Review of

Retrieval of the thickness of undeformed sea ice from simulated C-Band compact polarimetric SAR images - Revision 1

by

Zhang, X., et al.

Since this is the revised version of the manuscript I initially reviewed I skip to write a summary here. Instead I comment on the rebuttal letter which the authors wrote in a careful way, replying convincingly to the concerns I had with the previous version of the manuscript.

The manuscript has substantially improved. I have no general concerns anymore but I recommend that the authors spend a little more time in
i) clarifying the apparent differences between their model results - which start at a thickness of around 0.2 m - and the experimental results which show sea ice thickness values down to 0.1 m or even below that. A comment stating that the theory is valid for sea ice thickness values below 0.2 m and also continues in the way as suggested for sea ice thickness values close to that limit would be helpful (Note that the slope e.g. in Figure 5 decreases particularly for the larger incidence angles around 0.2 m, and it is not clear what is the thickness of the very first data point.);
ii) summarizing their results in a more modest way with regard to the uncertainty and with regard to the validity of their approach as a function of the sea ice thickness encountered;
iii) commenting on the obviously peculiar sea ice thickness distribution for the fast ice areas;
iv) using a more correct terminology in terms of ice types related to the thickness range they are considering.

All these points are mentioned again in the specific comments in more detail.

General comments:
None

Specific comments:
P1,L17: I am a bit confused about what is meant here with "radar parameters" given the fact that further up you highlight the CP-Ratio.

P2, L15: I suggest to replace "thin" by "first-year" or "seasonal" because "thin ice" by WMO definition is sea ice < 30 cm thick and this is not what Kwok et al. referred to in their paper.
I suggest to add an "e.g." as well in front of the citation.

P2, L18: Same as above, I suggest to add an "e.g." as well in front of the citation.

P3, L4: The authors could add "near-real time" to underline the difference between operational applications and climate applications.

P3, L10: What is "sufficient accuracy"? 1 m? 1 cm?

P4, L5: Why would one convert a PDF of freeboard into a PDF of draft?

P4, L19: I am not too happy with this sentence. Space-borne altimeters have been used to map sea ice thickness, yes. But from space-borne radiometers there are only a few attempts to derive sea ice thickness. Most of them - if not all of them - are severely limited concerning the maximum sea ice thickness to be retrieved. Therefore space-borne radiometers should not be mentioned in the context of being a main source of sea ice thickness retrieval.

P4, L22: I don't think that this sentence reflects the nature of the SMOS sea ice thickness retrieval. I suggest that the authors read the paper by Huntemann et al. (2014) they cited and also the paper by Tian-Kunze et al. (2014, The Cryosphere) to describe properly how sea ice thickness information is retrieved using SMOS data.

P5, L4-7: The citations refer to ICESat and CryoSat-2 only. I suggest the authors either remove the other two sensors or they add suitable citations for these other two sensors as well.

P5, L9: The authors could add "radar" in front of "altimeters systems" because they omitted ICESat which had a footprint of about 70 m diameter and ICESat-2 which will have footprints of the order of a few meters.

P6, L11: I suggest to write "A RADARSAT-2 Quad-pol scene ..." instead of "The RADARSAT-2 scene ..."

P6, L12/13: I suggest to omit the "()" around the information about the swath width.

P6, L14-16: Perhaps the authors could underline this statement with a citation?

P10, L17: Perhaps better "We consider the CTLR ..."?

P20, L17/18: The authors could switch the two last elements in this sentence:
"... of brine inclusions reduces due to desalination processes as ice thickness increases."

Figure 6: What is the motivation to use case 2 (light nilas) here instead of case 3 as is used in Figure 5? Is this to demonstrate the sensitivity (change) for even smoother sea ice than case 3 sea ice?

