The Cryosphere Discussions

The Cryosphere Discuss., referee comment RC3
https://doi.org/10.5194/tc-2021-359-RC3, 2022

Comment on tc-2021-359

Anonymous Referee #3

Referee comment on "Snow water equivalent change mapping from slope-correlated synthetic aperture radar interferometry (InSAR) phase variations" by Jayson Eppler et al., The Cryosphere Discuss., https://doi.org/10.5194/tc-2021-359-RC3, 2022

General comments:

The manuscript entitled “Snow water equivalent change mapping from slope correlated InSAR phase variations” presents a novel method for the estimation of SWE change in dry snow conditions between repeat acquisitions using repeat-pass SAR interferometry, demonstrated using a RADARSAT-2 dataset (5.405 GHz) focusing on the region surrounding Inuvik, NT, Canada. This method leverages topographic variation and avoids the problem of phase unwrapping which has challenged previous InSAR studies.

The manuscript is generally well-written, and the methods appear sound. There are some issues with clarity throughout, especially with respect to the introduction, definition and use of symbols and expressions. Similarly, some of the figures appear too small and hard to read. Finally, stronger support from references is needed to improve the manuscript and provide context for the study, especially in the introduction.

I will provide general questions, specific questions by line number, and technical corrections in (mostly) chronological order, in the following sections.

General Questions:

- Why did you choose the Inuvik area for this study? This wasn’t addressed in the manuscript. You mentioned on Line 749 that you expect SlopeVar to perform better in high-relief areas and in areas where SWE > 150 mm, so it seems there must be more appropriate regions for this study given its reliance on topographic variation.
- Did you provide a discussion about where this method should be used, in geographic

terms? Pan arctic? subarctic? alpine? I think you mentioned it should be used in areas with topographic features, but in terms of the Canadian landscape, where would this work or not work?

Specific Questions:

- Line 13 – include the frequency, even if its in parenthesis.
- Introduction – You need to mention the study site and explain why you chose it. What is the significance of this site and why did you not choose other sites with more topographic variation?
- Lines 26 – 28 – references needed – provide references for each point mentioned. This is important since it is setting the context for your entire study and should not be neglected.
- Lines 28-29 – may be worth noting that SWE is a function of depth x density as it will help uninitiated readers link SWE with commonly measured parameters.
- Lines 30 to 34 – references needed – these are very dense sentences that are setting up the need for your study. Provide references for studies which demonstrate some of the challenges listed (eg. influence of topography, vegetation, and temporal bias).
- Line 39 – this is an awkward description: "Snow depth, used to infer SWE when integrated with snow density". Rephrase this in a more straightforward way.
- Line 52 and 61, – The term 'grain-size' is out-dated. The term 'microstructure' is preferred. Also, be specific and refer to it as 'snow microstructure'. This is important because further in the paper you refer to 'soil microstructure'. It will help avoid confusion.
- Line 55-56 - awkward phrasing – rephrase using common expected terms like 'backscatter'. Try eg '...interpreting variation in backscattered radiation following interaction with the snowpack.' It seems in this sentence you are trying to do two things: 1.explain how a SAR works and 2. explain how it is used for SWE estimation. In reality, you should only be explaining 2. If you want to explain how a SAR works (ie. it transmits, and then receives backscatter), then you should do it earlier on.
- Line 72-73 – confusing and awkwardly phrased. Also, I'm not sure what is meant by 'spatially inhomogeneous changes to snow distribution. Should be reworded in a more plain and straightforward manner. Suggest something like: "Decorrelation increases with liquid water content, changes in snow distribution, and volume scattering."
- Figure 1. Label Inuvik on each panel.
- Figure 1. Is panel (d) really necessary? Do we really care if an area is alluvial or colluvial? I would suggest at least reclassifying to reduce the number of classifications as it is hard to read and not really useful Likewise for panel (c) – there are too many classifications. It is too hard to read, and the additional classes don't add additional value.
- Line 112 – how often should a new DEM be generated for this method (ideally)? What are the implications for accuracy?
- Line 123 – Fig. 1c doesn't depict vegetation density, only distribution of the vegetation classes.
- Line 123 – You mention the upland area east of the delta. It would be helpful if this was delineated in Figure 1.
- Line 125 – 127 - What does 'extensively developed lands' mean? I would suggest using an estimate of developed area in sq km, instead. Hard to imagine Inuvik being described as extensively developed.
- Line 132 - does this pose a problem when using an older DEM (that could potentially become outdated by land deformation), or using your methods in general, in this area

and across much of the low arctic? Why or why not?

