

I have been asked to review this manuscript after previous revision iterations already have been taken. The editor asked me, in particular, to assess if and how well the reviewers' comments have been answered.

In my opinion, the authors have provided convincing answers to the points raised by the reviewers and have implemented good updates into the manuscript. At this point, I have very little comments about the manuscript content per se, and I believe that the manuscript presents a good quality analysis of the data investigated, and convincing and best practice methodology. Naturally, there are always limits with accuracy and resolution in time and space with satellite data, but this is already well discussed, and the authors perform a number of mitigation techniques such as averaging and considering statistics.

Therefore, I do not have any major technical comments at this point.

I can recommend a couple of points that could be added in a few sentences to provide a couple more high level discussion points, but these are possibly quite subjective, so these are only recommendations and I understand if the authors do not want to add them

- This paper is an important contribution as it helps take in use satellite data for better understanding waves in ice and, as a consequence, greatly increases the amount of data and the width of the underlying sampling compared with previous small scale analysis. I believe this is a very important aspect of advancing this field and correcting previous mistakes. For example, small sample effects combined with peculiar ice conditions have to the best of my understanding, been proved to be the cause for the wrong equation 1 in the manuscript <https://www.nature.com/articles/nature13262> (Storm-induced sea-ice breakup and the implications for ice extent, 2014), which has been "debunked" in a follow up paper by a group of authors including some of the initial authors: "These results suggest that the conclusion in Kohout and others (2014), that large waves decay linearly, is an artefact of analysing a small dataset in different ice conditions." <https://doi.org/10.1017/aog.2020.36> . The present work, by allowing to take in use larger dataset, is an important step towards "robustifying" the results presented about waves in ice against such issues.

We agree with the reviewer that large datasets, particularly those that capture a wide range of environmental conditions, are critical in assessing wave dynamics in the polar regions. In particular, small datasets are more likely to be dominated by specific conditions or events (usually because they are obtained within the same region), which complicates the interpretation of any results that follow.

We have therefore added the following in the Discussion & Conclusions section (Lines 299-303 of revised manuscript):

"Nevertheless, averaging observations across a broad range of sea ice conditions has the advantage that it reduces the likelihood of bias from small sample size effects (e.g., Kohout et al., 2020), where the data may otherwise be disproportionately influenced by environmental conditions that are over-represented in a dataset. As such, the approach followed in our study may provide a more reliable methodology for identifying the overall effects of sea ice properties on wave attenuation rates."

- A somehow similar issue has been observed regarding the rollover effect, which is now believed to come from noise present specifically in buoys measurements combined with small dataset, as highlighted in "Spurious Rollover of Wave Attenuation Rates in Sea Ice Caused by Noise in Field Measurements" (2021).

We agree with the reviewer that this is a critical quality control consideration. The spurious rollover effect is not necessarily caused by small datasets, but rather by not considering an appropriate signal-to-noise ratio threshold during data quality control. This, if not appropriately addressed, will lead (and has led) to erroneous results/observations/conclusions. We note, however, that this was briefly mentioned already in the Results section (Line 160), and we therefore decided not to discuss this further in the Discussion section.

These two topics (there may be more similar examples that I am not aware of) have caused significant confusion in our community over the years and both are the consequence of closely related issues: low volume and limited quality of field data. In my view, this is typically the kind of challenges that the present study helps mitigate and correct, which is an important contribution to our community.