The Cryosphere: Eppler et al., Snow Water Equivalent Change Mapping from Slop Corrected InSAR Phase Variations

General Comments:

The study presented attempts to quantify snow water equivalent (SWE) using interferograms of wrapped phase from 9 years of RADARSAT-2 acquisitions over the Trail Valley Creek region of the Northwest Territories. The authors present a clear and sound scientific analysis of the interferometric principles and how they apply to snow overlying a variable topography with underlying tundra/shrub landcover classes. In essence the study is a significant contribution to the development of snow water equivalent retrievals using spaceborne SAR, especially C-band for relatively shallow snowpacks because it is generally understood that the snow depth/grains in tundra regions are too shallow/small to produce significant volume scatter, respectively. The understanding of the signal interaction with the snow depth and volume is well-presented, and is valuable for those entering this research space.

That being said, the theoretical construct of the paper to retrieve change in SWE (ΔSWE) hinges on the assumption of a consistent snow density across the study terrain, as well as year over year. As a reader this presents as problematic because in Section 6.2. the in-situ transects are presented, but the snow density is described is 0.3g/cm3 across the study region and times in the Winter season. In addition, there are only two years in which snow observations of the snow properties are incorporated into the analysis. There have been extensive observations of snow depth, density, and influence of vegetation going back to 2012 by Environment Canada, and it would be useful to see this incorporated into the understanding of snow density. Overall, the reliance of a bulk snow density also does not incorporate the reality of snow conditions in tundra regions of Trail Valley Creek, where snow is commonly a combination of a wind slab and depth hoar layer, with high and low snow densities, respectively. Conceivably, this could also change the signal interaction with the snow volume, as refraction and velocity would be slightly modified. This is not addressed as a limitation.

We thank the reviewer for their constructive comments and suggestions for clarifying the manuscript and improving the structure. We have addressed all comments and, in most cases have made the suggested changes. However, we disagree with the reviewer regarding several of the comments, most notably:

(1) those regarding the importance of prior knowledge of snow density and the suggestion that the proposed method is better suited as a depth estimator
(2) the suggestion that we obtain data from the Trail Valley Creek site to validate the results

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.

Some more general comments before specific comments:

- In Section 5.1. you discuss how snow density changes due to "settling". It's important to note that the density within the snowpack varies as well. Bulk density can be used commonly in these types of analysis, but it seems uniquely important here to address that the wind slab and depth hoar densities are quite different.
  - Several studies have also reported snow densities for this regions and study period, for example (among others):
    - Rutter, N., Sandells, M. J., Derksen, C., King, J., Toose, P., Wake, L., ... & Sturm, M. (2019). Effect of snow microstructure variability on Ku-band radar snow water equivalent retrievals. *The Cryosphere*, *13*(11), 3045-3059.
    - King, J., Derksen, C., Toose, P., Langlois, A., Larsen, C., Lemmetyinen, J., ... & Sturm, M. (2018). The influence of snow microstructure on dual-frequency radar measurements in a tundra environment. *Remote Sensing of Environment*, *215*, 242-254.
    - Meloche, J., Langlois, A., Rutter, N., Royer, A., King, J., & Walker, B. (2021). Characterizing Tundra snow sub-pixel variability to improve brightness temperature estimation in satellite SWE retrievals. *The Cryosphere Discussions*, 1-22.

    We thank the reviewer for the references and have included citation of Rutter et al (2019) and King et al (2018) below. Regarding wind slab, reviewer #3 asked a similar question regarding the commonly encountered wind slab over hoar profile. However, it can easily be shown based on the density misspecification analysis (i.e., Eq. (19)) that such a situation (even 500 kg/m^3 wind slab over very low density hoar) results in only a 2.5% estimation error when assuming $\rho = 0.3$. We have added the following text to the end of the section titled "Snow Density Misspecification":

    "Regarding the effect of vertical density layering, our study area is prone to wind slab formation where the late season snowpack can consist of dense wind slab overlaying a low density hoar layer (Rutter et al., 2019; King et al., 2018). Considering the extreme case of near zero density hoar overlain by $\rho = 0.5$ wind slab, assuming uniform $\rho = 0.3$ results in a +2.5% estimation bias which is still a small error compared to the other bias sources considered in our analysis."

