Review, J. Eppler at al. "Snow Water Equivalent Change Mapping from Slope Correlated InSAR Phase Variations"

General comments:

The manuscript provides a new method for estimation of snow water equivalent (SWE) based on repeat-pass SAR interferometry. Despite very promising results in specific cases, reliable estimation of SWE by SAR interferometry is an over 20 years old problem which could not be widely applied due to unknown phase offsets and ambiguities. The manuscript by Eppler et al. tackles this problem with a brilliant new idea. The authors demonstrate their method using an eight year long time series of Radarsat-2 SAR imagery.

The manuscript provides a clear description of methods, a very detailed analysis of error sources, and a validation of the method based on field measurements and model results. Despite beeing very radar-specific, the work is excellently suited for "The Cryosphere" because it adresses the important problem of SWE estimation which is done with a wide range of different sensors and methods.

Except for a list of small specific comments (below), including several suggestions to shorten the manuscript, I have two minor suggestions to make the method more clear and to improve the structure of the manuscript: 1) method: I suggest to better explain how the sketched 2D geometry is applied in the real-world 3D-geometry. 2) I suggest the discussion and analysis of error sources (section 5) be moved behind the result section and try to shorten section 5. This section 5 about error sources contributes to more than 25% of the manuscript and puts a long "barrier" between the method section and the seems to be better suited in the discussion part rather than between the methods and results.

We thank the reviewer for their constructive comments and suggestions for clarifying the manuscript and improving the structure. We have made all the suggested changes except for two: (1) regarding the suggestion to add a comparison figure for all bias sources; and (2) regarding the suggestion to quantify the coherence of snow free areas. Please see below for our rationale and response to each comment (colored in blue text). Note that the manuscript has been significantly revised and therefore some of the section, figure and equation numbers have changed. Our responses refer to the numbers in the reviewed version of the manuscript.

Specific comments: --- Abstract ---line 14: "RADARSAT-2": It might be good to mention C-band.

Agreed. The text has been changed to "…RADARSAT-2 (C-band) …"

--- Introduction ---line 63: You could add here two references to polarimetric methods to estimate quantitatively the amount of fresh snow. These dual-pol approach could, possibly, be used to provide complementary, non-terrain-dependent information about

SWE changes to you method (in case a dual-pol radar is available). See https://doi.org/10.5194/tc-10-1771-2016 and https://doi.org/10.5194/tc-14-51-2020.

We thank the reviewer for these references. They have been included in the following text "*Furthermore, polarimetric refraction-based methods have been proposed to exploit the structural anisotropy of snow to provide additional information about SWE change within a snowpack (Leinss et al., 2016, 2020), although in this article we focus on single-polarimetric methods*."

line 87-90: Could you add here half a sentence more to explain the "secret" of your method? Up to here, I have seen several promises and the key-ingredient of topographic variations. But half a sentence more of details might be worth to add. Something like "our method exploits the sensitivity/dependency of the signal/phase delay within the snowpack with respect to the local terrain slope".

We thank the reviewer for this suggestion. The following text has been added:

"Our method exploits the dependency of the refractive phase delay within the snowpack with respect to the local terrain slope."

--- Section 3: Method/Priliminaries ---line 150-160: general comment to these lines (see also the three specific comments follow below): In a quasi-2D coordinate system, these lines are convincing. However, in 3D-space, more precise definitions of angles are required. Please also define the coordinate system. I guess, Figure 3 and the definition of incidence angle are not defined perpendicular to the orbit direction but in the plane defined by the line-of-sight and the surface normal of the topography. Such a consideration, especially with respect to the geometry shown in Fig. 3, could require to consider refraction into the dimension of the orbit direction, e.g. for slopes where the surface normal vector has a component into the orbit-direction. Theta and alpha might not be located in the same plane.

We thank the reviewer for these comments. These comments are consistent with our treatment of the geometry. We agree that the text should be revised to provide additional clarity regarding the geometry. A couple of points:

(1) As noted by the reviewer, Fig 3 depicts the plane which includes both the SAR line-of-sight vector and the local slope normal (n). As such, it is not a special case, but is always fully correct regarding the refraction geometry. The refracted ray path will always occur in this plane. The local incidence angle is the angle between the reversed SAR line-of-sight and 'n' and is therefore correctly depicted in this diagram.

