

Signature of Arctic first-year ice melt pond fraction in X-band SAR imagery

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Review of the revised paper

General comments

The authors have presented detailed, proper answers to all my comments to the previous version of the paper, and made corresponding changes and additions to the paper. I think that the paper has improved considerably. Below are some further comments, the major one is related to the T4 data and its analysis.

The empirical models for the MPF estimation were developed using datasets over a large ice floe. Why were not all co-incident SAR imagery vs. airborne photography used? How results would change if they were?

As this study was performed on drifting sea ice, co-location between SAR scenes and helicopter data is very challenging. This is also commented on in the conclusion of the manuscript. Most of the airborne photos were not possible to be co-located with the satellite observations exact enough to meet our demands of a high quality study. However, for the investigated floe, we managed to do a reliable co-location, and the floe was also the only floe appearing in two of the scenes. We therefore chose to focus on this specific floe in our investigations to secure the quality of the study.

You could also explain this shortly in the paper.

Results Section could include short introductory at the start about its content.

Reference Divine et al. (2016, in review) describes the method to estimate DTM and surface roughness from stereo camera photos, but as its 'in review' readers do have access to it, any published references? Conference papers? I have to say I don't personally like these 'in review' references, as they may not get published in the end.

ENL is a far more complicated issue, and we do not see how including this would strengthen the manuscript. We are not statistically modelling the speckle distributions, and only look at the mean values after smoothing.

I was thinking about radiometric resolution: $10 \cdot \log_{10}(1 + 1/\sqrt{\text{ENL}})$, but it is likely insignificant for mean σ_0 values. I have estimated ENL over textureless open water areas. Your comment on the subject was ok.

Section 3.2 Data set: line 272. Reader gets an idea that NESZ was subtracted from SAR imagery, and then images are analyzed, but later Section 4.1 results are presented with and without noise reduction in SAR imagery. I think you should selected either way, or does having both add great value to your results and paper?

Section 4.1, lines 386-397 and Table 4: For SAR scene T4 acquired at very low wind speed, HH and VV σ_0 decreases with increasing melt pond fraction. This makes sense as there are more low backscatter smooth melt pond surface with increasing mpf. However, RVV/HH also decreases with increasing mpf (negative Spearman's correlation in Table 4), I found this puzzling as smooth surface should have high RVV/HH. In addition, in (Scharien et al. 2014a), Figure 5, RVV/HH is not decreasing with increasing mpf. Scharien et al. had strong positive relationship between RVV/HH and mpf is low wind speed (1.1 m/s) case. In Figure 10 of Yackel et al. (2000) s0VV slightly increases with increasing mpf under low wind speed conditions (1.5 m/s). Something wrong in your SAR data, too large contamination by SAR noise? If not, how do you explain the observations?

Would be nice to see RVV/HH vs. mpf plot for the T4 case. How RVV/HH vs. mpf in T4 case changes if you set a lower signal-to-noise ratio for s0 data, for example 5dB, i.e. only those RVV/HH datapoints with SNR larger than 5 dB for s0HH and s0VV are accepted? I think you need to investigate further your T4 data.

I am should have presented this observation in my first review, sorry about this...but I noticed this now.

Finally, Yackel and Barber (2000) speculated that MPF may be more closely related to the albedo than to melt pond fraction due to the fact that albedo results from the integration of all surface types (snow, saturated snow, melt ponds) which contribute to the measured MPF. What's the authors' view on this; would it be better to investigate the relationship between SAR data and albedo than SAR and melt pond fraction? Please, discuss this in Introduction Section.

This question might have a typing error, and we find it slightly unclear. We interpret the question to ask whether estimated melt pond fraction should be compared to albedo instead of observed melt pond fraction. Albedo refers to the average reflection of waves in the visible range of the microwave spectrum. As SAR uses microwaves to evaluate the sea ice surface, we find it more credible to utilize differences in the microwave signature between melt ponds and sea ice, or methods that employ statistical features describing fractional mixtures of surfaces.

In our study, albedo is not measured, and would have to be estimated by upscaling from melt pond fraction measurements and in situ measured albedo values of different surface types. This method would inevitably introduce additional uncertainty to the results (see f.ex. Divine et al., 2015 for estimates made for the study area), and is therefore not advisable for our data set. The relationship between albedo and polarimetric features will therefore not be discussed in detail in our manuscript.