- Figure 2 - What is snowfall water equivalent? Or do you just mean SWE? This seems a strange metric and I don't know how it was calculated or what it means. Can't you just use snowfall amount? This is what your readers will expect. In order to convert to snowfall water equivalent, I presume you would need to know the density of the precipitating snow which sounds difficult. This seems too complicated when good old fashioned snowfall amount will do.
- Line 145 – "Surface and volume scattering occur at the air-snow interface and within the snowpack respectively, but for sufficiently dry-snow, it can be expected that these contributions will be much less that the primary ground-scattered return." Provide a reference.
- Line 146 – 148 and Figure 3 - It may be useful here to mention and label the wave front in this diagram as in Fig. 7 of Leinss et al (2015). This makes it more clear for the reader why you are considering the particular segment lengths in Figure 3.
- Line 153 – Please specify if you are using just the real portion of ε.
- Line 155 - Is this appropriate for the high-density wind slab often found around Inuvik up to 500 kg/m3? What type of snow did Leinss et al (2015) consider? What range of density? Leinss et al (2015) was conducted at FMI in a forest clearing - likely not much wind slab to be found there. This may be worth considering as a potential limitation. If you are going to use this assumption, you need to demonstrate how similar or different the snowpack observed in Leinss et al (2015) was to the snowpack surrounding Inuvik.
- Line 332 – There have been enough studies around Inuvik (ie. Trail Valley Creek) that you could have generated an average density from real data instead of just assuming a value. Why didn't you use the data available?
- Line 170 – Define what is meant by 'local' with respect to a local spatial region? Is there an associated scale? What would be an ideal scale? Why? Make sure to support with references.
- Line 180 – 'Sensitivity of the dry-snow phase' seems slightly awkward. In the caption of Figure 5, you call it dry-snow phase sensitivity. This seems slightly better. More to the point, you should use a consistent name for these variables throughout.
- Line 183-185, Eq (4) – With eq (4) and all of your equations, you introduce them inline, within a sentence. This gets confusing, especially within complicated sentences and makes it difficult for your reader to establish the name of each variable being calculated. I'm not quite sure what ξ is actually called. It would be helpful to include the variable symbol in brackets next to each variable name in a sentence. I strongly suggest you introduce each expression in a more straightforward way, for clarity, such as: 'then the spatially variable sensitivity of the dry-snow phase to a uniform SWE layer (ξ) can be computed as in Eq.(4).
- Line 182 – you tend to introduce variable symbols, but then continue to use their name instead of the symbol. An example here is snow density (ρ). You have already introduced this variable earlier on, and you throughout the paper you continue to refer to it as snow density despite introducing the symbol ρ. You do this with a number of other variables throughout. In doing so, there are also cases where you refer to a variable by slightly different names which gets confusing. Go through your entire paper and make sure you introduce variables once, and then refer to them by the symbol thereafter. This should irradicate instances where you use different names for the same variables. I will try to point out other cases of this, but it will not be an exhaustive list, so I will leave it to you to go through your entire paper.
- Line 183-184 – another case where you've introduced a variable and expression inline with the text. It is difficult to understand is Φ_s interferometric phase contribution, or topographic sensitivity? There should be no ambiguity here. This should be rewritten for improved clarity. Put the symbol in brackets next to the variable name in the sentence.
- Figure 4 - is this a realistic depiction of how snow accumulates on slopes? What about drifting and accumulation on the leeward side vs. windward side? does $\alpha_1=\alpha_3$? It's not apparent in the figure. I don't think Figure 4 is mentioned in the text. Please describe in-

text. Did you discuss the change in ξ from foreslope to backslope? It is depicted in Figure 4 and should probably be mentioned in the caption.

