

Sea ice melt pond fraction estimation from dual-polarisation C-band SAR – Part 1: In situ observations

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General comments

I also reviewed the Part II of this paper, some of my comments for the Part II also concern this paper, and I will note when they do.

Both papers have quite long Introduction Sections where some issues are discussed in both papers. If the papers are not required to be ‘independent’ in The Cryosphere then paper I could have all introduction on melt ponds, discussion on previous studies etc., and paper II would have Introduction only to the work to be conducted in there, and main review of satellite remote sensing of melt ponds. I would favor this, but I guess this for the editor to decide.

The term ‘dual-polarisation C-band SAR’ is somewhat poor, sea ice remote sensing community would assume from this that you are referring to HH and HV images which are available from RS-2 and Sentinel-1, and not to HH/VV images which you are referring. How about: “from HH/VV polarisation C-band...”?

The authors have set up a theoretical framework (Bragg scattering model), designed and conducted field measurements of sea ice and melt pond roughness and backscattering coefficient signatures, and analyzed the data for the capability of the C-band co-polarization ratio in retrieval of melt pond fraction. This kind of work is needed for development of SAR based retrieval methods of melt pond fraction in Part II of their study, and in general for supporting SAR based sea ice remote sensing. In general, the paper is well written, and the data acquisition, processing and analysis methods are mainly scientifically sound and discussed in needed detail. However, there are some issues, outlined below, which require detailed response from the authors, and maybe also changes in data analysis methods and theoretical background.

An anonymous reviewer (RC 37 Review Regarding Field use of LiDAR) has expressed serious reservations on methods used for LIDAR data collection. I don’t have expertise on surface roughness measurements with LIDAR and ultrasonic distance sensor, but the expressed reservations seem to me scientifically correct, and the authors must properly answer to them, and, if necessary, make adjustments their surface roughness retrieval methods from the measured data. The authors could look into following two papers where laser profile data have been used in sea ice surface roughness determination to see if they help here.

Carlström, A., and L. M. H Ulander, “Validation of backscatter models for level and deformed ice sea ice in ERS-1 SAR images,” *Int. J. Remote Sensing*, vol. 16, no. 7, pp. 3245-3266, 1995.

Dierking, W., M. I. Pettersson and J. Askne, “Multifrequency scatterometer measurements of Baltic Sea ice during EMAC-95,” *Int. J. Remote Sensing*, vol. 20, no. 2, pp. 349-372, 1999.

The paper needs discussion on nature of surface roughness of natural targets, like cultivated soil and sea ice. The roughness has been observed in many papers to be fractal-like, for sea ice e.g. in (Manninen 1997), i.e. surface profiles follow fractional Brownian motion (fBm). fBm itself is a non-stationary process, but its increments are stationary. The functional dependence of rms roughness s of the fBm on the measurement length l has a power-law form (Church 1988):

$$\ln(s) = a + b \ln(l)$$

and correlation length L is linearly dependent on l (Church 1988):

$$L = k_0 l.$$

The coefficient a is related to the actual profile level, whereas b describes the variation of the profile with spatial frequency. The value of a depends on the unit used for l and s . The coefficients k_0 and b are functions of the fractal dimension D of the surface (Church 1988):

$$k_0 = \frac{2(2-D)^2}{(9-4D)}, \quad b = 2-D.$$

The authors need to justify why they are assuming stationary surfaces in their data analysis, i.e. l and s do not depend on the surface profile length or the area of the surface. I can note that both Carlström and Ulander (1995) and Dierking et al. (1999) assumed stationary surfaces in their work.

Church, E. L., "Fractal surface finish," *Applied Optics*, vol. 27, no. 8, pp. 1518-1526, 1988.

Manninen, T., "Surface roughness of Baltic Sea ice," *J. Geophysical Research*, vol. 102, no. C1, pp. 1119-1139, 1997.

For the Paper II I criticized using Bragg scattering model due to its small range of validity. The IEM would be better, but with it the co-polarization ratio is not independent of roughness. The discussion on the selection of the backscattering model should also be in this paper. There is also 'fractal'-version of the SPM scattering model where the σ^0 is a function of D , but in the co-polarization ratio the D -dependence cancels out.

Franceschetti et al., "Fractals and the small perturbation scattering model," *Radio Science*, vol. 34, pp. 1043-1056, Sept./Oct. 1999.

G. Franceschetti, A. Iodice, S. Maddaluno, and D. Riccio, "A fractal based theoretical framework for the retrieval of surface parameters from electromagnetic backscattering data," *IEEE Trans. Geosci. Remote Sens.*, vol. 38, no. 2, pp. 641-650, Mar. 2000.

As for the Paper II I pointed out that Yackel and Barber (2000) speculated that backscattering coefficient (σ^0) 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 σ^0 . What's the authors' view on this; would it be better to investigate the relationship between PR and albedo than PR and melt pond fraction? Discuss this in Introduction Section.

