Ocean Sci. Discuss.,
https://doi.org/10.5194/os-2018-90-RC1, 2018
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Interactive comment on "Wave boundary layer model in SWAN revisited" *by* Jianting Du et al.

Anonymous Referee #1

Received and published: 5 October 2018

The paper "Wave boundary layer model in SWAN revisited" touches an interesting topic, which deserves further attention. However the difficulty of simultaneously improving the wind input and white capping dissipation source functions should be stressed right from the beginning since the result comes from the difference of these two terms and it is always a weak point in an analysis to deal with difference quantities. Moreover this difference, based on the parameterized physics, goes together with the numerical diffusivity associated to the discretized equations. Therefore my suggestion would be to discuss right from the start the difficulty in adjusting a difference of terms which is also conditioned by the numerical diffusivity.

Regarding the approach mention is made of fetch limited and depth limited studies. Since the adjustment is clearly related to wave age I think some more analysis about the duration limit conditions or the state of wave development would be in order to enhance the value of the paper.

When mentioning (page 2, paragraph 30) the "parameters" for tuning, mention should be made of which parameters. In here and in several other instances in the manuscript it would be important to link the parameters and the adjustment to their physical meaning so as to make the argumentation behind more solid and suitable for extrapolation to other cases.

In section 2.1, in paragraph 25, mention is made of how to generalize the phase velocity for the case of misalignment between wind and wave direction. This is an important point since the directional dispersion plays an important role. Some more analysis about the effect of directionality and how this would apply to for instance slant waves would be beneficial.

When presenting the equations for the wind input source function it is mentioned that the model tends to underestimate wave growth at lower frequencies and then the wave age tuning parameter form WAM is proposed. Some discussion showing how this element from WAM can be transferred directly to SWAN would also be in order.

When introducing the mean frequency and wave number following Bidlot, to put more emphasis on the high frequencies, this should be discussed together with the over/under dissipation at these high frequencies.

When replacing (page 5) fp in equation 17 by 0.86 <f> this should be discussed showing how it applies to a variety of cases where there may be more than one fp (e.g. by modal waves) or where fp is not a robust estimate.

In section 2.3 mention is made of a threshold so that the WBLM solves the energy within that interval in the wave spectrum. Some discussion about the sensitivity to that threshold and the implications that it has for very long or very short waves should be included. Particularly together with the selection of the numerical algorithm which also has implications for the numerical diffusivity.

In the case study for the North Sea some more discussion about the fetch, duration and depth limitation for these two storms would be of interest.

When presenting in section 3.3 the NCEP Climate Forecast System version 2 for the wind forcing, some discussion of why these wind fields were selected and the uncertainty introduced by the wind selection should also be discussed, particularly related to the horizontal discretization of the atmospheric model and how that relates to the higher frequency components in the atmosphere and in the ocean.

When presenting the idealized fetch-limited study (section 4.1) a suggestion is made to use U'/10. Another possibility would be to scale with a value different from 10 meters/second, related to a length scale divided over a time scale. That would add a bit more of generality.

In this same section when increasing the number of parameters some discussion about the gain due to the higher number of fit parameters and the difficulty in application should be included.

Throughout the paper, particularly near the end, the advances obtained from the application of the WBLM should be strengthened, analysing the situation where the physics suggest there will be an improvement due to the explicit consideration of the wave boundary layer. This could serve to explain why the model underestimates high waves. This could also be related to the importance that wave-wave interactions play in such high waves, conditioned not only by the input and dissipation source terms. In this same line the underestimation of Tp could be explained from the physics, particularly stressing the wave boundary layer model dependence on frequency.

Finally in the discussion and conclusion section the wave boundary layer model advances should be discussed more in terms of the physics, linking the various descriptive paragraphs that appear now. That would strengthen the application and extrapolation of the proposed model.

A final small remark is that the paragraph (last paragraph, in section 5) explaining how

the Janssen wind-input source function was wrongly implemented in SWAN should be related to the rest of the paper, explaining how such an improved implementation leads to increase the robustness of the presented results if that is indeed the case.

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