Dear Anonymous Reviewer #3,

Thank you for your thorough review comments on both Parts of this paper. Where appropriate comments have been considered as they may apply to both papers.

General comments

1. *Regarding the long introduction sections.*

   The introduction sections in the final versions of Parts I and II will reflect the decision of the editor as suggested.

2. *Regarding the term "dual-polarisation C-band SAR" in the title.*

   As is consistent with our response to a similar comment by the reviewer regarding Part II, we are suggesting that the title not be changed to include "HH/VV". There is emphasis placed on the VV/HH ratio due to its utility for pond detection, and it is somewhat agreed that the community may be more likely to infer HH/HV from the term "dual-polarisation" in the title. However Parts I and II contain new results pertaining to HV/HH, as well as individual channels VV, HH, and HV (Figures 6-10 in Part I), and to state one polarisation combination in the title is to potentially exclude the others from the attention of the reader. As per our response to this issue in the review of Part II, we are revising the Abstract so that the reader can easily evaluate the paper's contributions in relation to their *a priori* interests.

3. *Regarding surface roughness measurements and the Bragg scattering model*.

   Concerns regarding the LiDAR and ultrasonic distance sensors and adjustments to surface roughness retrievals are addressed in the response to anonymous reviewer (RC 37 Review Regarding Field use of LiDAR).

   A discussion on nature of surface roughness of natural targets, like cultivated soil and sea ice, is included in the final version. Similarly a justification of the selection of scattering model, the IEM as also suggested (and in Part II), is included. Use of the IEM is further supported by previous work on sea ice which had shown that neither the SPM (Bragg) nor Kirchoff models are applicable for sea ice at C-band because sea ice falls within the intermediate roughness range, i.e. the rms-height is too large, or the correlation length too long, wither of those models respectively (Ulander et al. 1992).

   Further to the treatment of surface roughness, we originally used the Bragg model to assess the PR behaviour of the individual targets (ice/pond) and to provide a basis for the extension of the PR approach to pond fraction observations in SAR imagery. This follows the ocean literature where the Bragg model is used to as the basis for empirical formulations (Zhang et al. 2011). In this study the surface roughness data was used as means to answer the question of whether or not the surface was within the Bragg scattering regime, which it was shown to be except during period of high wind stress. Our experimental design did not require us to attempt to match modeled and measured backscatter coefficient, or to account for stationary and non-stationary roughness characteristics. However since we agree that the IEM is better and will improve our understanding of the role of wind stress and wave roughness on backscatter, the revised paper will include justification for the final treatment of surface roughness behaviour pending changes in the method to address the fractal-like nature and interdependence of surface roughness parameters following Church (1988) and Manninen (1997). Given our decision to use the IEM, and that the *D*-dependence of linear sigma-0 cancels out, the fractal-SPM and references therein are noted but not further considered.

Church, E. L. (1988), Fractal surface finish, *Applied Optics*, *27*(8), pp. 1518-1526.

Manninen, T. (1997), Surface roughness of Baltic Sea ice, *J. Geophys. Res.*, *102*(C1), pp. 1119-1139.

Ulander, L. M. H., R. Johansson, and J. Askne (1992), C-band radar backscatter of Baltic sea ice: Theoretical predictions compared with calibrated SAR measurements, *Int. J. Remote Sens.*, *13*(13), 2447-2468.

Zhang, B., W. Perrie, and Y. He (2011), Wind speed retrieval from RADARSAT-2 quad-polarization images using a new polarization ratio model, *J. Geophys. Res.*, *116*, C08008, doi:10.1029/2010JC006522.

4. *Regarding PR-albedo versus PR-pond fraction relationships.*

   In our response to the same query in Paper II we noted that the PR is more suited to pond fraction detection rather than albedo as the PR response is related to the presence/absence of ponds and not variations in pond albedo.

5. *Regarding the Scharien et al. 2007 study.*

   This work has been added to the introduction. As to the switch to PR vs. pond fraction, see the above response to #5. It can be said the switch is based on experience gained since the last study, and the application of that experience to the improved experimental design used in this study.

<u>Specific comments</u>

Abstract

L5: added.

