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Comment on tc-2021-18

Anonymous Referee #2

Referee comment on "InSAR monitoring of Arctic land fast sea ice deformation using L-band ALOS-2, C-band Radarsat-2 and Sentinel-1" by Zhaohua Chen et al., The Cryosphere Discuss., <https://doi.org/10.5194/tc-2021-18-RC2>, 2021

Review of:

InSAR monitoring of Arctic land fast sea ice deformation using L-band ALOS-2, C-band Radarsat-1 and Sentinel-1 by Z. Chen et al.

The topic of this paper is interesting and deserves attention. Unfortunately, the specific motivation and objective of this study is not clearly explained, the description of the approach is not sufficiently detailed, and the most important results appear to be not plausible. Therefore I cannot recommend the paper for publication.

(1) Objective: At the end of the introduction, the authors explain that they study deformation derived from InSAR measurements and compare it with surface height measured from a laser altimeter and ice draft data obtained from an ice profiler. They also study the sensitivity of SAR backscatter to sea ice surface height changes. In the conclusions, the authors mention that they investigated the potential of InSAR for observing land fast ice deformation in the Arctic Ocean. But why? And which type (s) of deformation? (See below.) The investigation is focussed on a local area of fast ice, hence results cannot be directly applied to the entire Arctic as implied by the authors in some of their statements (e.g. drifting pack ice - and are fast ice conditions at different regions always similar?). The direct relation to climate research is also unclear.

(2) The SBAS method is usually applied to land surfaces. It is not well known in the sea ice community and needs to be explained more in detail in the method section. The major idea is to reduce the effect of topography (at vertical scales of meters) and also of the atmosphere from the measured InSAR phase and thus obtain LoS surface movement at mm-cm scale. However, on sea ice, topography is caused, e.g. by edges of ice floes and ridges, and height variations rarely exceed a meter (only in case of large ridges). Hence, the effect of sea ice topography may be negligible over the fast ice area investigated

here?

(3) One part of the paper deals with relating radar backscatter at different frequencies to surface height obtained from ICESat-2 data (Sec. 4.1., Table 2, Figs.4,6,8). The text consists mainly of description of Figs. 4,6,8 - which could be shortened and be more focused on specific, unusual/unexpected details, since the reader can have a look at the figures. What is the effective spatial resolution of surface height variations (ICESat: 17 m footprint but 0.7 m sampling according to lines 156-157)? Table 2 lists 27 "NA" but only 18 correlation coefficients from which only 7 are ≥ 0.6 . From this result my conclusion is that it is not possible to retrieve ice surface height from SAR backscatter data with sufficient accuracy and robustness. By the way, a figure showing surface height versus backscatter intensity would be more useful for judging the possibility to establish a relationship between both.

This investigation is very much related to linking SAR backscatter with ice thickness. Until now, a sufficient correlation between both parameters was only found for the Sea of Okhotsk. The hypothesis in studies focussing on the Sea of Okhotsk was that in the seasonal ice zone (where the measurements took place), the ice surface roughness - to which in particular L-band radar is sensitive - is related to ice thickness. In fact, measurements showed a moderate correlation between roughness and thickness for the seasonal ice zone in the Sea of Okhotsk. In regions of the Arctic, investigations found only low correlation coefficients between backscatter and thickness. I recommend that the authors check the literature for studies relating backscatter and sea ice thickness. One important reason for the in general low correlation between ice thickness and radar backscatter is that small-scale properties (e.g. ice surface roughness at cm-scale and air bubbles of cm size in the ice volume, snow crusts due to brine wicking) have a strong - often even a dominant - influence on the intensity of the radar signal. But their spatial variations are not related to ice thickness.

(4) Section 4.2 provides information on the change of interferometric coherence as a function of the time interval between the two images used for generating the interferogram. Although it is not surprising that larger time gaps cause lower coherence, it is interesting to get actual numbers of the coherence - which should be summarized in a table, together with a description of ice conditions.

(5) I found the investigations on the deformation of the fast ice using the InSAR technique irritating and not satisfying. In the introduction, the authors assume that the vertical deformation is the dominant movement due to changes of ice thickness, since wind and ocean currents will have no influence on the fast ice movement. However, InSAR is sensitive to mm-cm ice movements and hence, also small horizontal deformation caused by internal ice stress is visible in the InSAR phase (the authors have several articles in the reference list dealing with this). In section 4.4 and on lines 429-436 (Discussion) the authors correctly state that the measured LoS represents both horizontal and vertical movement, but they don't describe in the method section how they separate both components. I don't think that it is possible to relate the shape of the fringes to the direction of movement (vertical or horizontal) - as discussed in section 5 - without simulations of deformation patterns and corresponding synthetic interferograms (see e.g. Dammann 2016 in the reference list, who did this for different types of horizontal

movements). Finally, I think that results shown in Fig. 9 are wrong. The numbers are partly extremely high (considering an ice thickness of >120 cm at the end of March, Fig. 2), which may indicate that there actually also horizontal movements have to be taken into account. Also the difference between results from ALOS and Sentinel-1 is partly very large, which is not explained in section 4.3. Shouldn't at least the signs of movement be the same?

There are more shortcomings which I will not comment in detail. In conclusion, I recommend to submit a new paper with a focus only on the InSAR part, a thorough separation between vertical and possible horizontal movements (or provision of evidence that there is no horizontal deformation), and a careful check of the results of the deformation analysis.