The paper overall reads somewhat like a dissertation rather than a manuscript. Sections do not necessarily flow like a common manuscript (Intro/Background/Data/Methods/Results/Discussion), rather segmented into several smaller sections. This is more of a comment than requesting a change. For example, Section 3 (Spatial Variations of Repeat-pass InSAR Dry-Snow Phase), Section 4 (Estimation Method), and Section 5 (Sources of Estimation Error) – are these all sections within the Methods?

We thank the reviewer for this comment. The manuscript has been significantly restructured into the following sections:
1.Introduction
2. Data
3. Methods (contains both section 3 and 4 from the reviewed version)
4. Results
5. Discussion (contains most of section 5 from the reviewed version as a discussion of errors)
6. Conclusions

- In terms of validation, were no snow depth or SWE large scale transects ($n > 100$) used in this study? I understand that the exact snow depth or SWE could not be collected for every location or date, but as it reads we are accepting that the SnowModel outputs are truth and validating against that?

  In situ validation was limited to the set of eight late snow seasons snow tube transects obtained in 2017 and 2018. We also used the ERA5 data as a temporal validation to assess the bias in the estimates.

  We did not use SnowModel for validation but instead just used it to investigate the magnitude of errors caused by spatial SWE change inhomogeneity.

- Overall, I am slightly confused as to why the authors are presenting this study as change in SWE, because SWE is dependent on the depth*density. The authors are prescribing density across the whole study, during the entire season. Therefore, what they are truly retrieving is the snow depth. When the authors are attempting to quantify bias to SWE from many sources, they present in mmSWE, when as I understand it, they are actually quantifying change in snow depth.

  We respectfully disagree with the reviewer regarding this comment and the other comments in this review which incorrectly stress the importance of densities for the proposed method.

  The method does require a prescribed density as an input. It is also true that regarding the three quantities: density, depth and SWE, knowing any two allows the third to be determined. These two facts do not imply that the method is retrieving depth instead of SWE.

The opposite is in fact true: the method retrieves SWE and requires the assumed density to infer depth. As such, the method is well suited as a SWE estimator but only suitable as a depth estimator if the density is well known. The reason for this is that the estimator is quite insensitive to density misspecification. Section 5.1 (Snow Density Misspecification) covers this in some detail. Furthermore, Leinss et al 2015 discuss this in some detail, reaching the same conclusion which is well summarized by their Eq. (18). They state in their conclusion: *"A sensitivity analysis with respect to snow density and incidence angle showed a very weak dependence on snow density."* Our analysis, described in Section 5.1, agrees with this conclusion regarding density.

- Section 6.4.: The discussion about the active layer of the ground surface promoting a bias underscores how this paper could be improved by looking to quantify snow depth change as opposed to SWE (with SWE being inferred after using apriori knowledge of density). That way, the heave associated with the freeze could be compensated for within a snow depth algorithm, the same way that freeboard could be for lake/sea ice. I would suggest that presenting the change in snow depth as opposed to SWE would make Section 6.5. more straightforward to account for.

  We respectfully disagree with the reviewer. First, as noted in our response to the previous comment regarding depth, our proposed method does not measure snow depth. It is true, that if density is well known, then depth could be inferred from the SWE estimates.

  Furthermore, our method is based on differences in the repeat-pass propagation phase, not differential measurements of topographic height which is what this comment is assuming.

- While interesting, it's my feeling that the inclusion of Section 7 is too much for this study. There are new datasets, models, methods, etc., that are introduced and it should be a standalone study. The authors portend as much, stating on line 682 that it is not within the scope of this paper.

  Reviewer #1 made a similar comment. We have removed Section 7 and will consider submitting it for a separate publication.

Specific Comments:

Page 6 Line 140: “Spatial Variations of Repeat-pass InSAR Dry-Snow Phase” – is this the beginning of the Methods section? Or a Background section?

We have added the follow at the very beginning of this section to clarify: *“This section begins with a brief background on the InSAR phase contribution from dry-snow on a uniform slope and then extends this to the more general case of slope varying terrain. Together these describe the source of the spatially varying InSAR phase which our proposed method exploits.”*

Page 14 Figure 7: The right y-axis label for frame (d) says mm SWE – I believe this should be “Change in mm SWE”.

