This manuscript describes the wave climate in the Kara sea as obtained from a 39-year long simulation using the wave model WAVEWATCH III. After introducing their motivations, the authors describe the configuration used to run their hindcast. They also give the details of the method used perform the storm activity analysis as well as a quick assessment of the wave model results. Using their simulation outputs, they perform a statistical analysis on the sea state over the 1979-2017 period with a particular focus on the wave climate and the occurrence of extreme events (high waves due to storms). They discuss the interannual trend and variability of the occurrence of storms, each storm event being categorized depending on the maximum wave height it is associated with. They find an increase of storm events in the Kara Sea over the studied period and show that the increase in the number of storms generating waves higher than 4m is correlated with the decrease in the ice cover. They then divide the studied domain into 6 sectors and show that the recurrence of storms obeys a Pareto distribution for most events. The distribution of events associated with the highest wave height does not however fit this Pareto law, and the authors classify them as "dragons". The probability of "dragons" is higher than foreseen by the Pareto distribution, and seems to be related to the decrease in the ice cover.

The statistics of extreme events is not my domain of expertise. I am however used to performing statistical analysis on wave model outputs, particularly in the Arctic, and my comments are below:

Although some of the results described by the authors could be worthy of being published, I cannot recommend this manuscript for publication in its current state for the following reasons:

- The choice of the parameterization should be more justified. Why are the authors not using a more recent version of WW3? It would offer different ways of accounting for the presence of sea ice, and thus could provide an estimate of the sensitivity of the results to the representation of wave-ice interactions. The manuscript also lacks a discussion on the sensitivity of the model to the choice of the parameterization.

- The validation of the model is very "light", while the quality of the sea state reproduced by the model is absolutely key to support the results described further in the manuscript.

- The manuscript is very hard to read and follow. The language used is very vague, and the English is often very confusing. For instance, the authors keep referring to "thresholds", "distributions", but often forget to mention "of what", forcing the reader to guess what the authors are referring to. The structure of the text should also be revised to emphasize more explicitly the authors' motivations and key results. The number of typos (missing verbs, misplaced brackets…) is also very high, which contributes to giving the impression of lack of rigour. I recommend a thorough rewriting of the entire manuscript before any resubmission.

I will try to be a bit more specific section per section:

Abstract:
Overall, the level of detail in the description of the results is very high, while the motivations for the study and the importance of the results remain unclear.

P1L16: *with different thresholds (from 3 to 7 m)*
Thresholds of what?

P1L2:3 *If in the entire sea the ice cover decreases that leads already to increase of the extreme storms.*
This sentence is very hard to follow, please use commas and check the English.

Introduction:
I get that interactions between waves and sea ice are not the core of your paper, but I would emphasize a bit more the challenge that sea ice represents for wave modelling. The quick change in sea ice conditions is what makes this study interesting.

P1L37->P2L44: I feel like the same arguments are repeated in every sentence, once should be enough.
P2L50 *data from models are preferable.*
They are also limited by the presence of sea ice, as waves-in-ice modelling remains quite challenging (e.g Squire, 2020). Also, I would not say that model data are "preferable" to observations.

P2L56 Please mention the method followed by Stopa et al.
P2L78 *These highlights […]*
I don't understand this sentence.

Data and methods:

P3L98 Why do the authors use WW3 v4.18 from 2014 and not a more recent version (5.16 or 6.07)? Version 4.18 is very limited in its ability to represent waves in ice

P3L110: As mentioned earlier, I would like more justifications for the choice of the parameterizations. Why ST1? It uses the same scheme as the 1st version of the WAM model at the end of the 80's. There have been some improvements since.

P3L110: IC0 simply considers an ice-covered grid point as land, this is the simplest solution of all. I also do not understand the comment on the exponential attenuation.

P4L132 In a spectral wave model, SWH is not computed as the average height value of the 33% highest waves.

P4L142 *within the sea sector*
What is the sea sector?

P4L144 *with waves more than this threshold.*
Please rephrase.

P4L150->153 All this paragraph is very confusing. It should start with the motivation (deep analysis of the extreme event with the SWH >3m), and the details should be rephrased to be less ambiguous (threshold of? What is the central part of the sea?).

P4L156 There is no conjugated verb in this sentence.

Fig2b is missing on my version of the manuscript.

P5L169 *We can conclude that WAVEWATCH III with set configuration adequately represents real conditions of the wind wave fields of the Kara Sea.*

I strongly disagree with this statement. Overall, I find section 2.3 very weak. Even with the panel (b) of Fig. 2, the evaluation of the simulation on which all the analysis relies is performed with only one mooring, over one month? This is far from being sufficient to me.
Moreover, the chosen period for the evaluation (September-October) corresponds to the ice-free period in the Kara Sea, which is likely to be the one for which the model performs the best. This quality assessment also ignores the effect of the forcing of the ice conditions: how do they affect the quality of the model? In the results, there is a lot of discussion about the links between wave height and ice conditions. In these conditions, the quality of WW3 outputs in winter should be evaluated as well.

Results

The analysis performed is interesting, and the figures are nice and readable. However, as written previously, I strongly disagree with the comments stating that the quality assessment is successful (P5L188). This section is also very hard to follow due to the presence of confusing expressions (for instance: "ice conditions become milder" P5L189; "allow confessing (?) the success" P5L188, "high sea ice conditions" P7L273 …). I would also recommend avoiding the use of words like "obvious", "easily" and statements like "it can be assumed that ice cover decrease in the whole sea" (P6L252).

P8L318 and P8L324: These two sentences lack a conjugated verb.

Discussions and Conclusion

This section is very long given that it is mostly a summary of the Results and the discussion part is very short. I agree that the quality assessment of the wave model for high waves is difficult. I also agree that wave height estimated from satellites might be inaccurate and not available for all seasons and the whole period. However, these limitations cannot justify a 39-year long simulation being considered as validated with a quality assessment over 2 points in September-October 2012 only.

References:

[Ocean Wave Interactions with Sea Ice: A Reappraisal](#)
Vernon A. Squire
Annual Review of Fluid Mechanics 2020 52:1, 37-60