(2) The plane depicted in Fig 3 does not, in general, include the local zenith vector, and therefore theta and alpha are not generally located in the same plane. Figure 4 does depict them in the same plane for illustration purposes but this is a special case. This may be misleading and therefore additional text has been added to the caption of Fig. 4 for clarity.

Figure 3 might gain value by adding a small 3D-sketch indicating how the two dimensions of the current figure 3 are defined. The figure caption should also explain the orientation of the shown 2D image in the 3D space.

A small 3D sketch has been included as an inset to Figure 3 showing the general 3D geometry and how alpha and theta angles relate to defining vectors $\boldsymbol{z}$, $\boldsymbol{n}$ and $\boldsymbol{l}$ (SAR line-of-sight direction). The figure caption has been revised include the following text: *"The inset figure shows the general 3D geometry. x, y and z refer to the local East, North and vertical directions.* $\boldsymbol{n}$ *and* $\boldsymbol{l}$ *refer to the local slope normal and SAR line-of-sight directions which, together, define the plane depicted in the 2D diagram."*

line 153: "local incidence angle theta": Comparing the derivation of Eq. (1) with Figure 3, I guess the local incidence angle is measured with respect to the terrain normal n. To avoid confusion with the "local incidence angle" with respect to e.g. the ellipsoid, I suggest to clearly state with respect to which direction (e.g. terrain surface normal) theta is defined. I suggest to also add, that such a definition makes theta also dependent on the aspect of the slope.

We agree. The following has been added: *"Note that θ is defined as the angle between* ***n*** *and -**l** (i.e. reversed SAR line-of-sight) and therefore depends on both the magnitude and aspect of the local slope."*

--- Section 4: Method ---In line 262 you speak about "aspect angle maps". I guess, it could make sense to introduce them here in line 150-160.

The following text has been added: *"As such, this geometry can be defined everywhere within a SAR scene given maps of spatially varying **l**, expressed in the local (East, North, vertical) coordinate system and DEM derived slope magnitude and aspect maps."*

line 156: "alpha is the local slope angle": How is alpha defined in the 3D geometry?

It is the angle between the slope normal and vertical directions. The text has been changed to *"…where α is the local topographic slope angle, defined as the angle between n and z."*

line 185: What is the unit of xi? rad/mm? or rad/mm SWE. It might be good to add a sentence about how much xi varies over a certain range of slope, e.g. for slopes between -30 and +30 degree, rho=0.3, lambda=... xi varies from 0.22(?) to 0.28 rad/mm.

We use "mm" rather than "mm SWE" throughout the document as the unit for SWE. Therefore, for consistency, ξ has been expressed in units of "rad/mm".

line 215: "Assuming that the first term ~xi is the dominant component": Could you provide some argument for this assumption?

On reflection, our wording is misleading/incorrect. For example, when <ΔS> is zero then the first term will be zero, and certainly not dominant. We were trying to say that the additional terms contributing to the bias need to be sufficiently small. This is already expressed in the subsequent paragraph, *"The estimator relies on the assumption that the horizontal SWE change distribution is uniform within the window and that other interferometric phase components are uncorrelated with $\tilde{\xi}$".* The text has been changed to *"Assuming $\tilde{\Phi}$ correlates with $\tilde{\xi}$ with the proportionality <ΔS>, we introduce the following correlation-based estimator for <ΔS>"*. See also, the next comment.

line 215: could you add: "... and that ~Phi correlates with ~xi with the proportionality <\Delta S>"

The suggested change has been made. See the previous comment.

line 261: "interpolation artifacts": where would they come from?

The use of "interpolation artifacts" is a poor word choice. We meant "interpolation error" which is simply the difference between the interpolated value and the true but unknowable value at the interpolation point. The text has been changed.

line 262: "artifacts from the cubic interpolation": (bi)cubic interpolation is known to cause overshoot. Why did you not use e.g. bilinear interpolation?

