causing this decay. For this case, the spectrum is initially very wide, and the nonlinear interaction try to narrow the spectrum towards an equilibrium situation, meanwhile pumping a lot of energy to the spectral tail.

We agree that the near-field behavior of the ocean swell is extremely difficult to explore experimentally. This is why we consider our results on the role of wave-wave interactions in the near field as important. Discussion of directional spreading of swell is now extended. Illustrations are given both for narrow and wide directional distributions (figs.6,7). Swell attenuation in fig.10 is presented for all the runs of Table 1: angular spreading has no essential effect on rate of wave energy leakage. At the same time, initially wide spectra (e.g. run sw330) demonstrate quite strong transformation of angular spreading (fig.7) and essential deviations from the stationary KZ reference in terms of the second Kolmogorov constant C_m (fig.9f);

3. **Thirdly, the role of a weak wind in even strengthening swells is much too hypothetical. Within certain assumptions this may be the result of a theoretical exercise, but I doubt whether unstated assumptions hold. In my feeling, a weak wind will lead to additional energy way beyond the swell peak of the spectrum, effectively changing the shape of the spectrum, causing a mismatch of self-similar spectra. A simple numerical test should be performed to shed more light on this issue. In the present manuscript a directional spreading of $30°$ is considered to be very narrow. This seems a**

proper choice, and the authors may refer to observations where directional spreadings in the order of $10° − 15°$ are described (Olagnon et al., 2013; Ewans 1998).

We do not consider the mechanism of wind wave absorption by swell as hypothetical. This effect has been discussed for experimental data (e.g. Pettersson, 2004; Young, 2006) and in numerical simulations of the Hasselmann equation (Badulin *et al.*, 2008). In the updated paper we analyze this physical effect as a competition of two spectral fluxes: direct cascade produced by swell and inverse cascade of wind-driven waves. Wind waves in this scheme are attempting to grow but are just feeding the swell because of relatively fast relaxation to the inherent swell state (see eq.37 for the relaxation rate). The concise estimate (eq.40) looks quite suggestive for possible experimental verification. The authors are grateful to the reviewer for addressing to works on swell evolution (e.g. Ewans *et al.*, 2004) that gave important experimental illustrations of our results.

Minor remarks (Page number/Line number):

1. **2/2 briefly explain the concept of e-folding**
   Explained in lines 2/2: 'Their e-folding scale (distance in which an exponentially decaying wave height decreases by a factor of e) about 4000 km is consistent with some today results. . .';

2. **2/5 elaborate on the algebraic law, for which process is such a law made.**
   Now 2/6. We added comments on the model deficiency. The mentioned model relies upon a number of questionable hypothesis and empirical observations and cannot be incorporated straightforwardly into existent wave models in a mathematically consistent way;

3. **2/10 I disagree with the generality of the statement that swell is considered a superposition of sinusoidal components without interaction. Maybe in the time of Barber and Ursell (1948), and Snodgrass (1966) and before the time of 3G-wave models. Although I agree that the DIA in the WAM model is not a nice example due to its limitations.**

We made the statement less radical (2/14): 'at most' instead of 'generally'. Unfortunately, the simplistic treatment of the swell is dominating today in time of 3G-wave models. We may refer to the feedback of the associate editor of Journal of Geophysical Research (the very first version of our paper has been rejected from JGR as it is mentioned in the submission form of NPG). Prof. Bruno Castelle wrote:

'Swell is rather unidirectional and monochromatic once it travels outside the storm area, the resonant interactions for such conditions should therefore be negligible, in contrast with your numerical experiments using a 'rectangular' spectral distribution'.

One of the referees of the JGR continues:

'As these waves propagate away from the storm generation site, frequency dispersion means that they separate out into almost monochromatic wave trains of the same frequency. These single frequency waves then propagate across oceanic basins and gradually decay.'

Thus, the hypotheses and the very first physical models of the ocean swell of brilliant papers by Barber and Ursell (1948), Snodgrass et al. (1966) are still alive without critical revision and without attentive reading of important parts of these works (e.g. sect.8 of Snodgrass et al., 1966);

4. **2/14 Briefly explain concept of e-folding**

It is explained above (see 2/2);

5. **2/15 You may reference to Kantha (2006) here concerning theories about swell decay.**
Thank you, it is just to the point (see ref. in 2/5);

6. **2/21 Which other motions are meant here?**
The issue is detailed, a reference is added (2/25);

7. **2/33 Add assumption of deep water and also note corresponding period range of 10 s–16 s**
Thank you, done (3/2);

