General comments:

This paper aims to study the uncertainty of sea ice concentration (SIC) retrieval from satellites with a particular focus on the effect of this uncertainty on the wave height estimate by spectral wave models in the MIZ. The authors compare 8 different SIC products inferred from 2 different passive microwave sensors with observations made during a cruise in the Chukchi Sea in 2018 and SAR images that coincide with this cruise. Their conclusion is that none of these products are able to provide a consistent estimate of the SIC in the MIZ, mostly because sea ice in this region is very dynamic and heterogeneous. Following this analysis, they run a set of hindcasts using the spectral wave model WW3 forced by the different SIC products and discuss the sensitivity of the resulting wave height in the MIZ to the estimated SIC field. They show that this sensitivity is substantial, mainly due to the fact that the wave height decay in ice is very quick, and the SIC products strongly disagree on the position of the sea ice edge. They end their discussion by showing that wave model results are actually more sensitive to the SIC forcing than to the choice of the wave-in-ice parameterisations that they tested.

This paper presents interesting results, confirming some statements about the sensitivity of wave-in-ice modelling to sea ice concentration made in previous studies. The fact that the uncertainty in the estimated sea ice concentration has a larger effect on the uncertainty of the wave height estimated in-ice than the change in the wave-in-ice attenuation parameterization is a nice finding that illustrates very well the difficulties faced by wave-in-ice modellers. However, the paper suffers from a writing style that is confusing in a number of places. This is particularly the case for sections 3, 4 and 5. It is a bit paradoxal, as sometimes the important information is hidden in a succession of very wordy sentences, making it hard for the reader to get the message, and sometimes it seems that the authors wanted to avoid repeating themselves whereas the reader would happily appreciate some help. In the following comments, I will try to point out some of these unclear paragraphs, but overall the whole paper should undergo a careful rewriting aiming to make it clearer. I am also not convinced by the usefulness and the novelty of section 3, at least in its current form. To me, the most interesting part of the study lies in section 5, but it is overshadowed by the lack of clarity of the previous sections. It would be worth shifting the emphasis of the paper to this section more quickly. I will only recommend this paper for publication after these problems have been properly addressed. I also have other specific comments concerning the content, which require at least an answer from the authors. I also suggest a non-exhaustive list of typos and sentences that, in my opinion, need to be rephrased.

Specific comments:

P1L22: "model uncertainties...", which models are the authors talking about here?

P2L25: The authors should consider giving a definition of the MIZ.

P2L36: "wave-ice interaction source term": the authors haven't introduced the principle of spectral wave models yet, so it is not clear for everyone what the wave-ice interaction source term is.

The introduction is overall pretty clear and the gap in knowledge clearly identified.

P3L64: Here the authors introduce the methods used for the measurements on board on the R/V Mirai. But the way the so called "sea truth" images were taken is only described in section 3.1 (P7L212), lost in comments on the results. Similarly, before the end of section 3.1, a definition of how the uncertainty of a given quantity is computed is suddenly introduced, without even a proper

transition (P8L225). This mix between comments on the results and details of the methods makes section 3.1 very confusing and much longer than it should be.

P4L88 I think the word "translated" is not appropriate here (and in some other places). The authors could consider using "interpreted", "inferred".

P4L118 The sentences about the different grids the authors could have used and the one they are actually using are really confusing. Maybe they should try to cut them into more but shorter sentences, each dealing with one region and one resolution.

P5L143 "A curvilinear grid [...] sea point cells." I found this whole paragraph very confusing. As a reader I found very difficult to understand the links between each sentence, and the expression "The grid" seems to be applied to different things. As an example, they first refer to the model "geographical" grid, then to the spectral grid, then they use again the expression "the grid" to give details about the bathymetry. A quick reminder of the region they are focusing on would also be welcome, especially as they refer to the "other seas" at the border of "the domain".

P6L152 "During the version upgrade of TodaiWW3-ArCS...": which version upgrade are the authors referring to? Are they sure it is relevant for the paper? I think the authors could just state that they are using the ST6 parameterization for the non-ice source terms as it was previously shown to give the best results for the case discussed in Nose et al. (2018) with the model being forced by ERA5 winds.

P6L157 "The s_ice term is composed": I think the use of composed is misleading here. The attenuation terms the authors mention are included in different parameterizations (ISX, ICX), and they are not all compatible with each other. I would suggest an expression like "The s_ice term represents wave-in-ice attenuation processes such as..." for instance.

P6L160 "The dominant floe size [...] IS0 switch.": Here I have the feeling that the authors want to justify why they did not include scattering terms in their wave-in-ice source terms. I think this justification is very long and with unnecessary information (the way scattering terms work in WW3 for instance). I think it would be much clearer simply stating that during the cruise, sea ice in the MIZ was mainly made of grease, nilas and pancake ice, for which scattering is not expected to be the dominant process (Montiel et al., 2018), and therefore scattering was not considered here. Specifying the WW3 switch IS0 is also unnecessary.

P6L170 "The underlying principle of sea ice models is that sea ice is treated a continuum." Firstly, there is a small typo, it should be "treated as...". Secondly, this statement might be true for the sea ice models used in climate models, but it ignores discrete elements sea ice models, often used for sea ice-structure interactions. They can also be used to study wave-ice interactions (Herman et al., 2015). Actually I think the sentences between P6L170 and P6L173 could be shortened. The reason the authors choose this parameterization is because it has been developed to represent similar ice conditions to the ones encountered by the R/V Mirai, which is not the case for the other parameterizations.