P21, L16-19 / Figure 7: I have difficulties to understand what the authors say here. The sensitivity of the CP-Ratio to sea ice thickness does not change as function of incidence angle; only the magnitude of the CP-ratio changes. Now: Why is it better to have a high CP-Ratio? Figure 7 reveals that for thin ice (0.2 m) the sensitivity is larger at smaller (20 degrees) than larger (60 degrees) incidence angle.
Figure 7 starts at 0.2 m but includes annotations for DN and LN, which are both sea ice types with a maximum sea ice thickness below 0.2 m. My understanding so far was that DN is something until about 5 cm while LN goes until 10 cm continued by grey ice and grey-white ice.

P21, L22: Yes, that is true - at least down to sea ice thickness values of about 0.2 m.

P22, L6/7: The sentence: "This regime ..." reads as if the sea ice thickness retrieval would only be possible for the range 0.2 to 0.4 m - which is not the case I assume.

P24, L14/15: I guess the authors meant "... overlain with the EMS flight tracks ..." because I cannot see any sea ice thickness information on Figure 8, bottom.

P24, L20: I suggest to replace "several open water and scattered frazil ice areas" by "several openings" because I doubt that the authors can make a statement about which is open water, which is dark nilas, and which is frazil ice.

P25, L3/4: May I ask at which air temperatures this heavy snow fall was reported? And what is "heavy"? 10 inches of snow?

P25, L5: I suggest to add "(not shown)" behind "... photos."

P25, L7-10: The authors may be right here with their statement but i) the study of Barber and Nghiem was based on mostly fast ice within the Canadian Arctic Archipelago which grows under different environmental conditions and ii) the fact that heavy snow fall was reported for 2-3 days beforehand could be an indication that the snow load may easily have been able to reduce the elevation of the ice-snow interfaces towards - if not even below - the water line. This could cause sea water entering the ice-snow interface and being wicked up also at air temperatures below -20 degree C. But at the end this might not be that relevant for this first case study.

P25, L12-15 / Figure 9:
- I suggest to add the bin width in the caption of Figure 9.
- I suggest further to mention which flight tracks are used to generate which histogram. The information is given on P26 but I suggest to place it closer to Figure 9.
- I find it a bit peculiar that the fast ice total ice thickness histogram has these multiple modes. In particular, the modal sea ice thickness is located at the bin centered at 20 cm (which seems odd in March, close to the end of the sea ice growth season), there is a second mode centered at 80 cm and a shoulder extending to about 190 cm. The authors could have commented on this.
- The authors mention in L14 that "it" consists of rafted floes. How do they know? Is this the interpretation of the total ice thickness histograms?
- L15: "reveals a deeper snow cover". Well, yes and no. The modal snow depth is 6 cm in both cases - but indeed the landfast ice has a second mode at about 15 cm which is missing for the drift ice. However, since the critical snow depth in your investigation is something like 20 cm you could perhaps give the percentage of data with snow depths below / above your "critical" snow depth.

P27, L8-10: Does this mean that you excluded deformed ice from you analysis as well? It gets not clear in what the authors write here. I can formulate the question also like this: Are areas with deformed ice in the SAR image still entering step 5 or not?

P27, 13: "ice zones of 50 m in length" ? Are the authors talking about the co-located, drift-corrected EMS tracks in the SAR image? Perhaps the authors could use here also "segment" or "flight track segment"? The authors could write more clearly that the flight tracks were cut into 50 m long segments. For these segments simulated CP SAR data were taken from the co-located, drift-corrected, segmented SAR images - provided that the 50 m segment contains a homogeneous piece of ice.
- What is the motivation to use 50 m?

P27, L19: Could it be that the 13 x 13 pixel window was chosen in accordance with the lee-filter used in the pre-processing?

P28, L21: Could be, yes, but we have seen that the used parametrization is sub-optimal at around 0.4 m sea ice thickness and creates an inconsistency.

