In their manuscript „Impact of floe size distribution on seasonal fragmentation and melt of Arctic sea ice", Adam Bateson and colleagues present results of a numerical sensitivity study concentrating on the role of the floe size distribution (FSD) in shaping the seasonal cycle of the sea ice cover in the Arctic. The study is based on a numerical sea ice model coupled with a mixed layer ocean model. Several processes influencing/influenced by the FSD are investigated, including the lateral melting of sea ice and ice fragmentation induced by waves. The FSDs considered are truncated power laws described by three parameters: the minimum and maximum floe size, and the exponent.

The manuscript is devoted to a subject which is very important for the performance of numerical sea ice models on both short (synoptic) and long (seasonal, climate) time scales. As the authors point out in the introduction, recent climate change and the associated negative trends in sea ice extent, thickness and strength have produced conditions in which interactions with waves are becoming more and more important over larger and larger areas. Fragmentation of sea ice by waves in turn influences ice-ocean-atmosphere heat flux and sea ice melting/freezing rates. At present, our limited understanding of many aspects of these coupled processes is a serious limitation for development of reliable parameterizations suitable for numerical sea ice models. In my opinion, the proposed manuscript is an important contribution to the subject, even though many solutions used in the model and assumptions underlying them are oversimplified (or maybe even wrong). As the authors correctly remark, these simplifying assumptions are to a large degree a result of the lack of observational data and/or theoretical understanding available. In this respect, the most important contribution of the manuscript is that it develops a framework in which future developments can be integrated, as new data and insights become available. In other words, in spite of some clear limitations of the solutions presented, I find this contribution very valuable, as it paves the way for further development.

Several aspects of the results are very interesting, for example the findings related to the (partial) compensation of the increased lateral-melting rates by decreased bottom-melting rates in simulations with power-law FSD compared to simulations with constant floe size; or the important role of the wave attenuation rates in shaping the seasonal cycle of sea ice. I think that those results are worth publishing in "The Cryosphere". My comments to the manuscript are listed below. My recommendation to the Editors is "major revision".

General comments:

1. The text of the manuscript is written in an untidy manner, it contains a lot of (mostly small) mistakes and should be carefully checked before publication. Moreover, the text is full of technical slang, colloquialisms, informal expressions which (in most cases) are comprehensible for a reader familiar with numerical modelling of ocean/sea ice, but should be avoided in a paper. It makes reading of the text tiresome.
2. I find the description in section 2.2 quite chaotic. The impression is that the choice of topics that are described there is pretty random, even though the first sentence announces that this section contains only "elements pertinent to our study". For example, is the CFL criterion relevant for the results/discussion presented further? In turn, some more details from the papers of Williams and colleagues regarding computation of wave-induced breaking etc. would be very useful, even though they can be found in those papers.
   But the most important comment to section 2.2 is related to the description of the wave forcing. It is very imprecise. Also, I think the Authors should better justify the methodology they use. As written in section 3, the only wave information from ERA reanalysis used as input to the sea ice model are H_s and T_p (I think this information should be provided in section 2.2, so that the text there can be better understood). Why don't the Authors use

wave directions from ERA? Or even the whole energy spectra?
The approach described seems very complicated and requires several arbitrary assumptions. First, the assumption about the correspondence between the wave direction and wind direction might be justified, e.g, in the Beaufort Sea, where waves are predominantly locally generated, but the wave climate in the North Atlantic is dominated by swell, so that the direction of waves reaching the ice edge likely has little in common with the local wind. Moreover, swell typically has a much narrower directional distribution than 45° used by the Authors (by the way: what does "total spread" mean? how is the wave energy distributed among the 5 directional bins?). This might seem a minor detail, but narrower directional distribution of wave energy means that the waves can penetrate deeper into the ice cover (with the same attenuation rate α_{dim}) – and the results presented in this paper suggest this might have a significant influence on the results.
Second, from the technical perspective, the approach requires advecting wave energy into the ice in a bin-by-bin manner, and waves from several locations can reach any given ice-covered grid cell. Using full ERA energy spectra would be much more elegant, but not much more complicated.