Also:

In a previous study with the same first author as here it was demonstrated that albedo of melt-pond-covered landfast FYI can be estimated with better accuracy using C-band SAR derived co-polarization ratio than using only co-polarized σ^0 at larger incidence angles. This is likely the first study where the usability of PR in albedo/melt pond fraction retrieval is discussed, and it should be mentioned in the Introduction.

Why in Scharien et al. (2007) you studied PR vs. albedo relationship, but now switched to PR vs. melt pond fraction?

R. K. Scharien, J. J. Yackel, M. A. Granskog, and B. G. T. Else, "Coincident high resolution optical-SAR image analysis for surface albedo estimation of first-year sea ice during summer melt," *Remote Sens. Environ.*, vol. 111, no. 2-3, pp. 160-171, 2007.

Specific comments

Page and line numbers refer here to the printer-friendly version of the article.

Abstract

line 5: “A field campaign was conducted on landfast first-year sea ice in the Canadian Arctic Archipelago during the summer of 2012...”

Mention that sea ice here is undeformed here, as you do in Section 3.1.

Introduction

p. 808, l. 15: However, ubiquitous cloud cover over the Arctic during summer prevents the application of this approach on time scales commensurate with intra-seasonal pond fraction variations.

Rösel and Kaleschke (2012) used 8-day composite MODIS in their melt pond study, and studied melt pond fraction evolution. Do you think that 8-day interval is too long? If so, when especially, at melt ponding start, peak of fraction etc.?

References Jagdhuber et al., Scheuchl et al. are missing. Check all your references!

Physical model

p. 812, l. 23: The asymmetry effect on single-polarisation backscatter from ponds is expected to be consistent with that of ocean waves, the behaviour of polarisation ratios requires analysis.

Are there really asymmetric waves in the melt ponds? I would guess there are no swell waves in ponds, just kind of small scale isotropic ‘ripples’.

p. 813, l. 19: (2) Scharien et al. (2012) found that the C-band linear polarisation backscatter and PR levels were similar for snow compared to bare FY ice during the ponding period.

I would guess this is the case for undeformed FY ice, over ridges and rubble fields wet snow will highly decrease backscattering. Mention degree of FY deformation here.

Data collection

Short description of aerial surveys and collected data would be good, or put reference to Part II paper.

Cscat (text and Table 1) what is the absolute accuracy of the σ_0 ? How small measured σ_0 changes are reliable (i.e. radiometric resolution), you can calculate this from the number of independent samples?

Data analysis

p. 818, l. 3: Explain the Rose criterion, with references if needed.

Evolution of sea ice cover

Melt onset

Figure 2 is not necessary, no trends in the s data, so you can just explain results in the text.

You could use mm unit for s.

In Section 3.2 you mentioned that also correlation length was estimated, why its values not discussed here?

Did you study the shape of the autocorrelation function?

Melt ponds

p. 820, l. 5: A U10 threshold of 6.4ms^{-1} , beyond which the PR is expected to be dependent on surface roughness, is derived from Fig. 4.

With the IEM model you could study the U10 threshold better, see the figure in my comments to your Paper II.

Microwave backscatter from pond-covered sea ice

Polarisation ratios

Here you present PR vs. incidence angle fit to your data, regardless of wind speed and wind orientation. In late Sections their effect is discussed, but not in the context that support equation (4). There should be discussion that support (4), maybe have Sections 4.2.2 and 4.2.3 before introduction of (4)?

Could you give some figures on statistical reliability of the PR fit in equation (4)?

Does a PR vs. incidence angle fit to the Bragg model also follow a second degree polynomial?

p. 821, lines 1-16: This make again use of the Bragg model questionable...

p. 822, l. 15: ... HV due to the absence of depolarisation effects.

On what basis you state this? What is the level of cross-pol σ_0 , it is close to noise floor? PRx around -10 dB is not that small, it is typical for deformed sea ice.

Upwind to crosswind ratios

Surface wave amplitude profiles in Figure 9 are not very informative, but wave spectra would be, can you calculate that? If not, remove the amplitude profile sub-figure.

Discussion

p. 826, l. 15: Finally, the formation of an ice lid on pond surfaces leads to a reduction in PR to levels similar to FY ice.

I guess it again undeformed FY here, say it so. Otherwise this Section has good discussion on the results and limits of your PR-Bragg method.

Conclusions

p. 826, l. 25: again say that it is undeformed FY

p. 828: Sentinel-1 does not have HH-VV imaging mode needed in your method.

Technical corrections

Fig. 2. The four sub-figures are very small, especially hard to see anything in the surface elevation map, which is not informative as it is a random sample, but the surface spectra would be.