The Cryosphere Discussions

The Cryosphere Discuss., referee comment RC2
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Comment on tc-2021-359

Anonymous Referee #2

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-RC2, 2022

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.

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.
  - 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?
  - 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?
  - 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.
  - 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.
  - 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.

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?

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

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.

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.

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.

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

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.

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?

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.

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.

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.

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.

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?