P6L175 "... the treatment of independent SIC and sea ice thickness data sets is not a trivial matter." I am not sure I understand this statement. Would it be possible to develop this idea a bit more?

P6L179 "the former [...] experiment." The first part of the sentence is unnecessary in my opinion. I would also recommend avoiding using the word "domain" for the the SIC product, as it usually refers to the study region.

P6L184 "By doing so [...] in atmospheric models." This passage is very confusing, I do not understand the point the authors are trying to make. They should either rewrite it, if they think it is important, or remove it.

P7L189 "WW3 is a standalone wave model...": In this case, it is indeed used in standalone mode, but WW3 can be coupled.

P7L190 "numerical stability is unaffected": By what?

P7L195 Section 3
I am not particularly convinced by the major interest of this section, and particularly by the interest of comparing pictures taken from the boat to the sea ice concentration products in section 3.1. Which angle does the pictures cover? Which surface area are they representative of? As the authors say, sea ice tends to cluster in the MIZ, and the fact that the sea ice concentration is not uniformly distributed spatially is well known by anyone who has had the opportunity to go in sea ice covered places. As I understand it, these observations motivated the study, but to me the interest of this paper does not lie here, and I actually think the removal of section 3.1 could potentially improve the clarity of the paper. If the authors want to keep it, they must make clearer the novelty of these observations and their interest. Moreover, I find the writing style very confusing in section 3.1. Section 3.2 is more convincing and clearer, but it is hard to see any novelty in it. It could maybe bring more to the study by linking it more closely to the results of section 4 and 5.

P7L197 I don't understand the use of "respectively" here.

P9L270 : "... the sea ice cut-off criterion is not clear in the documentation." This is not a very satisfying statement. Have the authors considered contacting the people in charge of ArcMFC to get more information about this criterion?

P9L275 "... but the Piper [...] did not reflect the sea state.": It is quite confusing, please rephrase.

P9L283 "Furthermore [...] an important role." This is very noticeable indeed. It would be very interesting to give an estimate of the spatial attenuation coefficient at the ice edge assuming an exponential wave attenuation, in order to show how it compares with the models and other reported observations (for instance Kohout et al., 2015)

P11L343 I don't think that one can write that the MIZ is aligned with the wind. The MIZ is an area. Maybe the authors could substitute MIZ by "the ice edge".

P12L352 "The figure only comprising...": I don't understand this sentence. What does "highly forced waves" mean?

P12L362 "this can occur": What is "this"?

P12L365 "Here, ..." I don't understand this sentence either, please consider rephrasing it.

P12L371 "The off-ice [...] of Appendix D." These two paragraphs are very confusing in my opinion, mostly because they are not well structured. It makes it very hard for the reader to understand the problem the authors are trying to address. They should be entirely rewritten.

P13L405 "Three principal parameters that form the sea ice forcing are": This formulation is misleading. I would instead say: "The three main parameters used to tune the wave-in-ice attenuation in the IC2,IC3 and IC5 parameterizations are"

P14L421 "The values here [...] the adopted default source term parameters." I am not sure I understand this sentence, it should be rewritten.

P14L422 "Our analysis demonstrates..." This statement should be at least discussed a bit more. For instance, the authors have used a limited number of wave-in-ice attenuation parameterizations, and none of them represent the wave scattering. Also, could these results change in a MIZ made of large floes and thicker ice for example? In addition, the authors have assumed a constant sea ice thickess of 10cm, and it is known that the behaviour of attenuation processes can change significantly depending on the sea ice thickness (see for instance Boutin et al., 2018). The sensitivity of these results to the sea ice thickness should be explored and discussed, for example by setting it to 20/30cm instead of 10cm.

P14L442 The conclusion is, in my opinion, much longer than it should be. I think it would have more impact if the results were synthesized in a few sentences only, and if it was ending with a discussion on the perspestives and the consequences of the findings presented here.

P14L443 "Reliable modelling [...] melt the Arctic Ocean sea ice." I don't really see the cause/consequence link in this sentence.

P16L490 "Reliable shipboard [...] was slow." I find the formulations used in these sentences a bit ambiguous. For instance, what do the authors mean by "seemingly sensible"? I think I get the idea, but it is not very clearly expressed. It is also not clear to me what is validated in the first sentence.

P25 Figure 4 "showing considerable uncertainty": This comment should be in the main text, not in the caption. The font size of the legend is also too small.

Technical corrections :

General : I would recommend using a roman text font for the subscritpts that are made of more than one character in the equations (low in f_low) for instance (If you are using latex, it means that you should add "\rm" or "\mathrm{my_subscript}" in your equations). It would improve the readability of the paper.

P1L21 "encountering high winds...": encounters is already the verb of the sentence, so no need to repeat.

P6L174 "Sea thickness" ->"Sea ice thickness"

P14L440 "have"->"has"

P15L478: ":microwave..." -> ":a microwave..."

P15L480: "a variant but similar device of Kohout et al..." -> "a device similar to the one used by Kohout et al. (2015)"

References:

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Kohout, A.L., Williams, M.J.M., Toyota, T., Lieser, J., Hutchings, J., 2015. In situ observations of wave-induced sea ice breakup. Deep Sea Research Part II: Topical Studies in Oceanography. https://doi.org/10.1016/j.dsr2.2015.06.010

Montiel, F., Squire, V. A., Doble, M., Thomson, J., & Wadhams, P. (2018). Attenuation and directional spreading of ocean waves during a storm event in the autumn Beaufort Sea marginal ice zone. *Journal of Geophysical Research: Oceans*, 123, 5912– 5932. https://doi.org/10.1029/2018JC013763