Reviewer 1

Thank you very much for your constructive and encouraging comments.

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,
In fact, we used 5 frequency bands, besides the mentioned ones also X-band
reflecting some previous feasibility studies for spaceborne missions at these frequencies. System (noise-equivalent σ0 (NESZ), incidence angle), orbital (normal and along-track baselines), and environmental (σ0, 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?
We think that this questions needs not to be directly addressed in the paper. Since infra-gravity waves with amplitudes of a few millimeters can be recognized in interferograms (see Mahoney et al., Geophys. Res.Lett. 43, 6383-6392, 2016), it may also be possible to measure swell parameters in the marginal ice zone. However, for a comprehensive answer to this question one needs to consider different aspects such as for example: How large is the interferometric decorrelation in the marginal ice zone under different ice and meteorological conditions? Are we talking about a closed sea ice cover? Does the sea ice cover behave like an elastic medium? Is it broken? What time difference between the two images forming the interferometric pair is optimal? Hence we can not provide a simple answer at this point.

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.
The SAR image was resampled. The SAR image and the topographic map now show the same area.
b. The increasing azimuth and ground range directions should be marked.
We added this information.
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!
We provided a color-coded DEM but with a different scale to preserve also the information on height variations on land (after some experiments with different color scales).
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.
The radar brightness is not directly related to the (meter-scale) surface topography. We addressed this item in the third paragraph of section 4.
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.
Unfortunately in the framework of this study it is too time-consuming to find a corresponding match between Figs. 2 and an image of the coastal radar and present them at the same projection. Nevertheless we would like to keep Figure 3 since it is instructive for readers not familiar with sea ice conditions.
f. Again, I would like the two parts of Fig. 4 to be resampled to ground range so that a direct comparison is possible.
Please see answer to comment 2a.
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?

We are not sure in what sense the "problem" is alleviated ? In fact, at L-band, the decorrelation time is larger by a factor of 10 compared to X-band. This means that at L-band the phase differences caused by surface water currents can be measured even at larger temporal separations between the two images forming the interferometric pairs than at X-band. We mention this point in the discussion of the second example, last paragraph of section 2.

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.

Please see answer to comment 2d.

Also, there is a marked height change between the shore-fast ice (10000-12500m) and the ice at 10000-5000 m. Can you comment on this observation?

The area between 5000 and 10000 m is not ice but open water, and the apparent height change is most probably due to the influence of surface currents. We discuss this in the last paragraph of section 4.

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.

We rewrote the last paragraph of section 5.3 to emphasize the uncertainties one has to face when estimating the elevation bias and retrieval error caused by varying penetration depths and volume decorrelations.

Some specific comments on the text

We considered all comments below – the corresponding changes are marked in the manuscript.

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. Reference was added

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.

Very good point! Thank you very much for calling our attention to this inconsistency. We separated baseline and the "system coefficient" p/vλ and modified the discussion accordingly, see third paragraph in section 5.1.

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.

We rephrased this sentence.

P18L17: 'Doppler shift', insert space.