3. It is hard to prove without numerical simulations, but my impression is that some conclusions from this study might be affected by the – very artificial – assumption that the minimum floe size is constant. For example, during freezing conditions the FSD becomes wider due to increasing l_{max}, but the small floes remain as small as they were initially. Thus, it is not surprising that the model sensitivity to changes of the relaxation time T_{rel} is rather limited: the effective floe size is all the time dominated by the small floes, as they contribute a lot to the total perimeter, but much less to the total surface area of the ice. No rapid increase of l_{eff} is possible, even if freezing is fast. To represent lateral freezing, it seems more natural to shift the whole distribution towards larger floe sizes.
I'm not suggesting that the Authors should extend their study to simulations with variable minimum floe sizes, but some discussion of the expected consequences of constant d_{min} would be very useful.
4. The important role of the wave attenuation is particularly interesting from the point of view of possible feedbacks with floe size. If the wave attenuation rates are dependent on floe size, with stronger attenuation in fields of small floes (due, e.g., to floe-floe collisions), then sea ice fragmentation close to the ice edge might modify wave propagation into the MIZ, in turn impacting the floe sizes in the inner MIZ. The sensitivity studies presented in this manuscript suggest that accounting for processes that influence wave attenuation might be important from the point of view of reproducing annual evolution of MIZ.

<u>Other comments</u>:

1. Page 2, lines 24-26: A strange sentence, formulated in a complicated way, but stating something obvious: that if the floe size in a model is constant, it cannot be modified by processes that modify the floe size in the real world.
2. Page 3, line 39: I think you should introduce notation for the minimum floe size, maximum floe size and the power-law exponent already here, and consequently use those symbols throughout the paper (e.g., in order to avoid formulations of the type: "all the different exponent-minimum permutations" (page 10, line30); I had to think for a while to realize what an "exponent-minimum permutation" is).
3. Page 4, line 20: "developed developed"

4. The same notation, α, is used for the floe-shape parameter (e.g., equation 1) and for the exponent of the FSD. Please use different symbols for those two things – for example, use α_{shape}, which is used just once on page 14, line 19.
5. Is A ice area or ice concentration? You introduce it in equation (1) as ice area, but then the same symbol appears in different equations where it has to be non-dimensional to make sense (e.g., eq. 20 and eq. 10, to which I return in one of the next comments).
6. ΔT in equation (2) is not defined.
7. Page 6, line 12: wave cover? And further: wave spectrum? Rather wave energy spectrum.
8. First line in section 2.3: I think it would be good to state explicitly that N is the number-weighted FSD (there are other alternatives, e.g., area-weighted FSD).
9. Equation (10): Replace '=' with '→' for consistency with eqs. (12) and (13).
   Also, something is wrong with this equation. First, if A denotes ice area, then $1/A$ cannot be subtracted from 1; if A is ice concentration, then $1-1/A$ is negative. Further, the whole expression should be multiplied by the time step or another quantity expressed in seconds.
10. Equation (12): What is d_{frag}?
11. Page 7, lines 11-14: What happens when new ice is formed in a grid cell that was ice free?
12. Page 7, line 17: I don't understand the second part of this sentence.
13. Is x floe radius or floe diameter? From eq. (9) it follows that the floe surface area is πx^2; from eq. (14) – $\pi x^2/4$.
14. Equation (16): Note that this is valid for $\alpha \neq 3$ and $\alpha \neq 2$ (for $\alpha=3$, the integral in eq.(14) is $\log(l_{max}) - \log(d_{min})$; the same is true for eq.(15) and $\alpha=2$). This is important as $\alpha=2$ and $\alpha=3$ are among parameters considered in simulations.
15. Page 7, line 32: it should be P_{fsd} instead of P.
16. Page 8, lines 4-7: Why introduce d_{con} if l_{eff} can be used already in eq. (17)?
17. Page 8, line 22-23: The sentence "A reference run is…" is a repetition of the sentence in lines 20-21.
18. Starting from section 4.1, the Authors use negative values of α (e.g., lines 35-36 on page 8). This is inconsistent with section 2.3, in which the FSD is formulated as $N \sim x^{-\alpha}$. Usually in the literature, when references are made to "an exponent of the power-law distribution", a positive number is meant, as in the expression $N \sim x^{-\alpha}$. I'd suggest therefore speaking of exponents as positive numbers, and modifying the text accordingly. For example, line 7 on page 10 should be: "The exponent is increased from 2.5 to 3.5" instead of "reduced from -2.5 to -3.5".
19. Page 11, line 25: "lower ice cover"? Rather lower ice concentration.
20. Page 12, lines 19-20: "…even prior to the melting season in March". This part of the sentence suggests that one should expect the wave-induced fragmentation to occur mainly during the melting season. Obviously, it is mainly related to storminess and the related wave "activity".
21. The first paragraph in section 5 is a summary rather than discussion, so I'm not sure if it belongs here or at the beginning of section 6.
    Similarly, I'd suggest moving the last part of section 5 to section 6. Or simply merging sections 5 and 6 into "discussion and conclusions".