Author response to tc-2019-285-RC1

Takehiko Nose on behalf of all authors

We sincerely appreciate the critique and insightful comments that have improved this manuscript. We are humbled by how succinct the revised manuscript is compared with the submitted version. We are grateful for the reviewer's attention to detail. Below outlines our response to each of the critical point raised by the reviewer. The document structure corresponds to that of tc-2019-285-RC1-supplement except for the first section.

Overview of major changes

We begin with an overview of major changes in the revised manuscript to address both reviewers' comment regarding readability of the paper. The change of section titles is shown in bold text (if changed).

Abstract The abstract has been rewritten to more clearly emphasise the focus of the study and its findings.

Section 2.3 The description of WAVEWATCH III ® (WW3) has been modified in a way that it is focused on the core idea of the paper: wave-ice interactions. The readability is improved by using equations and compact notations, thereby demonstrating clearly the link between sea ice concentration (SIC) and wave-ice models.

Section 3 Sea ice concentration: definition, characteristics, and the use in wave-ice models
The presentation of this section is one of the major changes in the revised version. We have rewritten this section to discuss the concept of SIC from a wave-ice modelling perspective. Despite different scales and objectives, the SIC is being used to moderate the attenuation rate of waves in the MIZ without the model having any consideration to the sea ice field heterogeneity, i.e., subgrid scale physics. We discuss the implication of the length scale of satellite retrieved SIC to the SIC and wave-ice model formulation.
Section 3.2 is shortened and moved to the beginning of Section 5 to give an overview of how satellite retrieved SIC varies on a regional scale so that the transition to the wave hindcast analysis of the region is more fluent.
Original Figures 3 and 4 are removed as they were deemed redundant.

Section 4 Δc_i effects on wave modelling at the observation sites
Note Δc_i is denoted as the SIC uncertainty in the text.

Section 5 Δc_i and wave modelling in the refreezing Chukchi Sea

Section 5.1 On- and off-ice wave evolution in the refreezing Chukchi Sea MIZs

Section 5.2 Relative significance of Δc_i compared with wave-ice interaction parameterisation uncertainty
We tested the robustness of the results based on sensitivity analyses that tested sea ice thickness (SIT) and the inclusion of scattering. The study findings remain unchanged.

Section 6 Conclusions and discussions
This section is shortened considerably and includes discussions on the study outcome.

General comments

Reviewer comment R#1-1 This paper presents interesting results, confirming some statements about the sensitivity of wave-inice 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.

Author response We are pleased the essence of this manuscript was conveyed to the reviewer. To enhance the clarity of our message, efforts were made to improve the readability of the paper by following reviewer's suggestions. Please see the overview of major changes.

Reviewer comment R#1-2 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.

Author response We appreciate the critical comment regarding the Section 3 content. Section 3 now reads as below.

Author's changes in manuscript WMO [2014] defines SIC as "the ratio expressed in tenths describing the amount of the sea surface covered by ice as a fraction of the whole area being considered". The so-called "area considered" presumably varies for different objectives. Length scale of $O(10)$ km may be adequate for sea ice extent climatology, but for wave-ice interactions, the wave provides a scale in a phase-resolved sense. Satellite derived SIC represents the fraction of ice-covered water over a large area, sufficiently large enough that the SIC represents a property of a continuum. In reality, the sea ice in the marginal ice zone (MIZ) is granular, and ice floes jam due to horizontal convergence by Langmuir circulation, internal waves, and wind variability, resulting in a formation of features such as ice bands and wind streaks—with which waves likely interact distinctively.
On 14 November 2018 during the MIZ transect observation, R/V Mirai encountered moderate on-ice waves with an H_{m0} up to around 2.00 m propagating towards the ice edge (this H_{m0} estimate is consistent from both the shipboard wave data described in Appendix A and hindcast models as discussed later). Figure 2 presents a series of snapshot images of the sea ice field during the encounter. R/V Mirai traversed over 10 km in the MIZ from the ice edge, and each image area extends at least over 1 km conservatively (using the crude distance to horizon calculation). These images depict the heterogeneous sea ice field, both in distribution and ice types, that waves propagate when they enter an MIZ. Because WW3 wave-ice interaction models are scaled according to $\frac{dN}{dt} = c_i s_{\text{ice}}$ (Equation 4), the subgrid scale physics is completely missing. It is plausible the subgrid scale distribution of SIC and ice types can be treated in a stochastic manner to provide meaningful mean values to the grid-scale model. On the other hand, c_i also affects the WW3 wave-ice model by means of scaling (Equation 4). Figure 2 shows SIC data from eight satellite retrieved products described in Section 2.2 during the event. The SIC estimates interpolated at the R/V Mirai positions largely deviate among the products, characterising the uncertainty of the satellite retrieved SIC. Moreover, the entire time series of the MIZ transect observation depicts Δc_i is persistent (Figures A4 to A6 of Appendix D). Hereafter, we show how large the effect of Δc_i on modelling MIZ waves can be, so much so that it overwhelms the choice of s_{ice}, e.g., ICX.