My idea was that as backscatter measured with SAR, if pixel size is not too small, is a mixture of backscatter from meltponds, bare ice, snow covered ice, then the backscatter could be more related to albedo than melt pond fraction. Albedo is naturally derivative of mpf. This is what Yackel and Barber (2000) speculated, see page 22068, left column. But you can leave this out the paper.

Specific comments

Abstract

“The regression fits gave good estimates of mean melt pond fraction for the full satellite scenes, deviating with less than 4% from the airborne retrieved melt 15 pond fractions in the investigated area.”

Mention shortly the airborne data used in the melt pond fraction estimation.

“A smoothing window of 51x51 pixels gave the best reproduction of the width of the melt pond fraction distribution.”

Give also pixel size of SAR images, only window size in pixels does not tell geometric size on the area.

“optical satellites”

better “optical imagery”

1 Introduction

1. 36: “Formation and evolution of melt ponds are poorly represented in sea ice models, potentially contributing to an underestimation of the observed sea ice extent reduction in model projections (Flocco et al., 2012; Holland et al., 2012; Flocco et al., 2015).”

Possible to explain shortly the reason for this underestimation? If not, then there are these three refs for the readers.

l. 42: “Several algorithms have been developed for retrieval of melt pond fraction...”

f_{MP} defined before, could be used instead of “melt pond fraction”. Check also whole text for the same issue.

l. 104: “TerraSAR-X offers very high resolution multi-polarimetric data...”

What do you mean by multi-polarimetric data? You have dual-polarisation TSX SAR images.

You could add at the end of Introduction Section short overview of the content paper Sections. Like “First, Section 2 presents ...”

2 Melt ponds in SAR imagery

l. 118: “During very calm conditions, the SAR signal of melt ponds is mainly specular.”

Maybe better “scattering from melt ponds is mainly specular”

p. 3, l. 127: “The coverage of melt ponds varies during the melt season, starting out with a high fractional cover, and reducing as the ponds drains. At the end of the melt season, the melt ponds refreezes.”

I think melt pond fraction first increases, has a maximum level, and then decreases, ending up with remaining ponds refreezing, see e.g Fig. 6 in Rösel, A., Kaleschke, L., and Birnbaum, G.: Melt ponds on Arctic sea ice determined from MODIS satellite data using an artificial neural network, *The Cryosphere*, 6, 431–446, doi:10.5194/tc-6-431-2012, 2012.

l. 184: “The sea ice surface roughness was found to high to fill the criterion...”

‘too high’

l. 201: “Entropy (H) is a part of the $H=A=$ polarimetric decomposition, based on the eigenvectors and eigenvalues of T , describing SAR scattering mechanisms.”

This sentence should have a reference to the polarimetric decomposition.

3 Methods

Figure 2 in grayscales could show better σ_0 variation.

4 Results

l. 375: “and values in parentheses show results after before NESZ subtraction of the signal”

...NESZ subtraction in the SAR imagery...

Figure 4: gray and blue dots does not separate well; very long minus sign on -0.49

Figure 6 and 10: what is the spatial size of the images in km?

5 Discussion

l. 558 “From the helicopter images, some of the very low RVV/HH values observed at the investigated floe in scene T3 were from slightly deformed areas, possibly explaining the negative ratios. However, no general trend in low RVV/HH values in deformed areas was found in our study.”

Paper: Sea ice type and open water discrimination using dual co-polarized C-band SAR, T Geldsetzer and J J Yackel, *Canadian Journal of Remote Sensing* Vol. 35 , Iss. 1,2009

may have interesting results in the context of your paper. Average RVV/HH larger than 0 dB for various ice types, but <0 dB observations also exist.

In Figure 4 large amount of negative RVV/HH values, not saying they are wrong, but kinda puzzling...calibration error between TSX HH and VV channels? If so, I guess there is nothing you can do to correct it.

6 Conclusions

1. 632: "At very low wind speeds (0.6 m/s), the backscatter signal from the melt ponds became too low for fMP retrieval based on polarimetric features. In that case, s0VV was found suitable for fMP estimation."

If SNR is too low for polarimetric features, then why it is not too low for s0? Again, the T4 data may be too contaminated with SAR noise for mpf retrieval studies.