We did not thoroughly investigate the issue of most appropriate interpolator and so there may be room for improvement here. The following text has been added: "*We did not thoroughly investigate the issue of most appropriate interpolator and so it may be possible to reduce these errors by using a different interpolator*."

Figure 7: What are the uncorrelated phase components? Could you provide a variable name?

This refers to the phase components discussed in the section titled 'Uncorrelated Phase Components'. A forward reference to the section has been added to the figure caption.

line 290: "normalized range bandwidth": is that bandwidth / central frequency?

Yes, the text has been changed to *"bandwidth of the SAR normalized with respect to carrier frequency"* for clarity.

--- section 5, Error sources ---Would it be possible to summarize all the errors discussed in the whole section 5 in a figure? Something with a caption line "Estimated magnitude of SWE errors through the estimator due to different error sources".

This is difficult because our approach to the error analysis has been to estimate the sensitivity of the SWE errors to particular factors which may vary significantly both in time and spatially within the scene (e.g., heave magnitude, solifluction rate, decorrelation). Furthermore, for soil moisture phase, we only derived an upper bound to the error. For these reasons it is difficult to provide such a summary of their relative contributions.

line 378: "to detect these events by analysis of the wind history": Would an analysis of SAR data from a different orbit direction cause an error with the same sign or would the errors average out? i.e. could a parallel observation from the opposite orbit direction also be used to detect such events?

This is an interesting question. Yes, since the effect is dependent on sensor flight direction, comparing results from differing geometries should allow for detecting this effect since the SWE change estimates should then differ. The effect should reverse sign for near-parallel but opposite flight paths so averaging such paired results would mitigate this effect to some degree. However, due to orbit inclination, it is not possible to achieve this parallel condition exactly, especially at higher latitudes. The following text has been added:

"Another potential mitigation is to use SAR images from the opposite pass direction. The bias from the near-opposite horizontal direction should have the opposite sign and therefore the estimated ΔSWE should differ significantly between pass directions, allowing for the effect to be detected and perhaps mitigated by averaging the results."

line 415: Describing the "static" component as a "horizontal mean component" appears confusing to me. Especially because the "horizontal mean component" is "modulated by topography". So, "horizontal mean component" might require some rephrasing, indicating where the modulation by topography comes from. I guess, static means related to the density of different horizontal air layers or simply different air pressure or humidity. Maybe, simply drop the words "horizontal mean component and horizontally variable component" and directly call them static and dynamic.

Agreed. The text has been changed to be simply: *"This delay can be decomposed into static and dynamic components."*

Section: 5.3.2 This section could be slightly shortened.

We have shortened this section by removing about seven lines of text.

line 466: "summer interferograms can be used to identify areas": As you describe, heave and subsidence are periodic, hence, in theory, observation of subsidence could be used to estimate the error due to heave.

Yes, good point. The following text has been added: "*It may also be possible to estimate and correct for the early-winter heave component by estimating the thaw-subsidence from summer-season interferograms and then applying a model for the cyclical deformation."*

section 5.3.3: Could be shortened.

We have shortened this section by removing about six lines of text.

section 5.4: could be shortened.

The part on Monte Carlo simulation has been slightly shortened and moved to the methods section. The rest of this section has been significantly shortened and moved to the start of Section 5.3. As a result, this section has been removed from the manuscript.

line 522: To make it easier for the reader to find where you describe the Monte Carlo estimations, I suggest to add here a sub-sub-section heading.

Agreed. A sub-sub-section heading has been added.

section 5.5: can be shortened.

Eq. (28) and several lines of text have been removed.

--- section 6: Results and Discussion ---585: "the in situ measurements correspond to an upper limit" - considering the many positive biases due to various error sources, I'm not sure if that's true.

This is a good point. The text has been revised: *"Therefore, for the comparison, the in situ measurements correspond to an upper-limit for the expected SlopeVar estimated total SWE maps, neglecting any biases. However, given the error sources described in Sect. 5, actual estimates may exceed this expected upper-limit."*

592: "sampled at the spatial mean position": Looking at Table 2, it seems the resolution of the estimator is not good enough to compare individual SWE samples with individual estimated values. It would be good to refer to "the transect length given in Table 2" to justify why the spatial and SWE mean values were used for comparison of measurements with estimated values.

