Review of „Instantaneous sea ice drift speed from TanDEM-X interferometry"
by D. O. Dammann and 7 co-authors, revised manuscript

General assessment: The major concern of my first review was the authors' judgment regarding the benefit of retrieving sea ice drift from S-ATI data for different applications such as marine operations and safety in sea-ice covered waters or sea ice science. The authors addressed my criticism in depth. The result is that it is now much easier for the reader to understand pros and cons of this technique. Hence I recommend the paper for publication but propose a few minor modifications.

Abstract: lines 14-15 "…S-ATI as a tool to assess ice drift, inherent limitations, and possible applications": this can also be read as "S-ATI as a tool to assess inherent limitations and possible applications". Should be rephrased.

Page 2, line 10: the Muckenhuber and Sandven reference is out of place here, the application of SAR for retrieving ice drift is described in many other papers as well – I think a reference is not needed here

Page 2, line 16: I propose to add two useful papers here:
A. A. Korosov and P. Rampal, A combination of feature tracking and pattern matching with optimal paramtrization for sea ice drift retrieval from SAR data, Remote Sensing 9, 258, 2017, doi:10.3390/rs9030258
T. Hollands, W. Dierking, "Performance of a multi-scale correlation algorithm for the estimation of sea ice drift from SAR images: initial results", *Annals of Glaciology* 52(57), pp. 311-317, 2011

Page 2, line 18: the error of the retrieved ice drift does not only depend on the complexity of the ice drift patterns but also on the spatial resolution of the SAR images and the temporal gap between the image pair from which ice displacements are retrieved. However, Hutchings et al. did not use SAR images for their analysis, and I guess this is also valid for the Haller paper? Hence the hints to those studies are misleading here.

Page 2, line 21: "Instantaneous drift estimates…supplement…traditional SAR-based ice drift algorithms for improved accuracy". I agree that both methods can supplement each other, but I don't see how S-ATI results can be used directly to improve the accuracy of the traditional methods.

Page 2, line 31: another useful paper regarding retrieval of sea ice topography on landfast ice is:
T. G. Yitayew , W Dierking, D. V. Divine, T. Eltoft, L. Ferro-Famil, A. Rösel, and J. Negrel, "Validation of sea-ice topographic heights derived from TanDEM-X interferometric SAR data with results from laser profiler and photogrammetry , *IEEE Trans. Geosci. Remote Sens.*, vol. 56, no. 11, pp. 6504-6520, 2018, doi:10.1109/TGRS.2018.2839590

Page 3, lines 25-26 "errors in the dislocation vectors" - do the given numbers refer to the magnitude of the vector? Or are they valid both for magnitude and direction (orientation)?

Page 4, line 2: "accuracy of BETTER than 0.01 ms^{-1}"?

Page 5, line 9 (above equation 2): since your displacement of ambiguity is given for ground range, you should make clear that also your v_ϕ is the *ground* speed in look-direction (most equations in the literature give the line-of-sight velocity since the vertical component of ground movement is not always zero).

Page 5, line 22: "sections of open water": the speed of Bragg waves is sensitive to short-scale wind changes, hence the motion of the sea surface may reveal larger variations, see e.g. the lead in Fig 4a in the Dierking et al 2017 TC paper (where motion is interpreted as height).

Page 6, line 4 "…the average phase of the flow can be used to describe the linear motion". Actually this is not valid. Even after phase unwrapping the phase is ambiguous, and ice floes are not necessarily symmetric, which is one assumption in the Scheiber-paper. Did you observe indications of rotation in your data? If not, you should mention it.

Page 7, lines 31-32: Rephrasing: "Another potential reason for an increasing speed with increasing distance from the shore would be a larger concentration of…"

Page 8, line 15: there is one v_ϕ too much.

Page 9, line 2 "…phase can be used to accurately derive ice…"?

Page 11, lines 21-28: One should mention that the consideration of the topographic phase is necessary here because the perpendicular baseline is > 0.

Page 11, line 28: In Fig. 4a there seem to be no indications of icebergs at positions of bright spots that occur in Fig. 4b. Are there other explanations for the outliers?

Page 11, lines 32-33, page 12 lines 1-2: The first sentence is somewhat oddly phrased. Suggestion: "Hence the question is how large the acceptable length of the normal baseline B_n is so that the effect of topography (quantified by the maximum height) corresponds to the effect of phase noise on the phase derived speed. By combining equations (3) and (6) and setting $v_e = \sigma_v$ we can determine the baseline B_n for a given height h_0. Assuming…" or something similar…