8. **3/9 A useful reference here is Delpey et al., 2009**
Thank you for the useful link. It is cited now (3/16);

9. **3/10 Note that wave dispersion and spatial divergence are considered important in ocean scale swell propagation, although for distances over 10.000 km convergence kicks in.**
The authors agree. It is noted in the revised text (e.g. Introduction and Discussion);

10. **3/13 The swell heightening by a weak background wind is rather speculative, see comments in appropriate section. I would not yet consider this a significant problem from a practical point of view. From a theoretical point of view it is interesting to figure out exactly what is happening.**
Effects of the swell 'eating' wind-driven waves are described in Young (2006); Kahma & Pettersson (1994) and reproduced numerically in Badulin *et al.* (2008). In this paper we just propose a tentative estimate of conditions when this effect can play. The discussion of this effect is extended, see sect.4.2 ;

11. **4/3 The scaling law (2) only works when spectra are self-similar, which may not be the case in nature.**
   It is not correct. The homogeneity property (2) is valid for any function $N(\mathbf{k})$. It is purely mathematical fact that can be checked easily by simple change of variables in the collision integral S_{nl};

12. **5/7 I would rather drop the very before preliminary. Otherwise, this result is not worth publishing yet.**
   You are right, thank you. Fixed in 5/9-10;

13. **8/1 The model setup should be specified in more detail. Just referencing to Badulin et al. 200X is insufficient. After some checking it appear that a 1-point model is used to mimic duration limited wave growth, see e.g. Eq.6 in Badulin et al. (2005). This is an important detail, especially since it violates the statement on page/line 3/10.**
   Description of the model setup is extended (see sect.3.1). We see no contradiction with the statement of 3/10 if we treat 1-point (in the words of the reviewer) and duration-limited setups as synonyms;

14. **8/8 $10°$ resolution may be adequate, although no reasoning is shown to back this claim, for the present application where $30°$ is the smallest directional spreading. In am not convinced whether this is sufficient for ocean swells in nature, where directional spreading in the range of $10° - 15°$ are common. For such situations a directional resolution of $5°$ is usually recommended.**
   Calculations with $5°$ resolution have been carried out for 'the most inconvenient' runs sw030 and sw330 for the duration $2 \cdot 10^6$s. No difference in evolution of integral parameters (energy, momentum, spectral peak period) is found while quantitative difference in angular distributions is visible for frequencies higher than the peak one (fig.7e-h). Comments and new figures are given in the paper

version. Robustness of the two-lobe angular distribution is stressed in sect.3.4. The necessity of higher directional resolution is stressed in final lines of the paper (18/25);

15. **8/10 The equation has some problems. The square 2 is at the wrong location. Further, the variables on each side of the equal sign are inconsistent. I suggest to use $N(\mathbf{k}, \theta)$ in the left-hand side. The frequencies ω_l, ω_h are not specified.**
   Thank you. The typo is corrected. The expression in terms of θ and ω for the spatial spectrum looks more transparent (the issue of $N(\mathbf{k}) = \text{const}$). Comments to the eclectic presentation are given to explain our preferences (8/19);

16. **8/17 Explain concept of hyper-dissipation, just the key notion is sufficient.**
   We added the comment in sect.3.1 (8/26 and below). In earlier versions of the code (Pushkarev *et al.*, 2003) the hyper-viscosity option has been used to guarantee stability of calculations at high frequencies. Later on it has been realized that calculations can be stable in absence of dissipation (free boundary conditions). The sufficiently strong dissipation does not essentially affect numerical solutions: dissipation is stronger – spectral magnitudes are lower, and the overall effect of the dissipation reaches a sort of saturation. The dissipation effect just absorbs a spectral cascade directed to small (infinitely small) wave scales. Free boundary conditions work in a similar way;

17. **8/19 Why mention here the number of 30 runs, whereas the table 1 only contains 5 entries? What happened with the other 25 runs?**
   Initial conditions are now described for all the series after 9/3. We focused on runs of Table 1 that cover the full set of angles (effect of anisotropy is our priority) and have no troubles with possible instability or too slow evolution;

18. **9/7 If 11 days is too short, why not extend the simulation longer? On the other hand, the Earth's oceans may be too small to see this effect in nature.**

This poses a conflict, in the applicability of these results. There is only a tendency to approach self-similar solutions.