We were inconsistent with our use of units regarding SWE and change in SWE, sometimes using “mm”, and sometimes “mm SWE”. We have gone through and changed both absolute and relative SWE units to be simply “mm”. In this specific case the ΔSWE standard deviation unit has also been changed to “mm”.

Page 15: Section 5 “Sources of Estimation Error” = Should this read “Sources of Estimation Error in the Proposed Method”? It currently reads as a Discussion before the Discussion section.

Reviewer #1 made a similar comment. We have moved this section to after the discussion section.

Page 16 Line 309: “which as shown in Eq.(3), depends on snow density” – Yes I agree- this is where in-situ observation would be useful, for within the winter season or year over year.

However, the section containing this line then goes on to show that the sensitivity of the estimated SWE to prescribed density is low. Please refer to our response to the previous comment regarding this issue.

Page 17 Lines 346 – 348: “Snow Model, implemented….” – This is the first that I’m reading of the incorporation in the snow model, and this is Section 5 (which I’m not sure if it’s the Methods section or not). If this is being used for validation, it should be discussed in the methods section earlier on, with the model runs, input data, etc., specified. The new methods are continued to be presented until line 363, which may mean that these new methods need to be restructured into an earlier section of the paper.

The snow model was not used for validation. It was used to investigate the likely magnitude of bias contributed from horizontal SWE change inhomogeneity. This section has been moved to the discussion along with other subsections discussing bias sources affecting the results.

Page 18 Figure 10: What is the high end label for frame (f) on the x-axis?

The ticks on this axis were too sparse. We thank the reviewer for pointing this out. The high end of this axis is ~20 mm. More ticks have been added to the axis to make this clearer.

Page 19 Line 402-405: I know that I recommended that Section 7 be removed, however it would be interesting to note what landcover type elicited the most error within going into too much detail.

It is a bit unclear what the reviewer is asking for here since Section 7 and Section 5.2 (containing lines 402-405) discuss different things. Section 7 summarizes an analysis of the estimated SWE changes integrated by land cover type and basin footprint whereas Section 5.2 reports on modelled biases due to spatially varying snow holding height.

If the reviewer is referring to the subject of Section 5.2, then we should point out that the land classes are input into the SnowModel as a single snow holding height per class rather than a spatial distribution and so their effect on the modelled error (correlation between ξ or ξ^2 and the modelled SWE over each estimation window) comes from the spatial variation in land class rather than the land classes themselves. With this in mind, we do not think it useful to report on the modelled error per land class.

If the reviewer is referring to the subject of Section 7, we do not have spatially continuous validation data and so cannot determine the estimation error per land class.

Page 23 Section 5.3.4: I don't understand this inclusion – how is this error potential derived with respect to soil moisture if there is no soil moisture data presented?

We have presented a theoretical argument to place an upper bound on the error. Such an argument requires no data to be presented.

Our presented SlopeVar method is an interferometric method and hence uses the repeat-pass InSAR phase. Soil moisture is a potential error source because of its InSAR phase contribution. The cited papers (De Zan et al (2014) and Rabus et al. (2010)) have presented results that show that there is an upper bound to this phase contribution. We have presented a 'worst case' scenario, i.e. assuming the upper bound soil moisture phase contribution and perfect correlation with the SlopeVar ξ

factor. Even for this worst possible case, we have shown that the resulting bias for the SlopeVar estimator is relatively small (i.e. 1.3 mm SWE).

Page 26, Section 6: Sections 3 – 5 were an extensive description of the methods (and could conceivably be truncated and merged into a single section for clarity), and we're getting to the results of Page 24 of the paper. My concern here harkens back to my comment that the paper reads more like a thesis dissertation than manuscript, because the Results and Discussion (including Section 7, which I believe should be omitted) only take up 10 pages, and is the most impactful portion of the work.

We thank the reviewer for this suggestion to improve the flow of the manuscript. We have made efforts to reduce the length of the error discussion and have moved it to the discussion section.