Agreed. The following text has been added: *"This was done because the transect lengths, listed in Table 2, were 200 m or less, being well within the 500 m SlopeVar estimator window size."*

634: "likely, because only snow-free areas (..) are coherent": I think this should be easy to check to make a better confirmed statement. Something like: "most melting snow areas have a coherence below ... which are not considered in the estimator." (check that the method section contains the information how to deal with low-coherent areas).

We disagree with the reviewer on this point because it is not straightforward to identify ‘melting snow areas’ as spatial subsets within each interferogram. One could attempt to estimate these based on the coherence magnitude itself but then the resulting coherence estimate for melting areas will depend on the chosen threshold.

The ‘Estimator Implementation’ section has been revised to include information regarding dealing with low-coherent areas:

“Solutions where the maximum was within 2 grid intervals from the edge of the search range were labeled as invalid. This provides a means for excluding poor results from low coherence areas since for these areas the periodogram analysis typically does not result in a peak within the search range. We considered adding an additional exclusion threshold based on coherence magnitude but found it unnecessary. Water-body areas were also labeled as invalid.”

--- section 7 ---I don't see the relevance of section 7 for the paper. This section could well be published as a seperate contribution/letter. I suggest to remove at to make the paper more concise.

We included this section in an effort to appeal to a wider readership and provide a demonstration of how the method might be applied but recognize that it adds additional length and can be removed without affecting the method presentation which is the primary goal of the manuscript. The section has been removed and we will consider including the analysis in a separate publication.

--- section XX: discussion ---The "result and discussion section (6)" has only a few references to section 5 (error sources). However, the 10-page long section 5 puts a significant barrier between the method and the result section. Therefore I suggest section 5 be moved behind the result section. An exception might be the paragraph after line 522 (monte carlo approach) which could be incorporated into the method section.

The manuscript has been significantly restructured. Former Sections 3 and 4 have been incorporated in a single ‘Methods’ section (now Section 3). The results from ‘Results and Discussion’ have been put in the ‘Results’ section (now section 4) and the error sources (former Section 5) have been merged with the other discussion subsections into a ‘Discussion’ section (now Section 5). The subsection on Monte Carlo approach has been moved to the methods section.

line 715: "DEM-derived dry-snow phase sensitivity map" - I would add "[DEM-derived,] slope-dependent..."

The suggested change has been made.

--- conclusion ---

line 718-730: Similar to my sugggestion regarding section 5, I suggest this paragraph be moved behind line 741. I also suggest to make this paragraph as compact as possible.

This paragraph has been moved and shortened significantly.

line 752 - 755: similar to section 7, I suggest to remove these analysis. Line 742-751 provide a good finish of the manuscript. (check also line 18-20 in the abstract).

Agreed. These lines, and those in the abstract relating to section 7 have been removed.

Technical corrections:

1-6: "as an alternate technique": I guess you mean "alternative". You could also start with "Another option is repeat-pass ..., that allows"

Agreed. The text has been changed to: “Another method is repeat-pass … that allows…”

line 149 "the phase of the SAR signal" -> "the unwrapped phase of the SAR signal" (to define Φ; see also comment below for line 235.)

The suggested change has been made.

line 171: "horizontally uniform": Could it be better to say "spatially uniform"? I know what you mean by "horizontaly uniform" but it might be confusing to first read "constant topographic slope" and then "horizontally".

Agreed. The suggested change has been made.

line 174 "dry-snow snow" -> "dry-snow"

The suggested change has been made.

Figure 4, caption: "While vertical snow depth is constant" -> "While vertical snow depth Z_s is constant"

The suggested change has been made.

Figure 5, caption(b): "Topo-corrected 24-day interferogram": Specify, if this is an unwrapped interferogram.

The shown interferogram is wrapped. This has been added to the caption.

line 232/234: "alternate" do you mean alternative, altered or modified? I understand alternate as swapping back and forth.

Agreed. The suggested change has been made.

line 235: "the set of wrapped phases" -> "the set of wrapped phases ϕ" (makes it easier to follow the argument that exp(j*phi) = exp(j*Phi).

The suggested change has been made.

great work!

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