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The Cryosphere Discussions

Interactive comment on "Sea ice local surface topography from single-pass satellite InSAR measurements: a feasibility study" *by* Wolfgang Dierking et al.

Anonymous Referee #1

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This paper provides a comprehensive study of the application of spaceborne cross-track SAR interferometry for the measurement of sea ice topography. The paper anticipates two satellites flying in some form of tandem orbit, and examines both the geometric stereo effect, and the complication of the short time delay between the two image acquisitions. Results are given for potential tandem missions in four frequency bands; L, C, Ku and Ka, reflecting some previous feasibility studies for spaceborne missions at these frequencies. System (noise-equivalent $\sigma 0$ (NESZ), incidence angle), orbital (normal and along-track baselines), and environmental ($\sigma 0$, penetration and associated volume decorrelation, ice motion, etc.) factors are considered in the analysis. The results are of interest to the sea ice remote sensing community, and the paper

will be a key resource in the evaluation of future tandem InSAR missions which might include sea ice topography as a potential application. I am happy to endorse this paper for inclusion in The Cryosphere but I would like the authors to consider the comments below.

General comments.

1. Would it be possible to measure wave parameters, in particular height, when ocean swell propagates into the pack ice in the marginal ice zone with any of the proposed configurations?

2. Although there is no 'ground truth' for the two examples of ice 'topography' derived from the TanDEM-X, the results in section 4 are still of significant interest and, I think, this section could be improved.

a. the SAR image (Fig. 2b) should be resampled to ground range and the area for which the topography is shown outlined on the image.

b. The increasing azimuth and ground range directions should be marked.

c. The result of interest is the ice topography, so why not show the height directly as a colour coded DEM with a color-bar extending from -1 to 3 m in Fig. 2a? The shaded relief image is nice but not as 'informative' as a more direct illustration of the topography. You are allowed to remove tilts, if necessary!

d. There are areas in the SAR image (Fig. 2b) which suggest variable surface roughness, a profile through the very bright or dark regions would be of interest allowing a comparison between the image radiometry and the large scale roughness.

e. Figure 3a adds little to the science and, as presented, the 'sea ice' radar image in 3b also adds little. However, if the three images (Figs 2a, 2b and 3b) were resampled to ground range with the same scale, the comparison would be interesting. It should be possible to 'see' the same ridges in Fig. 2b and 3b.

f. Again, I would like the two parts of Fig. 4 to be resampled to ground range so that a direct comparison is possible.

g. As discussed in the text, Fig 4c is very revealing about the problem of line-of-sight motion even when the temporal baseline is 6 milliseconds. Maybe emphasize in the text that this problem is somewhat alleviated at longer wavelengths?

h. In Fig. 4 the ice at 'A' (0-400 m) and ~1000-5000 m in the profile is very bright in the SAR image but the height variation suggests that the roughness is relatively small scale. Also, there is a marked height change between the shore-fast ice (10000-12500 m) and the ice at 10000-5000 m. Can you comment on this observation?

3. In sections 5.2 to 5.4 the authors, quite legitimately, have concentrated on a quantitative examination of the 'penetration depth', d. In a couple of instances, the height error associated with penetration was estimated as ~ 0.5 d. I think that there should be a clear recognition of the fact that there need not be a simple relation between the penetration depth and the effective horizon in the ice from which the returns appear to be from. For example, at L-band the penetration depth could be significant in cold multiyear ice, but if the ice is relatively uniform in structure then the surface backscattering component could still dominate over the volume component and the effective backscatter horizon could be closer to the surface than 0.5 d. While this is acknowledged in the text I think it could be made clearer.

Some specific comments on the text...

P3L3: 'The length of the across-track baseline determines the sensitivity to height variations...'. Strictly speaking, this should be... 'The component of the across-track baseline perpendicular to the line-of-sight direction determines the sensitivity to height variations...'.

P4L29: 'to be considered: one the one hand'... presumably 'on the one hand'.

P5L10: 'no spectral shift filter is applied.'... Perhaps a suitable reference should be

added here, in case the reader is unaware of this step in some InSAR processing.

P10L10: 'In Figure 5, the "critical system-normalized" along-track baseline Baln = | pBal / vλ | is plotted... '. The trouble is that this has units of inverse velocity, not distance. Consequently, I think a better name for this could be 'critical inverse line-of-sight velocity'. Figure 5 would then need to have a different y axis label, although the units are correct, and some rethinking of the following text on page 10 might be necessary.

P10L16-18: Table 3 is referred to twice; this should be Table 5.

P13L29: temperature; missing r.

P14L19: 'elder' is not appropriate, in fact even 'older' is not strictly necessary.

P15L26: 'acceptabe', missing l.

P17L18; the sentence beginning 'This is not made subject of this study, since...', is not clear.

P18L17: 'Doppler shift', insert space.

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