Page 26, Section 6.2.: "Comparison of SWE estimates with In-situ Measurements" – This information and data needs to be presented in the Data section. You provide the description of the different transects in Table 2, without listing what the values actually are- what are the snow depths? Snow densities? You state that you conducted these measurements with an ESC-30 snow density sampler, instead of listing a mean bulk density for instance.

We thank the reviewer for pointing this out. We have added a plot of all sample snow depths and densities for all eight transects and have revised the transect summary table (formerly Table 2) to include mean transect depth and density for each transect. Also, we have moved this description of the transect data to the data section.

Page 27, Lines 605 – 606: "SWE change predicted by the ECMWF ERA5 reanalysis model over the same time interval". Now, in the Results section, we are introducing a new data variable, one that has a km scale resolution, which is surprising for the reader. The ERA5 model spatial resolution is 9 km, meaning that the variability that is so crucial to this study is lost. You show one data point for each winter season to compare to the ERA5, so you are averaging spatially, and over time. There are existing snow depth and density records that have been extensively collected over Trail Valley creek, and I encourage the authors to reach out to those authors to obtain validation datasets.

We thank the reviewer for their comments. However, they are a significant mischaracterization of our study as conducted and described in the manuscript. ERA5 was used as a means of assessing the bias in the SlopeVar estimates and spatial averaging is appropriate for such a bias assessment. Furthermore, we disagree with the statement that 'one data point was used per winter season'. In fact, 46 snow-season maps over 10 winter-seasons (2 partial and 8 full) were used in the snow-season portion of the analysis, corresponding to, on average 4 or 5 data points per winter season. We also disagree with the statement that the results were averaged over time. No averaging over time was conducted for this comparison which compares 24-day interval SWE change estimates.

Regarding the idea of validation with Trail Valley Creek datasets, our dataset image footprint is centered over the town of Inuvik which is 43 km south of the Trail Valley Creek site. Furthermore, Trail Valley Creek is situated above the treeline whereas Inuvik is below the treeline. For these reasons it is unclear how applicable Trail Valley Creek datasets would be to our study or even how they could be used for validation since there is no spatial overlap between the two sites.

Page 28, Figure 14: This graph presents a lack of detail based on the output of the analysis. What about histograms of change in SWE, to reflect the distribution of the data? Or statistical analysis of the in-situ vs slopevar estimator? For how exhaustive the methods and error source documentation was, the results here compared to in-situ data seem to be glossed over.

We thank the reviewer for these suggestions to improve the description of the results. Regarding statistical analysis of the in-situ vs SlopeVar estimator, there are eight transects and each is summarized by a mean SWE and computed standard error. These we compared to spatio-temporal interpolations of the accumulated SlopeVar estimates, currently displayed as a scatter plot with x&y error bars and we have quoted a computed global RMSE of 15 mm. We considered conducting a p-value based analysis of the hypothesis that the SlopeVar estimates are consistent with the in situ values assuming Gaussian error statistics. The problem with this is that the SlopeVar time-accumulated values 'miss' early season snow accumulation and therefore the in situ values represent an upper-bound for the SWE change captured by the SlopeVar estimates. We have additionally computed the bias (mean difference between the in situ and SlopeVar SWE values) and added it to the text: "*Treating the transect mean values as truth, and neglecting the unaccounted early snow-season SWE, the RMSE for all transect comparisons is 14.8 mm and the bias is -6.6 mm.".*

Regarding histograms of SWE change, we did include these in Section 7, Fig. 17a, partitioned according to aggregated land cover class but these have now been removed along with all of Section 7 as requested. We have recomputed similar histograms but partitioned according to the {'Oct-Dec', 'Jan-Mar' and 'Snow Free'} temporal subsets and added these to Fig 17 (Fig 16 in the reviewed manuscript).

Page 29, Table 3: Looking at the subset for seasonality, are these averaged over multiple years? Or just years with in-situ data? How does the averaging of multiple snow seasons together affect the results?

These seasonal subset statistics include data from all years spanned by the dataset. However, they are not computed by first averaging across the years. For example the 'RMSE', 'Jan-Mar' table cell is the RMSE of all (SlopeVar_spatial_average – ERA5) values computed over the set of all intervals occurring between 01-Jan and 31-Mar of each year.