Specific comments

Reviewer comment R#1-3 : P1L22. "model uncertainties...", which models are the authors talking about here?

Author response We made a change to this passage (bold text) as below.

Author's changes in manuscript . . . From a practical view point, this downward trend of sea ice decline opens trans-Arctic shipping routes connecting Europe and Asia for longer times of the year; potential global economic benefits of non-ice breakers accessing routes like Northern Sea Route and North West Passage are substantial [Stephenson et al., 2013, Bekkers et al., 2018]. **The increasing vessel traffic implies that adequate prediction capabilities will become crucial to assist ships in polar waters to circumnavigate hazards such as high winds and waves, collision with perennial sea ice, and sea-spray icing; however, Jung et al. [2016] describe that the existing polar prediction systems need to be urgently enhanced to effectively manage the risks and opportunities associated with growing human activities, and the Polar Prediction Project (PPP) has contributed to advancing the predictive capabilities. While wave forecasting in polar oceans is still in its early years, the need for advancing wave forecast capacity will only grow in the emerging Arctic Ocean.** This paper focuses on the effect of SIC uncertainty on third-generation spectral wave model simulations in and near a MIZ.

Reviewer comment R#1-4 : P2L25. The authors should consider giving a definition of the MIZ.

Author response The following text is added.

Author's changes in manuscript WMO [2014] defines the MIZ as "the region of an ice cover which is affected by waves and swell penetrating into the ice from the open ocean". This study is primarily focused on the MIZ region near the open ocean.

Reviewer comment R#1-5 : 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.

Author response Changed the object from "the wave-ice interaction source term" to "the wave-ice interaction parameterisation".

Reviewer comment R#1-6 : 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.

Author response Please see author's response to R#1-2. Regarding the uncertainty definition, it is moved to Section 2.2 and introduced where the eight SIC products are described.

Reviewer comment R#1-7 : P4L88. I think the word "translated" is not appropriate here (and in some other places). The authors could consider using "interpreted", "inferred".

Author response SIC is a variable that is calculated from brightness temperatures, so we would prefer to have a quantitative phrase rather than being qualitative. In that sense, "calculate" is more appropriate here. Regarding the use of "translate" in other places, we have replaced them with more straightforward expressions.

Author's changes in manuscript SIC estimates from Earth-orbiting satellites are an indirect measurement calculated from microwave brightness temperatures.

Reviewer comment R#1-8 : 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.

Author response Changed to a shorter sentence as suggested.

Author's changes in manuscript ARTIST-Sea-Ice—this algorithm uses 89 GHz frequency signal to produce high resolution SIC estimates. This algorithm was selected as accurate higher resolution forcing is generally desirable for numerical models. For this product, we only use the AMSR2 data but analyse two different grids: the pan-Arctic data with 6.250 km resolution and the regional Chukchi-Beaufort data with 3.125 km grid resolution.

Reviewer comment R#1-9 : 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".

Author response Adding appropriate modifiers to "grid" was necessary, and the spectral grid description was mixed up in the regional grid description, which was confusing. The paragraph is clarified and shortened as follows.