- Line 190 – You write: "According to Eq.(5), if the absolute unwrapped dry-snow phase, Φs, can be recovered, then the spatially varying SWE change can be directly estimated at the same spatial resolution as the interferogram." You have already introduced the variable Φ_s in the text surrounding Eq.5 so you should just be using the symbol here. Furthermore, I was confused about what you called Φ_s from your ambiguous description in the text surrounding Eq.5, but now I am even more confused because here you call it "absolute unwrapped dry-snow phase." This was certainly not what you called it earlier. This is similar to my comment about Line 182 which emphasizes the need for you to carefully review the manuscript for these ambiguities.
- Line 97 – Similar to my question #22. What is the size of 'local' estimation window to which you are referring? Quantify 'sufficiently large' or give some recommendation of appropriate size. The spatial window is also discussed on line 228. Be sure that your quantification of local window is appropriate for each use in the manuscript.
- Eq (9). Check that has been defined. It seems like you define it on Line 221, but it should be defined here.
- Line 217 – Provide an explanation and a physical basis for this assumption.
- Line 220 – you've already defined . Just use the symbol here. Note your description of on line 220 is slightly different from how you described it in Eq.(7). The definition on Line 220 seems clearer, so it should be adapted for use in the text describing Eq.(7).
- Line 221 – you should have already defined while introducing Eq.(9). You should just use the symbol here.
- Line 224 – You have already defined on line 210. Just use the symbol here. Note, your definition of on Line 224 is clearer and more straightforward than what you've written on Line 210. I suggest you replace the definition on 210 with that on Line 224.
- Line 228 – You have already defined . Just use the symbol here.
- Line 244 – what are the implications associated with ΔSWE = 28 mm?
- Figure 6 – Is 6d a histogram? it looks like a scatterplot. If it's a scatterplot, can you provide some statistics to quantify this association?
- FIgure 6 - Use consistent headings and symbols in the figures. Eg. if 6d is based on 6a and 6b, then use either the same symbol, or the same name. If the x axis of 6c is the same as the heading of 6a, then it should be written the same way (use either just the symbol, or the same name). Similarly, for the y-axis on 6c, if it is meant to be the same as the title of 6b, then write it the same way (use either centered phase OR uncorrected phase, but not both). Be consistent.
- FIgure 6 - IF the axes on 6c are not the same as 6a and 6b, why not? you indicate a correlation between 6a and 6b but then show us a different relationship in 6c in order to demonstrate the correlation? This should be changed to a scatterplot of 6a vs 6b.
- Figure 6 - It's hard to see a good correlation between 6a and 6b. in 6a I see two areas of high SWE sensitivity - yellow patches at mid-right and top right. In Fig 6b, these seem to match with a heavily speckled area (top right), and a largely pink area (mid-right). It seems maybe some inconsistency in SWE sensitivity. Can you quantify the correlation? It's not clear that it is a great correlation, and having it quantified would allow us to put appropriate weight to these results. I imagine that's why you chose not to say 'a strong correlation'
- Lines 255-256 – It would be useful to provide a little more information, even though you have provided a reference. Why do non-sequential interferograms provide additional information, and what information do they provide? I don't think you need too much detail, but just expand the sentence a little.
- Line 263 – Cubic interpolation – Why was this method chosen? Did you try any other methods? Justify your choice.
- Line 263 – blunders in the raw DEM – What does this mean? What blunders occurred, and what were their magnitudes?
- Line 265 – smoothed DEM to 90 m resolution - If lateral heterogeneity of arctic tundra snowpack peaks at 100 m according to Sturm and Benson (2004), I wonder what

potential error smoothing to a spatial resolution of 90 m may introduce given that local scale variability <= 90 m may be missed in the estimation since the smoothed DEM will not include topography which is influential in the local scale variability of snow accumulation. How would this effect the accuracy of your methods? This could be discussed around Line 559 where you briefly note the method has its limits.

