The Cryosphere Discuss.,
https://doi.org/10.5194/tc-2017-103-RC2, 2017

The Cryosphere Discussions
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Interactive comment on "Inter-comparison of snow depth retrievals over Arctic sea ice from radar data acquired by Operation IceBridge" *by* Ron Kwok et al.

Anonymous Referee #2

Received and published: 27 July 2017

The paper provides an inter-comparison of five different NASA OIB snow depth products that have been developed in recent years. The OIB products are compared against in-situ data from two field campaigns and snow depth estimates derived from the ERA-Interim reanalysis and a modified snow climatology.

This work addresses an important issue for the Arctic sea ice community - what is the snow depth over Arctic sea ice and which (if any!) OIB derived snow depth product should we be using? The effort in bringing together these various snow groups and datasets is laudable. I believe the study should be published - we need to see these differences and have a baseline for snow inter-comparison discussions - but I believe

the paper has some shortcomings that need to be addressed first.

Main comments:

1. I think you need to discuss more the basin-scale differences between the products. I also think this should be moved further up the paper, as it is what motivates the whole exercise in my opinion, especially as you don't go into huge detail regarding the algorithm differences and echogram interface detection. It is also what most of the readers will be interested in seeing.

For example, I think the huge differences in the means across the products for the three ice classes should be its own figure near the start (although I also have issues with the use of age classes, see later main comment, so think this should be by region instead). There are differences of $\sim$100% between some products in the earlier OIB years, while some products show strong regional trends and others don't. It's pretty crazy to me that this is not discussed more as a motivating factor to look more closely at the algorithms, and I feel these differences have been somewhat hidden later in the paper. The more detailed in-situ comparisons could then follow on to help understand why this is.

2. Any comments on how 'tuned' these data have been, especially to the other snow depth data included in this paper? I believe I'm right in thinking the different groups have all had access to these in-situ data and ERA-I snow depth fields (especially as the lead author has previously produced the ERA-I derived snow depth maps used in the inter-comparison) so is that one reason why some fits are better than others? I understand that tuning happens and is often needed, but I think we need to understand this more to really understand if the differences are due to the choice of algorithm or other factors. Also, I think comments should be made if the individual algorithms were also compared against any other in-situ datasets in their respective papers and how good those fits were. The fact all authors are involved should make this easier.

3. As all the algorithm developers were part of the paper, I'm surprised a bigger com-

ment was not made of what actually will happen next. Will one/multiple algorithms be scrapped or combined? Are there pros/cons of certain algorithms that will be adopted/used by the Operation IceBridge sea ice group? You do state in the paper that: "The aim of this paper is to examine these algorithms and to use the assessment results to inform the development of the next generation algorithm", but the path forward is unclear to me and I really hope we don't continue with multiple algorithms floating around that different groups/papers use for different reasons.

4. I'm not a huge fan of using the ice type mask to delineate the results. The comment on Page 14: " MYI that advected into this region, which was used in the construction of the modW99 fields but is absent in the ERAI-sf fields (because the MYI is not used in the estimates). " implies that the presence of MYI doesn't mean much for snow depth. I believe other cited studies (from co-authors) have come to similar conclusions (e.g. recent King and Webster papers). The modified Warren climatology seems just plain wrong in my opinion so I would be tempted to drop that entirely unless you want to make the point that some groups are using this now and we need to explore its potential biases.

5. Why were only these specific field campaigns chosen?

6. Why ERA-Interim for derived snowfall?

7. Why were the Wavelet retrievals not available? The Newman et al., (2014) paper shows that data were produced in 2012..? This seems odd.

Specific Comments:

P2, L14 - maybe 'needs to be inferred by other methods' instead of left to be measured or modeled

P2, L14 - I think (if I've interpreted this right) that you should say why snow density matters before saying we need routine measurements of it.

P2, L16 - you say hence, but then start by discussing forecasting, which seems odd.

P2, L20 - I think more should be made of the fact that people still use this climatology, despite it being many decades old!

P2, L23-25 - 'of about several centimeters' and 'broadly consistent' seems pretty loose. Drop or further clarify.

P2, L26 - mention that this is predominantly the western Arctic, (except for 2017 which isn't included in this study).

P2, L29 - add something like 'from the OIB snow radar..'

P5, L30 - this sentence is poorly worded.

P6, L4 - why and how did it vary with ice topography? Just because it was older

do we think this is an exhaustive list of retrieval algorithms?

P8, L12 - unsure of the comment " The initial application to existing OIB snow radar data from various campaigns (2009 - 2012) and the need for it to be applicable to future campaigns, required a process that would adapt to the data and not be dependent on fixed thresholds in the radar return signal ". Why can't you apply thresholds and update these each year when you process the data?

P9, L6 - how is it robust? It seems we are testing that in this paper, no? What do you mean by this?

P9 - Does the removal of deformed ice from the Wavelet algorithm introduce a bias compared to other algorithms?

P9, L20 - is this the only thing that has been removed from the algorithm?

P10, L11 - this should be Figure 3.

P10, L15 - I'm a bit confused by this. the resolution of each radar footprint is around 5-10 m, right? So how does a 20 m radius circle correspond to 9 radar spots again?

P10, L18 - you mean the mean AND standard deviation, right? In which case I don't get

how using an averaging window changes the mean value. An average of an average should produce the same average..? It should obviously change the shape of the distribution though (reducing the tail).

P11, L25 - can you list them here? It looks like the JPL and Wavelet correlations decrease. Any comment on how this compares with the htopo parameter? i.e. do you expect surface roughness to co-vary with the snow depth variability?

P12, L14 why only four of five algorithms? Pretty interested to see the NSIDC differences, especially as this is probably the most commonly used..?

P12, L27 - this seems the fundamental tenet of the whole paper, no?!

P14, L20 onwards - this should go at the start of the section in my mind as it's a pretty key point.

Interpreting Table 2 and 3 was pretty painstaking at first. Can you make it more obvious that the diagonal elements are taken from Table 2 and maybe draw a box around these?

Figure 9 - Confused by the numbers in Figure 9a. There is a lot of information being crammed in and I struggled to understand what it all means.

- Why is this saturated at 15 cm?

- Why are the NSIDC panels missing the repeat tracks and distributions?

Multiple figures - The Jet color scale introduces false boundaries, isn't good for people with colorblindness, and should thus not be used in my opinion! Very bad for comparing geospatial data by eye.

Figure 10 - this seems pretty pointless so I would be inclined to drop it.

Figure 12 and 13 should be split up and made more readable.

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