Introduction

P808/L15: Published in situ observations of pond fraction on FYI and MYI, summarized in Fig. 1 of Polashenski et al. (2012), suggest that the 8-day interval is too long when FYI is considered during all phases of ponding, or when FYI and MYI are both considered at the start of ponding. An 8-day interval likely will not capture the pond fraction signal in these situations since large fluctuations (several %) are known to occur during a diurnal period. During early ponding this is due to competing melt water inflows and outflows, with melt water inflow driven by the rapidly decaying snow cover and outflows by factors affecting lateral transport of water over the impermeable ice cover (Eicken et al., 2002). While MYI pond fraction stabilizes in subsequent phases as the melt water settles in the topography, the FYI pond fraction varies on smaller than 8-day intervals as horizontal and vertical pathways evolve in the decaying and weakening ice cover. Using an 8-day composite to e.g. determine ice albedo, estimate a radiation budget, assess feedbacks, or make inter-annual comparisons should be done with an appreciation for the limitations imposed by the coarse sampling interval of such a dynamic and variable feature. Of course that is not to say the 8-day composite product does not represent a valuable contribution to the field and an important source of insight into large-scale variability of pond fraction.

Eicken, H., H. R. Krouse, D. Kadko, and D. K. Perovich (2002), Tracer studies of pathways and rates of meltwater transport through Arctic summer sea ice, *J. Geophys. Res., 107*(C10), 8046, doi:10.1029/2000JC000583.

Polashenski, C., D. Perovich, and Z. Courville (2012), The mechanisms of sea ice melt pond formation and evolution, *J. Geophys. Res., 117,* C01001, doi:10.1029/2011JC007231.

References for Jagdhuber et al., Scheuchl et al. were added. Thank you.

Physical model

P811/L23: Here we are referring to asymmetric backscatter response due to the orientation of the radar beam relative to the sea surface (and wind vector), and not asymmetry specifically due to wave shape. It is described by the mathematical function (Ulaby et al. 1986):

$$\sigma° = A + Bcos(\varphi - \varphi_R) + Ccos2(\varphi - \varphi_R)$$

where $\sigma°$ is the normalized radar cross-section, φ is the azimuth look angle relative to some reference direction, and φ_R the wind ripple orientation relative the reference direction, and A, B, and C are coefficients for mean backscatter, upwind/downwind variation (if present), and asymmetry due to ripples. So wave shape is considered though not exclusively. Having said this we do expect some difference in shape between the leading and trailing edges of the small scale wind driven waves, i.e. parasitic capillary waves on the forward face (Garbe et al. (eds.), 2007). Agreed: there are no swell waves in ponds.

Garbe, C.S., R.A. Handler, B. Jahne (eds.) (2007), Transport at the Air Sea Interface, pp. 159-168, Springer-Verlag Berlin, Heidelberg.

Ulaby, F.T., Moore, R.K. & Fung, A.K. (1986). Microwave Remote Sensing: Active and Passive. From Theory to Applications, Vol. III, Artech House, Inc., Norwood, Massachusetts.

P813/L19: we will specify undeformed sea ice; agreed that wet snow decreased backscatter over ridges and rubble.

Data collection

Description of aerial survey and collected data added.

Cscat accuracy and resolution is detailed in Geldsetzer et al. (2007), a paper dedicated to this system and its use on sea ice. We only refer the reader to the paper, however given the importance of accuracy and resolution these will be described in more detail in the revised manuscript as suggested.

Geldsetzer, T., Mead, J.B., Yackel, J.J., Scharien, R.K., & Howell, S.E.L. (2007), Surface-based polarimetric C-band scatterometer for field measurements of sea ice, *IEEE Trans. Geosci. Remote Sens.*, 45(11), 3405 – 3416.

Data analysis

P818/L3: Rose criterion states that a detectable signal should be at least 5 times as strong as the background noise level. Explanation and reference added.

Evolution of sea ice cover
Melt onset

Figure 2, the lidar map, has been removed. Agreed: it is not useful as demonstrated.

s is now expressed in mm units.

Yes, correlation length estimates were made and the shape of autocorrelation functions tested – and shown to be close to the exponential. The revised paper will include these details as applicable to the IEM backscatter model and related concerns made by the reviewer above in general comments section.