Author's changes in manuscript The TodaiWW3-ArCS used in this study has a horizontal resolution of 4 km, and its domain covers most of the Pacific side of the Arctic Ocean including the East Siberian, Chukchi, and Beaufort Seas. The model boundaries connected to the seas of the Arctic Ocean was enclosed by ice cover during the November 2018 modelling period (corresponding to the R/V Mirai observation), so nesting was unnecessary. Similar to Rogers et al. [2016], we neglected swell penetration through the Bering Strait. The technical details of TodaiWW3-ArCS 's geographical and spectral grids are provided in Appendix B.

Reviewer comment R#1-10 : 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.

Author response Corrected as suggested. We have rephrased the sentence to make this paragraph clearer.

Author's changes in manuscript s_{wind} and $s_{\text{dissipation}}$ parameterisations and wind forcing for The Arctic Ocean wave model developed at the University of Tokyo (TodaiWW3-ArCS) were tested. We compared the most commonly used physics packages, ST4 [Ardhuin et al., 2010, Rascle and Ardhuin, 2013] and ST6 [Rogers et al., 2012, Zieger et al., 2015, Liu et al., 2019], using European Centre for Medium-Range Weather Forecasts (ECMWF) global reanalysis (ERA5) 10 m wind (U_{10}) against the 2016 September storm [Nose et al., 2018] when TodaiWW3-ArCS and observations agreed well. The ST6 parameterisation showed marginally improved agreement using the default parameters; so all simulations used the ST6 parameterisation and were forced with ERA5 wind fields. The default $s_{\text{non-linear interactions}}$, which is not affected numerically by sea ice, was used for all simulations.

Reviewer comment R#1-11 : 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.

Author response The passage relating to the WW3 wave-ice interaction term is one of the major changes. This now passage reads as follows.

Author's changes in manuscript . . . The sum of these source terms s is expressed based on the following default scaling in ice-covered waters:

$$s = (1 - c_i)(s_{\text{wind}} + s_{\text{dissipation}}) + c_i s_{\text{ice}} + s_{\text{non-linear interactions}}.$$

Specifically to this study, c_i relates to the satellite retrieved SIC and s_{ice} to the ice type, i.e., how the model treats sea ice. The effect of sea ice on waves are represented via the modified dispersion relation $\sigma = \sigma(\bar{k})$ where $|\bar{k}| = k = k_r + ik_i$. The real part k_r is the physical wavenumber and alters the propagation speed of waves in a sea ice field (analogous to effects of shoaling and refraction by bathymetry), and the imaginary part k_i is the exponential decay coefficient. k_i is introduced in the model as $s_{\text{ice}} = -2c_g k_i N$ for fully ice-covered sea, i.e., c_i=1, and the solution to $\frac{dN}{dt} = s_{\text{ice}}$ is $N_0 e^{-2c_g k_i t}$. There are five options for treating sea ice in WW3 denoted as IC1–5; c_i provides the scaling in the linkage between s_{ice} and ICX as

$$\frac{dN}{dt} = c_i s_{\text{ice}} = -2c_i c_g k_i(f, p_1, ..., p_n)N$$

where $p_1, ..., p_n$ are the sea ice properties, e.g., effective shear modulus and effective viscosity. Therefore, the rate of attenuation depends on the wave period and sea ice properties, which is moderated by c_i, i.e., $N_0 e^{-2c_i c_g k_i t}$.

The wave-ice models implemented in WW3 that calculate k_r to model k_i are as follows: IC2 calculates dissipation due to basal friction in the boundary layer below an ice sheet, which is modelled as a continuous thin elastic plate based on the work of Liu and Mollo-Christensen [1988]; IC3 treats sea ice as a visco-elastic layer based on Wang and Shen [2010], which calculates the internal stress of the ice cover based on storage and dissipation; and IC5 is a visco-elastic beam model based on Mosig et al. [2015]. The dispersion relation of these models are provided in Appendix B. Ardhuin et al. [2018], Boutin et al. [2018] (IC2) and Rogers et al. [2016], Cheng et al. [2017] (IC3) describe the progress of these s_{ice} parameterisations using the refreezing Beaufort Sea data of Thomson et al. [2018]. These wave-ice models can be combined with an energy-conservative scattering attenuation model denoted as IS1 and IS2 [Meylan and Masson, 2006, Dumont et al., 2011, Williams et al., 2013, Ardhuin et al., 2018, Boutin et al., 2018].