Calculations for our main series (Table 1) are extended to $2 \cdot 10^6$ s to better specify tendency of wave parameters (height, period) and spectral shapes to a self-similar behavior and to specify 'pure effect' of nonlinear transfer due to four-wave interactions. It appears again 'too short'. Anyway, the tendency to self-similarity is better than tendency to nowhere. 'The effect in nature' requires an advanced setup with wave dispersion and spatial divergence/convergence taken into account as the reviewer himself stressed;

19. **10/10 Which definition of sigma is used: the linear or the circular definition. Note that the latter is commonly used in wave model to quantify the directional spreading**
    Linear definition (in degrees) of σ and θ is used everywhere in the text and in figures. Hope, it makes no problem for the paper potential readers;

20. **10/13 Take a look at Ewans (1998) and Olagnon et al. (2013) for realistic estimates of swell widths, these are close to your definition of directional narrowness of** $\Theta = 30°$**.**
    Thank you for this reminder. We had the authentic report of Ewans *et al.* (2004) and now use it in the updated text. This work give extremely wide range of estimates of directional spreading. We knew about this report when preparing the first version of the paper but it seemed too radical in following linear model of swell propagation. Now this and other papers (e.g. Ewans, 2001) are cited in the context of angular spreading of swell (sects.3.3,3.4);

21. **11/15 Equation number (31) is missing here. Renumber all follow-up equations**
    There are no references to this equation in the text below. We leave the equation unnumbered;

C9

22. **11/18 There are also negative fluxes!**
    You are right. We added 'negative' and 'positive' in the previous paragraph when discussing the hybrid nature of swell solutions (13/9, 13/10);

23. **11/26 Why not provide the other estimates for the reader to judge whether the results of this study are consistent?**
    The values are provided (14/1-6), a reference (Deike *et al.*, 2014) to an experimental estimate of C_p is added;

24. **12/14 (Likely, 14/24) I am still surprised by this statement that such attenuation has never been seen in nature. Is it the result of your model setup of using only a 1-point model and only duration limited wave growth?**
    The effect of attenuation of swell has never been discussed as one observed in nature. Other 'visible' mechanisms of swell decay like spatial dispersion or dissipation are in the focus of swell studies. Moreover, the fact itself of non-conservation of wave energy and momentum is not accepted by majority of researchers (Janssen, 2004, p.182, comments to eq.4.20 or p.137, sect. *Conservation laws* in Komen *et al.* (1995)),

25. **12/25 I wonder whether the case shown in Figure 10 is properly chosen. Sw330 can hardly be seen as representative for ocean swell in nature. Why not use the case sw030 here to illustrate the point. Now, I am afraid that completely different types of spectra are inter-compared, leading to false interpretation.**
    Figure 10 is re-drawn. Upper panel shows all runs of the series with no essential quantitative difference. Thus, our choice representative. See also comments to page 29 below;

26. **13/20 Although the algebra may be trivial, mention the starting point of this exercise**
    It is given in more details in sect.4.2 now;

27. **13/32 This may appear an interesting result, but it is only valid within certain assumptions of self-similar spectra. I doubt that this condition holds in case of some wind growth. I expect that some local enhancement of spectral density will appear, which will not cause any effect on the low-frequency part. Having said that, only detailed numerical experiments can shed light on this issue. So, I welcome this hypothesis, but for now it do not (yet) believe in this consequence.**
The effect is seen fairly well in previous numerical experiments (Badulin *et al.*, 2008). We also have new results on this effect and hope to publish them soon;

28. **14/1 I disagree with the choice of the word 'clearly', see my previous comment. It is only an hypothesis within some assumptions.**
Thank you. We deleted it (17/5);

29. **14/11 Also quantitatively?**
Thank you. Now *'quantitatively and even qualitatively'* (17/15);

30. **14/15 I disagree that this can be used as a benchmark for real ocean swells in view of the limited size of earth's oceans. See comment 9/7.**
Thank you. Now *'features KZ solutions can be used as a reference'*... (17/29);

31. **14/25 I disagree that today's models do not account for this effect. In case of the DIA, the most common method for S_{nl4}, this may be crude or wrong, but it does something.**
Thank you. Now we say: *'This mechanism is beyond the today models of sea swell...'* (17/31). The problem can be addressed to the DIA, first, to uncover whether the models are accounting for this effect;

32. **14/25 I am not convinced that this 'near field' effect has never been observed or noted. It is now too easy stated that this is a problem. Still, it is an interesting notion for further investigations**

C11

We did not say 'never been observed and noted'. The today studies of swell from space do avoid discussion the near field effects and, thus, skip an essential physics of sea wave dynamics. The text is modified (bottom of p.17, top p.18);

