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Nonlinear Processes in Geophysics
Discussions

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

Anonymous Referee #1

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Ocean Swell within the kinetic equation for water waves Zakharov and Badulin

The authors present interesting notions about swell evolution for long durations and it seems worth publishing. Based on my considerations I recommend a major review.

There are 3 important notions that should be discussed or modified in more detail. One concerns the model setup used for the numerical simulations, the second concerns the discussion of the initial swell decay for the near field, and the third concerns the role of weak winds.

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 ef-

fectively 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 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.

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).

The English style of writing is good, although at some places small grammatical errors are made. One last round of a native speaker is recommended when this manuscript may reach its final stage. I am very satisfied with the quality and clarity of the figures.

Some detailed remarks, also recommendations for improved readability Page number/Line number

2/2 briefly explain the concept of c-folding 2/5 elaborate on the algebraic law, for which process is such a law made 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. 2/14 Briefly explain concept of e-folding 2/15 You may reference to Kantha (2006) here concerning theories about swell decay. 2/21 Which other motions are meant here? 2/33 Add assumption of deep water and also note corresponding period range of 10 s – 16 s 3/9 A useful reference here is Delpey et al., 2009 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. 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. 4/3 The scaling law (2) only works when spectra are self-similar, which may not be the case in nature. 5/7 I would rather drop the very before preliminary. Otherwise, this result is not worth publishing yet. 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. 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. 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(k,ïĄś) in the left-hand side. The frequencies ïĄů_l and ïĄů_h are not specified. 8/17 Explain concept of hyper-dissipation, just the key notion is sufficient. 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. 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. 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 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 ïĄŚ=30°. 11/15 Equation number (31) is missing here. Renumber all follow-up equations 11/18 There are also negative fluxes! 11/26 Why not provide the other estimates for the reader to judge whether the results of this study are consistent? 12/14 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? 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. 13/20 Although the algebra may be trivial, mention the starting point of this exercise 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. 14/1 I disagree with the choice of the word 'clearly', see my previous comment. It is only an hypothesis within some assumptions. 14/11 Also quantitatively? 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. 14/25 I disagree that today's models do not account for this effect. In case of the DIA, the most common method for Snl4, this may be crude or wrong, but it does something. 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 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. 15/12 No clear recommendations are given for further studies. See also previous point, which is probably one of the most important steps forward. 16/11 This reference cannot be found on the workshop website, only the abstract resides there. 16/32 The journal of Chen et al., 2002 is wrong. Please correct. 19 Table 1 only list 5 of the 30 cases. What are the remaining 25 cases? 20 The initial shape at t=0 does not match with Eq. 23. 20 The unit along the vertical axis is incomplete > mˆ2/(rad/s) 22 How do you explain the significant mismatch in behavior for case sw330? 24 It is known that Snl4 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. 25 The scale of the vertical axis is inconsistent with the one in Figure 5. 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, Snl4 tends to force a uniform shape.

28 Same comment in relation to choice of SW170 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 29 There is an inconsistency between figure legend and body text concerning reference to Badulin.

References: Barber and Ursell, 1948, The generation and propagation of ocean waves and swell. Proc. Roy Soc., A824. Kantha, 2006, A note on the decay rate of swell, Ocean Modelling, 11. Olganon, M., K. Ewans, G. Forristall, M. Prevosto, 2013, West Africa Swell Spectral Shapes. OMAE2013-11228. Delpey, Ardhuin, Collard, Chapron, 2010, Space-time structure of long ocean swell fields. JGR,115. Ewans, 1998, Observations of the directional distribution of fetch-limited waves. J. Phys. Oceanogr. 28.

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