Reviewer comment R#1-12: 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.

Author response Thank you for this suggestion. The suggested text is succinct and used in the manuscript.

Author's changes in manuscript During the cruise, sea ice in the MIZ was mainly grease, nilas, and pancake ice, so the hindcast experiment was conducted using the IC3 package with the default parameters as it has been designed for these ice types [Rogers et al., 2016, Cheng et al., 2017]. Scattering is not expected to be the dominant process in this type of ice fields [Montiel et al., 2018], so it was not considered in the experiment.

Reviewer comment R#1-13: 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.

Author response We have incorporated the reviewer's comment. Please see the first sentence of author's response to R#1-12.

Reviewer comment R#1-14: 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?

Author response It is much clearer just to say the constant thickness was applied, so we can evaluate solely the SIC uncertainty on wave modelling. The text is modified as below.

Author's changes in manuscript Regarding sea ice thickness forcing, a homogeneous input option with a value of 10 cm was applied; the constant forcing was applied so we can evaluate solely the ΔSIC effect on wave-ice interaction parameterisations. 10 cm was initially chosen because the MIZ transect observation was mostly characterised by new and young ice whose upper bound of sea ice thickness is of a similar order [Canadian Ice Service-Environment Canada, 2005].

Reviewer comment R#1-15: 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.

Author response Corrected as suggested.

Author's changes in manuscript ASI-3km and OSISAF-AMSR2 data were excluded for the wave hindcast experiment. The former has a regional coverage that is too small for the TodaiWW3-ArCS domain, and the OSISAF-AMSR2 data have noise in the open ocean, which yield erroneous wave simulation results when they are used as model forcing (as described in Appendix C).

Reviewer comment R#1-16: 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.

Author response The passage is rewritten as below.

Author's changes in manuscript It should be noted that when satellite derived SIC data are used as forcing, the heat and momentum fluxes are distorted in the marine atmospheric boundary layer because the lower atmosphere and the ocean surface are no longer coupled. Inoue et al. [2011] evaluated surface heat transfer from three reanalysis products by focusing on how the models treat sea ice; they found the accuracy of SIC is a key variable for estimating surface turbulent heat fluxes. Guest et al. [2018] have elucidated the ice-edge jet generation mechanism based on the in situ data obtained in the refreezing Beaufort Sea. Undoubtedly, altering the sea ice field would feedback to the wind, but this is not captured in the wave hindcast experiment.

Reviewer comment R#1-17: P7L189. "WW3 is a standalone wave model...": In this case, it is indeed used in standalone mode, but WW3 can be coupled.

Author response This is true the modifier "standalone" was unnecessary. Further, the entire sentence turned out to be superfluous when we addressed R#1-16, and as such, the sentence is removed.

Reviewer comment R#1-18: P7L190. "numerical stability is unaffected": By what?

Author response This sentence is removed. Please see author's responses to R#-16 and R#-17.

Reviewer comment R#1-19: 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.

Author response Please see the overview of major changes and author's response to R#1-2.

Reviewer comment R#1-20: P7L197. I don't understand the use of "respectively" here.

Author response This passage is moved to an appendix, and the "respectively" is removed there.

Reviewer comment R#1-21: 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?

Author response Thank you for this comment: the statement was unsatisfactory. We found out that the wave-ice interaction of Sutherland et al. [2019] was implemented in December, 2019: the month after our Arctic Ocean observation. We still find value in reporting the ARCMFC model data in our results, but we have amended the ARCMFC model description.

Reviewer comment R#1-22: P9L275. "... but the Piper [...] did not reflect the sea state.": It is quite confusing, please rephrase.