33. **15/8 This is an interesting statement, but in view of comment 8/1 both dispersion and spatial divergence are important. Only a true 2-d spherical model of swell propagation over the oceans can shed light on this issue. It is disappointing that this notion is not mentioned by the authors.**
Ok, we turn our cards over. Perspectives of the study are given in more details now (18/19 and below);

34. **15/12 No clear recommendations are given for further studies. See also previous point, which is probably one of the most important steps forward.**
Thank you. Corrected, see previous note;

35. **16/11 This reference cannot be found on the workshop website, only the abstract resides there.**
It is a pity. Reference to ResearchGate source of the paper is added. Similarly, the conference paper of Lavrenov *et al.* (2002) is put into supplement of the ResearchGate web-page of Badulin *et al.* (2002) and the corresponding reference is given. Unfortunately, Prof. Igor Lavrenov deceased in 2009 and its paper resides now at this web-page;

36. **16/32 The journal of Chen et al., 2002 is wrong. Please correct. Journal of Atmospheric and Oceanic Technology**
Thank you. Fixed;

37. **19 Table 1 only list 5 of the 30 cases. What are the remaining 25 cases?**
Parameters of simulations are described in more details in sect.3.1;

38. **20 The initial shape at $t = 0$ does not match with Eq. 23.**
We see no problem. Eq.23 (eq.25 now) gives spectral density of wave action

C12

$N(\mathbf{k})$ while Fig.1 shows evolution of energy frequency spectrum

$$E(\omega) = \int_{-\pi}^{\pi} \frac{2\omega^4 N(\mathbf{k}(\omega,\theta))}{g^2} d\theta$$

(see for refs. Badulin *et al.*, 2005, unnumbered equations after eq.30);

39. **20 The unit along the vertical axis is incomplete m^2/(rad/s)**
Thank you. Corrected for two times longer evolution;

40. **22 How do you explain the significant mismatch in behavior for case sw330?**
Calculations are continued up to $2 \cdot 10^6$s, Figs.2,3 are redrawn. The explanation can be found in sect.3.2-3.5. The case is 'too isotropic' and non-self-similar background corrupts a bit the simple asymptotics;

41. **24 It is known that S_{nl4} is weaker in directions than in frequencies to show self-similar behavior. This was for instance noted in the directional response behavior of the spectrum after a change in wind direction. I do not think the 1984 and 1985 are proper examples. See also remark 10/30.**
We leave 1985 and added WASP from Ewans *et al.* (2004). Weakness of S_{nl4} in direction is misleading. The relaxation rate depends on magnitude of excursion. This is what we see in fig.6 for sw330. See also 10/30 – speculations on different scales of evolution due to wave-wave interactions;

42. **25 The scale of the vertical axis is inconsistent with the one in Figure 5.**
You are right. In fig.5 normalized (by value at $\theta = 0$) values for different runs are shown while in fig.6 we give absolute values at different times for the same run in order to demonstrate the phenomenon of relaxation to a universal (our hypothesis) angular distribution;

C13

43. **27 I am surprised that case sw170 is used is as an example. This deviates from other choices. Please comment on or argue this choice. Also, note the small instability for $t = 1$ hour. Also note that also the negative fluxes tend to diminish. Also, argue choice of sw170 for this example. What happens for other choices? In general, the behavior of sw030 or sw050 is much more interesting in relation to real ocean swells. Although, it is of interest that even for initial broad spectra, S_{nl4} tends to force a uniform shape.**
This figure is re-drawn. Results are shown for two extreme cases sw030 and sw330;

Sorry, we do not see any instability for red curves $t = 1$hr.

We answered the question on negative fluxes (hybrid nature of swell evolution) in the note 11/18. Negative fluxes follow the same tendency as positive fluxes when solutions are tending to self-similar behavior. We see no reason to emphasize this point here.

Thanks for your last phrase of this note. You stressed the very important finding of our work: S_{nl4} provides a uniform (we say universal) shapes of swell irrespectively to initial spectral distribution;

44. **28 Same comment in relation to choice of SW170**
Three cases are shown now in fig.9. The only outlier is sw330 for the second Kolmogorov constant C_m;

45. **29 I am surprised that for this figure sw330 is taken to compare with observations. Why not sw030 or sw050 as that is much closer to field data**
Re-drawn. All cases are shown. Time and coordinate axes are logarithmic now to see the 'near-field' better;

46. **29 There is an inconsistency between figure legend and body text concerning reference to Badulin.**

C14

Thank you. The figure is re-drawn. Time and fetch axes are log-spaced now in order to demonstrate strong drop of wave heights in near zone (less than 1000 km). Curves are given for all runs of the series and show quite close behavior for different initial distributions.

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