

The Cryosphere Discuss., referee comment RC1
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Comment on tc-2021-359

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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-RC1>, 2022

Review, J. Eppler et 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 being very radar-specific, the work is excellently suited for "The Cryosphere" because it addresses 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.

Specific comments:

--- Abstract ---

line 14: "RADARSAT-2": It might be good to mention 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 your 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>.

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".

--- 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.

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.

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.

--- 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.

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

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.

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

line 215: could you add: "... and that $\sim \Phi$ correlates with $\sim \xi$ with the proportionality $\langle \Delta S \rangle$ "

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

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

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

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

--- 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".

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?

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.

Section: 5.3.2 This section could be slightly shortened.

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.

section 5.3.3: Could be shortened.

section 5.4: could be shortened.

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.

section 5.5: can be shortened.

--- 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.

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.

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).

--- section 7 ---

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

--- 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.

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

--- conclusion ---

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

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).

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"

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.)

line 171: "horizontally uniform": Could it be better to say "spatially uniform"? I know what

you mean by "horizontal uniform" but it might be confusing to first read "constant topographic slope" and then "horizontally".

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

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

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

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

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

great work!