Author response Waves in the Chukchi Sea during the study period was dominated by wind seas, which have shorter wavelength relative to the ship dimensions. These short waves are impeded by R/V Mirai's hull, so the shipboard Piper#15 has limitations on measuring wind seas. As such, most of the Piper#15 data did not reflect the true wave field. We have rephrased the paragraph, and the explanation of this point is moved to an appendix. So it is removed from the main text. Please refer to author's changes in manuscript to R#1-34 for how we addressed this comment.

Reviewer comment R#1-23: 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).

Author response Thank you for the comment. We realised we used incorrect expressions to describe the wave height uncertainty earlier in this paragraph, which likely led the reviewer to inquire about the attenuation rates. In P9L278 of the original manuscript, we stated "waves decay with varying attenuation rates"; however, this should have been "waves decay at different timing depending on the sea ice edge location of the respective SIC forcing used". The whole point of the experiment is testing different SIC forcing using the consistent wave-ice interaction source term setting, i.e., the same attenuation coefficient. We apologise for the misleading sentence.

We are also mindful that a successful study of wave attenuation rates depends strictly on the knowledge of the ice edge or availability of two and more buoys along the fetch. As such, we are unable to repeat the novel analysis of Kohout et al. [2016] because we did not have enough measurements along the fetch.

Reviewer comment R#1-24: 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".

Author response The sentence is corrected by replacing "MIZs" with "ice edges".

Reviewer comment R#1-25: P12L352. "The figure only comprising...": I don't understand this sentence. What does "highly forced waves" mean?

Author response Highly forced waves imply a wave field, specifically wind seas, that is rapidly growing under the wind forcing. The sentence is rewritten to clarify the point.

Author's changes in manuscript The figure shows only blue and light-blue markers, which indicate the waves generated by the strong localised wind decayed with limited wave penetration no farther than $mean(c_i) = 0.40$.

Reviewer comment R#1-26: P12L362. "this can occur": What is "this"?

Author response "this" is specified.

Author's changes in manuscript . . . and although the enhanced scatter plot disparages data with low $mean(c_i)$, high ΔH_{m0} with low $mean(c_i)$ does exist. Analogous to the on-ice wave case, high ΔH_{m0} can occur near . . .

Reviewer comment R#1-27: P12L365. "Here, ..." I don't understand this sentence either, please consider rephrasing it.

Author response In this sentence, "highly forced" was unnecessary and is removed. The subject of the sentence is also clarified instead of using "Here".

Author's changes in manuscript Along this transect, ASI-6km and BST-AMSR2 have the most north east ice edge, and the waves rapidly grow under the strong north easterly wind forcing whereas . . .

Reviewer comment R#1-28: 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.

Author response The repeat introduction of the equation in both paragraphs made them seem unstructured. We also made the second paragraph more succinct. The two paragraphs are modified as below.

Author's changes in manuscript Off-ice wave evolution is a complex process because the fetch is not only controlled by the location of the ice edge, but also wave-ice interactions as implemented in WW3. The current numerical approach to simulate wind pumping energy into waves in ice cover is dictated by c_i because waves grow when $(1 - c_i)(s_{\text{wind}} + s_{\text{dissipation}}) > c_i s_{\text{ice}}$. Whether wave evolution in ice cover follows the Equation 3 scaling has been discussed in Rogers et al. [2016], Thomson et al. [2018]; the latter cites Li et al. [2017] who confirmed wind input to high frequency wave energy in the Antarctic Ocean. The off-ice ΔH_{m0} is apparently also influenced by the cumulative effect of the Δc_i along the fetch distance affected by the wave-ice interactions as implemented in WW3.
Lastly, for both on- and off-ice wave cases, significant ΔH_{m0} extends to the waters where the wind forcing is orientated along the ice edge; so the model data are briefly examined in the region of MIZs north east of Wrangel Island, which is shown as Quadrilateral 1 in Figure 8a along the sea ice edge and north easterly wind forcing orientation. This region has considerable Δc_i (not shown here) in a similar manner to Figure 5, and the model ΔH_{m0} is just as sizeable under the influence of high wind forcing. There is evidence of correlated bivariate uncertainty data in Figure 8b, and a combination of on- and off-ice wave features for the respective enhanced plots discussed in the previous paragraphs are depicted. Deciphering the physical processes is complicated; however, the bivariate uncertainty data along a transect illustrates how Δc_i and ΔH_{m0} are related; Figure 9b shows these results for a cross section oriented along the ice edge (the long axis of Quadrilateral 2) on 21 November 2018 18:00.

