

The Cryosphere Discuss., referee comment RC2
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Comment on tc-2022-39

Anonymous Referee #2

Referee comment on "Winter Arctic sea ice thickness from ICESat-2: upgrades to freeboard and snow loading estimates and an assessment of the first three winters of data collection" by Alek A. Petty et al., The Cryosphere Discuss.,
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This paper discusses improvements in the ICESat-2 (IS2) processing of sea ice thickness retrievals from different releases of the IS2 products. As such it feels more like a NASA technical report that discusses how the different versions change the thickness retrievals. The author previous published in 2020 on the processing chain to IS2 and I do not find that with the changes this now warrants an updated assessment of thickness changes and a new publication. The question is what do we really gain from this paper vs. having a NASA technical report on the changes in data processing?

This is in part because any sea ice thickness (SIT) assessment depends strongly on the choice of snow loading used. It also depends strongly on the choice of snow depth processing applied to OIB data for validation of your snow loading, and the seasonality of this validation period. It seems that with the changes presented to NESOSIM there are minimal changes anyway to the snow loading and thus it is the changes to the lead detection that seem to have the largest influence. To make this paper more impactful and not just a technical report on updates to IS2 data processing, one way forward could be to assess the choice of snow loading in the IS2 SIT retrievals. Since Zhou et al. (2020) already showed how different these data products can be, and other studies such as Mallett et al. (2021) and Glissenaar et al. (2021) detailed how using different snow loading can lead to different trends, one really cannot trust any assessment of thickness changes over the 3 years evaluated here without addressing the uncertainty in the snow loading. How would Figures 8-10 look using different snow data sets for example? You state that it's the freeboard processing that results in the largest changes (again indicative that this should be a technical report), but given the wide variety of snow depth data sets out there, the 3 years analysed here may be quite different depending on data set applied. And is analysing 3 years of data really useful for assessing drivers of SIT variability? At the moment I really do not see much value in having this as a publication in The Cryosphere for an incremental update to the IS2 processing chain. That doesn't mean it shouldn't be published someplace, but The Cryosphere should be for more impactful papers.

More specific major comments:

It is stated that NESOSIM is updated to use ERA5 calibrated against CloudSat and a new blowing snow term. However, there is no validation of this blowing snow loss term, or discussion on how the coefficients, i.e. wind action threshold, blowing snow loss coefficient and atmosphere snow loss coefficients are derived and validated. There is no in situ evidence that a significant amount of snow is lost to leads in the winter (any lead in winter quickly refreezes in a matter of a few hours), and there is no assessment here of the magnitude of this new snow loss term, and comparison to the old (and presumably still used) snow loss term to leads. Since SIT retrievals depend very strongly on the snow loading, at a minimum some quantitative analysis is needed on what these changes represent in terms of the overall snow mass, and some science justification is needed for doing this in the first place. It seems that some artificial tuning is based on trying to reduce the mean difference with OIB snow depths, but of course those are not perfect either. And they are done only in the springtime, and the question is how valid this bias-correction is for other months during the winter season?

The author is wrong about what SM-LG does at the end of summer as it keeps the snow cover in places where it doesn't entirely melt out. Also, snow can start to accumulate before September in the Arctic, and thus it seems these changes are made purely to reduce your bias but there is no physical reason to justify these changes. I do not think that because NESOSIM matches mW99 in October that you can conclude you have "good" snow depths. In fact given delays in freeze-up, I would expect much thinner snow in October compared to mW99 based on the fact that ice is forming later than it used to.

Zhou et al. (2020) showed large differences between the various atmospheric reanalysis-based approaches to snow loading as well as the remote sensing-based retrievals, with the SM-LG (Liston et al. 2020) providing more spatial structure to the snow depth/density distributions, whereas products such as NESOSIM are artificially smoothed products. I see you get around this by taking your smoothed products and then adding some artificial spatial structure to match IS2 resolution, but why regrid to 100km in the first place? Anyone who has spent time on sea ice knows the snow is very heterogeneous and thus the artificially smoothed 100km NESOSIM product seems unrealistic. Some justification for regridding the snow depth to 100km is needed and why you think this artificially smoothed data set is a good representation of snow over sea ice. Also, the impact of the redistribution then to 30m resolution is needed.

Some assessment of the impact of using different ice motion products is also needed. It is not true that updated ice motion from NSIDC is not available, and the author could have contacted the data provider for updated ice motion fields. Since OSI SAF and NSIDC ice motion vectors do not agree, how does this influence your results? It is also unclear now how the Warren et al. climatology is used, are you assigning MYI snow depths on September 1 based on W99 and then accumulating snow? And finally, I'm not sure why so much smoothing is applied to both the snow and SIT retrievals, and some justification for this is needed. What does your SIT data product really give us if so much smoothing is applied? Snow and ice are highly spatially variable and thus is this a data product that is really useful to the community if it is artificially smoothed? Wanting "pretty" maps is not a reason to do this.

I do not find much value in the CS2 to IS2 comparison. In particular, now suddenly the mW99 climatology is applied after spending much time discussing updates to NESOSIM. This seems to be only because you want to use existing products out there, which we already know are not realistic because they do not have a realistic snow loading representations. Instead, maybe comparison of the freeboards would be a better thing to do, as you can convert the IS2 snow freeboards to ice freeboards with your snow loading from NESOSIM. Then we can better understand differences on the ice freeboard level, and may be get some insights into where the dominant scattering surface from CS2 is located as well as the influence of surface roughness on the freeboard retrievals. The use of PIOMAS is also not useful in my opinion, it's a model and has known biases, so adding it here just distracts from the overall paper.

The abstract is too long and reads more like a technical report.