Reviewer 2

Thank you very much for your constructive and encouraging comments.

Summary
This manuscript presents what I believe may be the most comprehensive review of the application of InSAR to the study of sea ice. The authors focus their attention on the use of single-pass InSAR for the measurement of sea ice topography, but their discussion addresses many aspects that are relevant for other sea ice applications of InSAR such as the detection of motion or deformation. These include the physical constraints on useful baselines, incidence angles and radar wavelengths for deriving useful estimates of sea ice surface height. The authors also consider the influence of sea ice type, surface roughness and snow depth on the accuracy of these measurements. In addition to discussing the potential opportunities for InSAR-derived ice topography measurements from existing and future SAR constellations, the manuscript also presents topographic results derived from a bistatic InSAR acquisition over sea ice near Point Barrow, Alaska. Overall, the manuscript is well written and is likely to be an important contribution to the sea ice InSAR literature, particularly as we enter a new era of publicly available data from a growing number of SAR constellations. I have some minor comments about the discussion of elevation results over thin and drifting, which I describe in more detail below, but I believe these should be relatively easy to address.

General comments
1. Clarification of phase interpretation over young and drifting ice
In Figure 4a, the authors present surface elevation derived from phase variations over sea ice near Point Barrow. The accompanying SAR amplitude image (Fig 4b) shows a region of landfast ice attached to the coast and separated from drifting sea ice by a lead, which contains bands of frazil. For readers unfamiliar with SAR images of sea ice, it might be helpful to label these features in the amplitude image. In the text, the authors note the "non-negligible height offset" due to surface motion that occurred during the 6 ms temporal baseline between image acquisitions. I recognize that the focus of the paper is on the retrieval of sea ice topography, but this is an interesting and important result that I think would be worth discussing further. For example, in section 5.1 the authors could apply equation 9 to derive the look direction component of ice velocity. This could even be validated using ice velocity measurements from the Barrow sea ice radar (referenced in Fig 3) or from an oceanographic mooring located in the vicinity of point A in Fig 4(doi:10.18739/A2MT1D).
We agree that the effect of open water surface currents and of sea ice drift on the interferometric phase is an interesting topic that deserves more detailed research. However, in the context of this paper, and considering the lack of complementary information we decided not to include a discussion concerning the derivation of the line-of-sight velocity field over the open water areas and the drift speed of the ice. The spatial variation of open water surface currents influenced by Langmuir circulation is rather complex. We provide a short description of Langmuir circulation in the second example, last paragraph of section 4. We also calculated ice drift assuming that the elevation difference between the landfast ice (10000-14000) and the drifting ice (1000-5000) is caused by the movement of the latter. However, the resulting velocities are rather large and seem unrealistic. The text for example 2 in section 4 was completely modified.
Also, the authors drawn attention to the apparent roughness of the surface of the lead, which they attribute to "alternating water and frazil stripes", but I feel this explanation could be expanded. The preceding text discusses the phase contribution due to surface currents parallel to the look direction and also makes references to Langmuir circulation, but it is left to the reader to connect the dots. For readers not familiar with slant-range geometries or Langmuir circulation I would suggest the following changes:
i) label the look direction on Fig 4a and b (see also my note about Fig's 2 and 4 below)
On the new versions of Fig. 2 and Fig. 4, arrows indicate azimuth and range direction.
ii) indicate the wind direction, which can be estimated based on the orientation of the frazil bands
We mention in the new text that the streaks are parallel to the wind direction.
iii) state or illustrated how surface currents might vary according to Langmuir circulation.
The text of the second example was extended to provide some information about surface currents formed and influenced by Langmuir circulation (see above).

Specific comments
P1, Line 24:
I would not describe the change in ice surface topography as "steady". Perhaps "near-constant" would be a better phrase.
We used the notation "intermittent".
P2, Line 2:

I feel that “surface roughness” might be amore appropriate term than “surface height variations”
We used “surface height variations” on purpose to make clear that here we focus on elevation changes on the order of decimeters to meters. “Surface roughness” includes the small-scale roughness in the mm- to cm-range. This is now explained explicitly in the text.
P3, Lines 31-32:
I suggest replacing "neither" with "not" and beginning the sentence with "Also" so that it reads: “Also, if the alongtrack baseline is zero, the interferometric phase is not affected by ice drift”.
We rephrased the sentence completely.
P8, Line 3:
Note that as of December 1, 2016, the city of Barrow changed its name to UtqiaÄ¸avik. I suggest replacing all references to city of Barrow with its new name and adding “(formerly known as Barrow)” after the first instance. Note that Point Barrow has not changed its name.
In the text we give a hint to the new name.
P8, Lines 16-17:
Actually, independent measurements of sea ice topography were made for a region of landfast sea ice within the coverage of this DEM. These data are presented in an article by D.O. Dammann recently submitted to the IEEE Journal of Selected Topics in Applied Earth Observations and Remote Sensing.
Thank you for this information. As additional information: At the University of Tromsø one of the authors (Dierking) is involved in a study in which retrievals of sea ice elevation from interferometric data are compared to data from a scanning laser and stereo photography.
P11, Line 15:
It should be noted that surface wave amplitudes can be much greater in the marginal ice zone and so this statement should be qualified by noting that infra-gravity waves can be neglected in the central ice pack.
We noted it.
P11, Line 27: According to the stated definition, I believe the authors mean “average height of each sail”, rather than “average sail height”
We took the notation “average sail height” from the paper by Strub-Klein and Sudom, but we now specified it as suggested.
P12, Line 1: I realize that this is partly a matter of style, but I recommend the authors read the following short article on parentheses use:
Robock, A. (2010), Parentheses are (are not) for references and clarification (saving Space), Eos Trans. AGU, 91(45), 419–419, doi:10.1029/2010EO450004.
Thanks for this hint. We considered it in some but not in all cases. Sometimes the use of parentheses is simply practical…
P12, Lines 1-2: I do not feel this statement is correct in the case where ridges are larger than the resolution cell of the SAR data. While the elevation of the cells near the peak of the sail might be higher than the average height of the overall sail, the cells on the flanks of the sail will have lower-than-average elevations.
Very good point! We modified the text accordingly.
P12, Lines 16-18:
Could the authors please provide a citation for these penetration depths?
All are from Lewis et al. (1987), which is now clarified in the text.
P13, Line18:
Should this be “2.5” instead of “2-5”?
Corrected.
P14, Line 19:
Correct “elder” to “older”
Following the suggestion of reviewer 1, we don’t use “elder” or “older”.
Figure 2:
It would help the reader interpret these results if the data in panels (a) and (b) could be presented in the same projection.
In the new Figs. 2 and 4 the projections of the topographic map and the SAR image are the same.
Figure 4:
As with Figure 2, it would be helpful if the data in panels (a) and (b) could be presented in the same projection. It might also help if there were tick marks at key intervals along the AB transect, with corresponding marks on panel (c).
Tick marks used in (c) are shown on the profile lines in (a).