- Line 267 – provide the appropriate reference for the permittivity relationship (it's not Leinss et al., 2015).
- Figure 7 – what is the grey on each panel? This should be included in the legend or mentioned in caption.
- Line 289 – You have already defined ξ. Just use the symbol here. Note the definition on Line 289 is slightly different from that given on Line 212. Verify these are meant to be the same thing.
- Line 308 – You have already defined ξ. Just use the symbol.
- Line 309 – You have already defined ρ. Just use the symbol.
- Line 346 - what was the model resolution or grid cell size? How does this compare to topography around Inuvik, and correlation length of snow depth in the region?
- Lines 356 – 359 - Environment Canada (EC) = Environment and Climate Change Canada (ECCC); these coords are odd...better written as 68.74°N, 133.54°W; it would be more helpful to show these all on a map instead of descriptions like '43 km north of scene center'; also how was the 3 met station forcing data integrated? eg. if all 3 recorded a different precip amount at a given time?
- Line 364 – Already defined ξ. Just use the symbol here.
- Figure 10 - in f) it would be helpful to include more graduations on x axis such as -20, -10, 0, 10, 20(if it fits). I see you included more in panel c) so why not here? It would help us understand the data spread > 0
- FIgure 10 - maps in panel a) and d) are too small and hard to see. Figures should be larger. Text in panels b) and e) looks too crowded.
- Line 387 – Its not clear to me how we know the spatial pattern is correlated from the topography by looking at Fig. 10a. Please add a little more description to clarify this point.
- Lines 418- 420, Eq(22) – What is Φ_{SA}? Is it 'phase modelled from a simple linear function?' It is not clear from this sentence. What does SA stand for?
- Line 425 – OK, great! I think this is what I was looking for earlier. You have given an estimate of the window – is this the same as the 'local window' mentioned earlier? This quantification should be provided much earlier, along with a discussion of why that window was chosen and what is the max and min recommended sizes and why.
- Figure 12 - maps in panels a) b) and c) are too small
- FIgure 12 - text in panels d), e), and f) looks crammed in and messy.
- FIgure 12 - seems odd to have the panels for heave and solifluction presented here, and not in their respective sections. At least provide the reader some indication as to where they will be discussed (perhaps in the caption, if nowhere else).
- FIgure 12 - shouldn't the x-axes in panels a), b), and c) be in ground range?
- Lines 458 – 460 – What about annual displacement amplitudes for heavily organic soil (eg. peat)? This is present around Inuvik and other high-latitude sites. It is an important consideration if you plan to use your methods eg. pan arctic.
- Line 463 – What is your accuracy target for SWE estimation and why? Did you mention it already?
- Lines 468 – You write 'Of course, this is not possible for the case of widespread seasonal surface displacement as is common in periglacial regions." This is an interesting point. What are the implications for pan-arctic implementation? What proportion of, say, Canada's north is periglacial? In other words, how big of a problem is this? Quantify it.
- Line 472 – Should a snow-free baseline be taken each year?
- Line 484 - What is the physical meaning behind this scaling factor? how is it determined? What is the acceptable range of values and how much does it affect the outcome if using the min vs, the max value? why can we assume it is constant over the

window - provide support for this - references perhaps?

- Line 552 - Is this assumption valid for all conditions? eg. shallow snow, or when delta SWE = 0?
- Line 573 – 575 – it may help the reader to mention, here, that the transect details can be found in Table 2.
- Line 559 – 600 - Can you describe the topographic variation in terms of surface roughness height, or standard deviation of surface roughness? Trying to get an idea visually, of what this area looks like - perhaps you can get photos of the topography of each the low topo area and the greater topo area - it would help the reader connect the values in the rightmost column of table 2 which is unintuitive, with what's actually on the ground. It is important for the reader to have a clear idea what is meant by low and high topography.
- Line 602 - the error bars are large for sites A, B, and C – the low topography areas - perhaps these areas should be masked out - if they were masked out, how much would your accuracy improve over this study region? what % of area would you have left if you masked out the low-topography areas? Did you account for this while choosing this region for the study?
- Line 743 – perhaps restate the RMSE for each.
- Line 750 – How did you determine this method works better if total SWE > 150 mm? Was this discussed or demonstrated?

Technical corrections:

Line 179 – should this read 'Eq 4' ?

Line 618 and 623 – inconsistent use of Fig. and Figure. Check entire document and be consistent. There are more cases – I won't list them all here.

Table 2 – Fix formatting of table. Column heading 'Length' overruns the column width, and the 'h' is on the next line. The last entry in the same column is also too wide for the column and reads 'Not report ed'. Longitude coordinates, again, seem odd. Usually written as 133.775° W, for example.