Reviewer comment R#1-29: 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"

Author response The sentence is changed as suggested.

Reviewer comment R#1-30: P14L421. "The values here [...] the adopted default source term parameters." I am not sure I understand this sentence, it should be rewritten.

Author response This sentence was unnecessary and is removed.

Reviewer comment R#1-31: 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 thickness 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.

Author response Agreed that further discussion is warranted. We tested robustness of the study finding by checking sensitivity to SIT and scattering. The modified text is as follows.

Author's changes in manuscript . . . Uncertainty distributions are visualised in a Q-Q plot by simply sorting each dataset, and this is shown in Figure 10. The figure depicts that both uncertainties are considerable with $max(\Delta H_{m0})$ values of 1.95 m and 1.44 m for the Δc_i and s_{ice} uncertainty experiments, respectively. The robustness of this result was examined via SIT forcing sensitivity analysis. From a physical view point, the choice of 10 cm was made to match the observed sea ice types during the R/V Mirai MIZ transect observation. For observational evidence of SIT in the refreezing Arctic Ocean, we defer to Ardhuin et al. [2018] to determine the test case and selected 50 cm. From a wave-ice modelling perspective, SIT effectively serves as a tuning parameter when forced as a homogeneous field. For example, the attenuation rate k_i of IC2 as shown in Appendix B has SIT in the form of $(1 + k_r M)$ in the denominator: $M = \frac{\rho h_i}{\rho_w}$ [Liu and Mollo-Christensen, 1988] where h_i is the SIT, and ρ and ρ_w are the ice and sea water density. If we take a deep water wavelength corresponding to 7 s wave period, changing the SIT from 10 cm to 50 cm increase the k_i by at most 3 %. k_i sensitivity on SIT examined in Wang and Shen [2010], Mosig et al. [2015] (IC3 and IC5) appears more sensitive; as such, sensitivity analysis was conducted for our model. Repeating the Δc_i and s_{ice} uncertainty experiments using 50 cm SIT, $max(\Delta H_{m0})$ values increased respectively to 2.34 m and 1.95 m, but the Δc_i remains as the dominant error source. Further, IC3 was most affected by the SIT change for the equivalent transects of Figure 9 (not shown). Even though there was no event during the study period when scattering were expected to be the dominant process (the implication of this is given in Section 6), sensitivity of the finding to scattering was also examined by combining IS2 scattering with IC2 and IC3 with the default parameters. The results remained robust. Lastly, sensitivity to the choice of SIC data used in the s_{ice} uncertainty experiment was examined by using ASI-6km instead of BST-AMSR2: the experiment also resulted in the same outcome.

Reviewer comment R#1-32: P14L422 . 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 perspectives and the consequences of the findings presented here.

Author response The conclusion section has been made more succinct and reads as follows.

Author's changes in manuscript The WW3 wave-ice models represent the exponential decay of waves in the presence of sea ice as $\frac{dN}{dt} = c_i s_{\text{ice}} = -2c_i c_g k_i(f, p_1, ..., p_n)N$ (Equation 4). We investigated the effect of the satellite derived SIC uncertainty Δc_i on modelling waves in the refreezing Chukchi Sea MIZ using six SIC data sets based on the four commonly used retrieval algorithms: NASA-Team, Bootstrap, OSISAF, and ARTIST-sea-ice. The wave hindcast experiment reveals Δc_i causes model wave height uncertainty ΔH_{m0}, and there is evidence that bivariate uncertainty data (ΔH_{m0} and Δc_i) are correlated, although off-ice wave growth is more complicated due to the cumulative effect of Δc_i along an MIZ fetch