Figue 10:
- If I compare Figure 10 with Figure 5-7 I can see that the measurements end up at a smaller CP-ratio than the theoretical computations. The authors could comment on this in the paper - i.e. that this is not relevant and to be expected as the model underlying the theoretical calculations was an over-simplication of the actual situation.
- I like the 90% confidence intervals of the fitted lines. It is, however, nice to see that there are hardly any CP-ratio values below 0.03 (except one). Could this be taken as sort of an uncertainty range for the CP-ratio derived from SAR? How accurate can you (theoretically) estimate - with your current knowledge about the accuracy of the used RADARSAT-2 SAR data - the CP-ratio? I am asking because in P29, L10, you set the upper limit to 1.8 m and I am wondering what the motivation for this upper limit is?
- The authors state on P29, L9, that all data points (320 samples) are used. Am I correct assuming that this is the entire number of data points shown in Figure 10? I am wondering whether the authors could make a comment - if deemed relevant - about the fact that for incidence angle 49 degrees they could use almost only (except 1) sea ice thickness values > 0.5 m.
- The ice thickness shown here (and in Figure 11) is the total (ice + snow) thickness minust the snow depth, right?

P29, L10: While I see that your measurements went all the way down to even less than 0.1 m, in your theory you did not go down to these low sea ice thickness values. Figure 5-7 stop at about 0.2 m and do (by the way) not suggest a further increase in the sensitivity to sea ice thickness as indicated in Figure 10. Is this a limitation of the theory?

P30, P2: The authors could add that the division of the 159 samples into two sub-samples was done in an arbitrary way.

P30, L6: The authors could add that these coefficients are (of course) different from those derived in Eq. (14) from the same two SAR images.

P30, L12/13: I am not sure what the authors wish to stress with the sentence put in (...) here. The latter case are the smaller sea ice thickness values.

P30, L14-17: I like this statement. It can be also seen from Figure 11 a) that until CP-ratios of 0.08 the sea ice thickness range retrieved is relatively small whereas - at least in the example shown - for CP-ratios < 0.08 the range of retrieved sea ice thickness values increases a lot. In a way your example suggests a "saturation" thickness of between 0.6 m and 0.8 m to be retrieved with the CP-ratio with reasonable accuracy. This is also supported by the break in the good relationship between estimated and observed sea ice thickness between 0.6 m and 0.8 m shown in Figure 11 b).

Figure 11, Caption, L5: What is meant by "standard deviation of the plots"?

P31,L7-10: I suggest to the authors (following up with my comment just above) that they are even more careful in making statements about the sea ice thickness range and the accuracy with which the sea ice thickness can be retrieved from the CP-ratio with their method. Figure 11 clearly reveals that the relationship between CP-ratio and sea ice thickness potentially breaks down above a sea ice thickness of about 0.6 m to 0.8 m - at least for the sea ice encountered in their area of interest. Stating that the RMS error is 0.12 m for the range 0.1 - 1.8 m is a bit misleading seeing the fraction of data points with sea ice thickness > 0.7 m; only 8 data points are above 0.7 m which is 1/10 of the number of samples shown in Figure 11a). Hence the low RMS-error for the large range is dominated by the RMS of the other 90% of the data which are at sea ice thickness < 0.7 m.
In addition to that, in order to obtain the sea ice thickness values shown in Figures 10 and 11 you needed to discared regions of deformed sea ice from the SAR images as decribed in section 4.2. How likely is it that the relatively few remaining thick sea ice cases are in fact not level thick sea ice but also originate from deformation events?
The histograms of the total (ice + snow) thickness reveal dominant modes at around 0.5 m and around 0.8 m for drift ice and fast ice, respectively, which points to that likely these are the "typical" total thickness values observed - in addition to the yet unexplained 1st fast ice thickness mode at 0.2 m.
Therefore, in the light of the above-said and in the light of the environmental conditions I would be "honest" and state perhaps the range 0.1 m to 0.8 m as the most useful range for your application at the current stage.

P31, L11: I suggest to replace "is" by "can be".

Typos:

P3, L2: "e. g." --> "e.g."

P10, L11: "conisdering" --> "considering"

Figure 4: Legend and y-axis annotation says "The volumes fraction ..." Shouldn't it be "The volume fraction ..."?

The authors might check whether "CP-Ratio" is written in italic font in the running text. They might also check whether they always wrote "CP-Ratio" and not just "CP" (for the latter see P21, L15).

P23, L12: "G Hz" --> "GHz"

P24, L15: "According the" --> "According to the"

P28, L21: Delete "since"