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Comment on egusphere-2022-870

Anonymous Referee #3

Referee comment on "Feasibility of retrieving Arctic sea ice thickness from the Chinese HY-2B Ku-band radar altimeter" by Zhaoqing Dong et al., EGUsphere, https://doi.org/10.5194/egusphere-2022-870-RC3, 2022

This manuscript introduces the retrieval of Arctic sea ice freeboard and thickness with the radar altimeter onboard HY-2B. The inclination angle of HY-2B allows the coverage of up to 82N, and the satellite potentially constitutes an important source of information for sea ice of both polar regions. Specifically in the manuscript, the authors focus on the processing of converting existing range (or elevation) product of HY-2B into radar/ice freeboard and thickness. I consider the data from HY2B and this submission a good contribution to the community, but I do have the following comments that in my opinion that should be addressed first.

The overall uncertainty analysis needs to be more clear and precise, especially in terms of the determination of freeboard uncertainty. First, the (claimed) uncertainty used for further analysis is sigma_SGDR, which is only 2cm and should be a lower bound of the actual range uncertainty. This uncertainty, in its true quantity, applies to both SSHA and individual freeboard. Since SGDR is the upstream dataset this work relies on, it is not necessary a quantity that needs much elaboration within this work. However, its value needs to be justified. For example, what is the gate resolution of HY-2B? Is the 2cm uncertainty due to the combination of several footprints (therefore smaller)? Furthermore, the authors state that Ricker et al. (2014) `believed' the random uncertainty of radar freeboard to be determined by radar speckle noise. Is it possible to provide any other proof of how is the reference relevance to the treatment here?

Second, the uncertainty of SSH is computed as the standard deviation (SD) of SSH points (of the along-track 25km segment). I think this is a crude guess, and unfortunately, probably an underestimation of SSH-induced uncertainty. Because the along-track points that are not used for determining SSH share the same uncertainty caused by the same set of SSH points, and therefore the uncertainty of the retrieved freeboard samples on the same track is systematic (rather than random). Therefore, in Eq. (11) it should be averaged out and diminished much more slowly.

Third, although the authors treat the snow-induced uncertainty as random error (possibly a typo on line 400), it is hardly the case, despite that the snow depth is based on both climatology and PMI retrieval. A simple counter argument is that: the PMI product is based on C-band PMI onboard AMSR-E/2, which is at about 60km in resolution. Not to mention the 8-grid smoothing that is carried out over the snow depth retrieval. Then there exists large local correlation of the snow depth uncertainty, given that 25km grid is used in this study. Arguably more importantly, the climatology of snow depth from W99 plays a very important role in the snow-depth composite of AWI, which is evident in the respective technical report. Given that W99 is halved for the combined product, its induced uncertainty is still very large and will dominate the portion caused by snow in the uncertainty of the final ice thickness product. Therefore, I will be very cautions to treat the snow depth related uncertainty as random errors.

Finally, the density of snow and ice are also treated as random errors. This should be also a very `optimistic' estimation, especially the effect on the final ice thickness uncertainty. Overall I suggest that the authors reconsider the uncertainty quantification process and divide into the random part and the systematic part, and be cautious about which category each term belongs to. Differentiation of the uncertainty also ensures fairer comparison with the AWI product, and the author seemed to have used the random error of it, which is much lower in may part of the basin (e.g., Beaufort Sea).

Another major issue I noticed is the limited data availability of HY-2B even in the later months of the winter. As shown in Figure 6 and 8, there are large areas with no valid radar freeboard on the monthly scale: on the Eurasia continental shelf, Beaufort Sea, etc. What is the cause of the missing data? Invalid waveforms. I suppose that at this latitude HY-2B should have better coverage than CS2.

Also, the authors mainly compared the HY-2B retrieval with CS2. However, Sentinel-3 and AltiKa have the same inclination angle as HY-2B, and especially Sentinel-3 satellites work on Ku-band (although they are of delay-Doppler type). Is it better to compare against Sentinel-3 retrievals? This is only a suggestion, out of my curiosity. The authors can decide whether this is an option or not.

The comparison against CS2-IS2 data contains inconsistencies in the methodology. The comparison of radar freeboard is inherently between two CS2 retrievals (one from AWI and the other one carried out by Kwok). Therefore it's not a fair comparison, since the two products based on CS2 arises from the same source of information. So many issues are not present, such as limited representation by altimetry.

Minor issues:

Some figures contain maps that are too small to read, such as Fig. 6 and 8. Pls increase the font and maps accordingly.

For the third-party and auxiliary datasets, I suggest that the authors just introduce them, without too much comments on the specific advantages. For each product there are potentially uncertainties that are fully addressed. I think just stating the basic status quo of the products is already enough.

The reference list is not strictly ordered.

The language usage needs improvements. General suggestions include avoiding long sentences, and avoiding complex statements. Several typos are present. I suggest that the authors give an overhaul of the manuscript after the major issues are addressed.