Melt ponds

P820/L5: We have considered the comments here, and those made to Paper II, and agree that a better and more rigorous analysis of backscatter and wind speed/pond roughness dependencies can be made through use of the IEM model. The revised paper will include this suggested analysis, i.e. we will address the question of how much the PR is affected by roughness beyond the wind speed threshold.

Microwave backscatter from pond-covered sea ice
Polarisation ratios

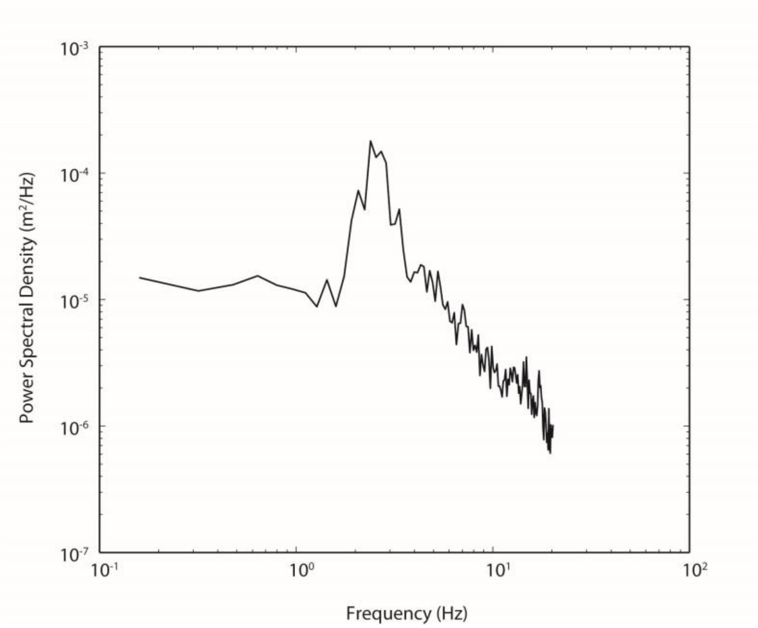
Agreed that the PR vs. incidence angle fit should include supporting context with regards to wind speed and azimuth look angle. The sections have been re-arranged and edited accordingly for the final version. The final version will include details on the statistical reliability of eq (4). We are not concerned with the PR fit to the Bragg model since we are using the IEM model now.

P821/L1-16: Agreed that the Bragg model is inappropriate, though we still find it a noteworthy result that our PR measurements from very calm conditions, expected to be well within the Bragg smooth surface limit, fall so far below PR estimated from the Bragg model. Thompson et al. (1998) suggest a deficiency in the Bragg scattering model, model estimates of the HH polarisation lower than observed (PR is higher than observed), and suggest empirical fit to observed PR data as being more robust. So, yes, the Bragg model is in appropriate. See comments regarding the use of the IEM model instead.

P822/L15: Yes, the cross-pol sigma is near to the noise floor (see Fig. 7). The large PRx at large incidence angle from water is due to a greater reduction in HH relative to HV in a single scattering environment (no wave breaking or foam).

Upwind to crosswind ratios

Agreed that surface wave spectra are more informative and yes, they can be calculated. See the example below, which is the wave-height spectrum for melt ponds as derived from 64 wind-wave periodograms across the 2.0 to 11.4 m s^{-1} wind speed range. This is an example only: a revised version of this plot and appropriate description of the data will be included in the revised version of the paper. Consideration is also being given to resolving issues regarding the ultrasonic wave height measurement technique provided by another reviewer. As suggested the surface wave amplitude profiles will be removed from Fig. 9.



Discussion

P826/L15: added.

P864/L18: Yes, this is an abbreviation. It’s now correctly defined in Section 3.3. The same was done for the Bragg and cross-validation models for consistency.

Conclusions

P826/L25: added.

P828: Agreed that HH+VV does not apply to Sentinel-1; however the paragraph discusses the need to further advance the use of SAR for sea ice process and modeling studies regardless of the polarisation specific information addressed in the paper. However the relevant polarisation characteristics of the missions have been added so as to not mislead the reader as to the relevance of Sentinel to the main results.

Technical Corrections

Fig.2: the elevation map has been removed. Another reviewer pointed this out as well. Surface spectra data have been added where appropriate.