We compared the ΔH_{m0} distribution of the Δc_i experiment with that of the s_{ice} uncertainty experiment. Both uncertainties are found to be considerable during the simulation period with maximum ΔH_{m0} values of 1.95 m and 1.44 m, respectively. This result is found to be robust based on the sensitivity analyses that tested the SIT forcing and the inclusion of scattering. Despite the s_{ice} parameterisations being derived from different concepts and the WW3 wave-ice models completely missing the subgrid scale physics relating to sea ice field heterogeneity, the accuracy of satellite retrieved SIC used as model forcing is the primary error source of modelling MIZ waves in the refreezing ocean. The study outcome suggests wave-ice model tuning may not be as effective at this time when the knowledge of the true SIC field is too uncertain. It is worthy to note that swell waves that propagate $O(100)$ km into the ice-covered water where the scattering would likely be the dominant process were not observed during the study period. As such, the effect of Δc_i for such waves remains to be resolved. Future improvements on the wave-ice models should come from two ends; continual developments of parameterised physics on the regional and pan-Arctic scale and working on a subgrid scale physical model on the other end. Solid and robust observational evidence through remote sensing and shipboard measurements is likely the key to connecting these two ends.

Reviewer comment R#1-33: P14L443. "Reliable modelling [...] melt the Arctic Ocean sea ice." I don't really see the cause/consequence link in this sentence.

Author response The sentence is removed as it was unnecessary. See author's changes in manuscript to R#1-32.

Reviewer comment R#1-34: 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.

Author response How these data were used in the study is described with more clarity. We have rewritten this passage in Appendix A explaining how the shipboard data were used as below.

Author's changes in manuscript Wave observations during the campaign from the WM-2 integrated analog system [TSK Tsurumi Seiki Co., 2019] were significantly affected by Doppler correction errors. Collins III et al. [2015] have shown shipboard measurements are less affected by this effect when ship speed is < 3 ms^{-1}. Applying a 2 ms^{-1} ship speed threshold greatly reduced conspicuously spurious data, and these data are used as indicative wave heights in this study (e.g., Figure 6). Piper#15 on board the vessel relies on an IMU. The processing method is consistent with Kohout et al. [2015] except 15 minute intervals were used instead of 1 hour. Waves in the Chukchi Sea during the study period was dominated by wind seas, which have shorter wavelength relative to the ship dimensions. These waves are impeded by R/V Mirai's hull, so the shipboard Piper#15 has limitations on measuring wind seas. Response Amplitude Operator of R/V Mirai and the WM-2 data can be combined in theory to transfer IMU's high frequency signals to true surface elevation signals, but post-processing remains ongoing work. Although most of the Piper#15 data did not reflect the true wave field, the peak Piper#15 H_{m0} of 2.00 m during the on-ice wave event on 14 November 2018 agreed with the peak WM-2 H_{m0}; this value is also comparable with the ERA5 H_{m0} as well. This provides confidence that the waves observed during this event was at least around 2.00 m.

Reviewer comment R#1-35: 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.

Author response The phrase pointed out by the reviewer is removed from the caption, and bigger legend text is inserted.

Technical corrections

Reviewer comment R#1-36: General. I would recommend using a roman text font for the subscripts 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 "\mathrmmy_subscript" in your equations). It would improve the readability of the paper.

Author response Subscripts are changed to roman text as recommended.

Reviewer comment R#1-37: P1L21. "encountering high winds...": encounters is already the verb of the sentence, so no need to repeat.

Author response Removed the verb phrase. Please see author's changes to manuscript in R#1-3.

Reviewer comment R#1-38: P6L174. "Sea thickness"→"Sea ice thickness"

Author response Corrected.

Reviewer comment R#1-39: P14L440. "have"→"has".

Author response Corrected.

Reviewer comment R#1-40: P15L478. ":microwave..."→":a microwave...".

Author response Corrected.

Reviewer comment R#1-41: P15L480. "a variant but similar device of Kohout et al..."→"a device similar to the one used by Kohout et al. (2015)".

Author response Changed as suggested.

Thank you kindly for your review and consideration.

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