This paper presents interesting results that offer both hope and caution for L-band InSAR and InSAR generally. The quality and extent of the field validation data are impressive, perhaps the best I've seen from a field snow experiment.

Three suggestions to improve the analysis and the presentation:

1. My major critique is that the target audience has to know a lot about radar and radar remote sensing of snow properties to understand the paper's implications. Craig Bohren has a pointed phrase about a subject being "well known to those that know it well," and this paper unfortunately hits that spot especially in the Introduction. Some colleagues tell me that the coded vocabulary makes the radar remote sensing literature hard to penetrate.
2. The approach for all the methods for SWE retrieval seems to combine a measurement of depth by some remotely sensed method, and then to multiply those depths by estimates of density. With snow depths retrieved from lidar or photogrammetry, this is the viable approach, but from InSAR data it's feasible (and probably better) to directly retrieve SWE without estimating depth or density.
3. The explanations for getting SWE from InSAR are scattered throughout: in the Introduction, Section 3 (Methods), or the Appendix. Perhaps consolidating might be the answer, or advise some readers to read the Appendix first.

Some line-by-line comments, but consider in the context of the three points above.

Line 30: maybe insert a short parenthetical definition of L-band (frequency 1-2 GHz).

Line 35: I tend to avoid adjectives ("high" here) to describe statistical measures like correlation. In some spectroscopic retrievals I work on, $r<0.9$ is awful. Present the values themselves.

Line 34: Is "coherence" the same as "correlation"? Without knowing that, some of the rest of the Abstract is hard to interpret. In general, this issue pervades the paper. Coherence is shown to be important but isn't defined.

Line 36: poor in one year, good the next. Any explanation? I see on Line 420 that this may be an artifact of mis-registration between airborne and in situ data.

Line 37: The sentence "We found that …" seems incongruous with RMSE between 19-22 mm. It would also be useful to specify the ranges of SWE (total) and ΔSWE (between passes) in the experiment. This information does show up later in the paper.

Line 47: The Wrzesien 2018 paper covered North America but the sentence is global. Maybe cite the 2019 paper instead (DO10.1029/2019WR025350I) or clarify that the sentence applies to North America in the 2018 paper.

Line 54: SNOTEL stations are all on nearly flat terrain, hence interpolating between them misses effects of slope and orientation. This sampling bias, combined with the spatial and elevational extent of the snow pillow network, subjects interpolation to artifacts.

Line 56, let's correct a misunderstanding: National Academies of Sciences, Engineering and Medicine are NOT a "government agency."

Line 60: I don't think SnowEx was a "mission." The Durand et al. 2018 reference uses "campaign."

Line 65: “is” not “are”.

Line 72: Need a short tutorial here explaining what backscatter, time-of-flight, and co-polar phase difference are. And then a sentence about why the paper focuses on InSAR (which indeed is defensible). The reference to Borah et al. 2023 perhaps distracts. If indeed we can measure SWE up to 800 mm based on backscattering at X- and Ku-band, why go to interferometry? Earlier work by Jiancheng Shi also got impressive results based on multifrequency multipolarization backscatter, albeit with validation by a only few snow pits.
Consider this comment in the context of data processing. Then the details of how you measure coherence, time delay, phase angle, etc. (now Lines 85-107) can be covered in Section 3 or in the Appendix (but make the forward reference).

Line 74 et seq. At the first introduction of “frequency,” it would be useful to include a short table that translates between “Q”-band, frequency, and wavelength. I hope that this paper will be read by people who have no idea what X-band is, or whether X-band’s frequency is greater than or less than P-band’s.

Line 85: Maybe a sketch here to explain what a phase change and a coherent reflection are, or cite where one can find an explanation, or refer to Section 3 or the Appendix. In the current version, it’s difficult to figure out how one goes from measurement to estimate of phase change.

Line 86: “The technique was first established at C-band . . .” First established to do what? Does this remark refer especially to snow, or to interferometric retrievals of elevation?

Line 88: “interferogram” indeed well known to a small community, possibly obtuse to other readers.

Line 98: Not sure what “only two of these studies have not considered atmospheric signal delays” means. Does it imply that signal delays are important, but seemingly well covered?

Line 100-108: This paragraph has information, but not enough to know how one gets a measurement of phase difference between an interferometric pair. Also, is coherence the same as a product-moment correlation? Or something related but different?

Line 170: I suggest expanding section 3.1 with material from the Introduction (line 85-107) For the less informed reader, the relationship between coherence and phase is arcane. In particular, the snow properties that degrade coherence are important and affect the need for frequent image acquisition. How is the interferometric phase angle determined from the correlated (cohered?) pairs?

Line 177: And then we have to worry about “phase unwrapping,” but this text doesn’t tell us what that is. Also, is phase unwrapping a problem generally with SAR at L-band and higher frequency? Perhaps interpret the equations in Leinss et al. 2015 to explain? (Later I see phase unwrapping at ~100 mm)

Figure 2 and Line 196: Calculations of Incidence Angles from the Copernicus DEM lead to an uncertainty in cosine(incidence) of ~0.1 (from my own work, DOI 10.1029/2022JG007147), but are you able to overcome this problem because repeated images get you the right incidence geometry? Otherwise this is a source of uncertainty, even with the best available global DEM.

Line 215: Can you include a equation that defines Coherence? Or is it just Pearson product-moment correlation?

Line 235: Maybe include a citation to Reflex W? I may not need to know what a "de-wow" filter is, but I'd like to know that I could find out.

Line 248: The title of Section 3.2.3 is "TLS" but the section also covers the UAV lidar.

Line 283: "phase cycle" appears here for the first time. The cognoscenti know what this is but some readers may not.

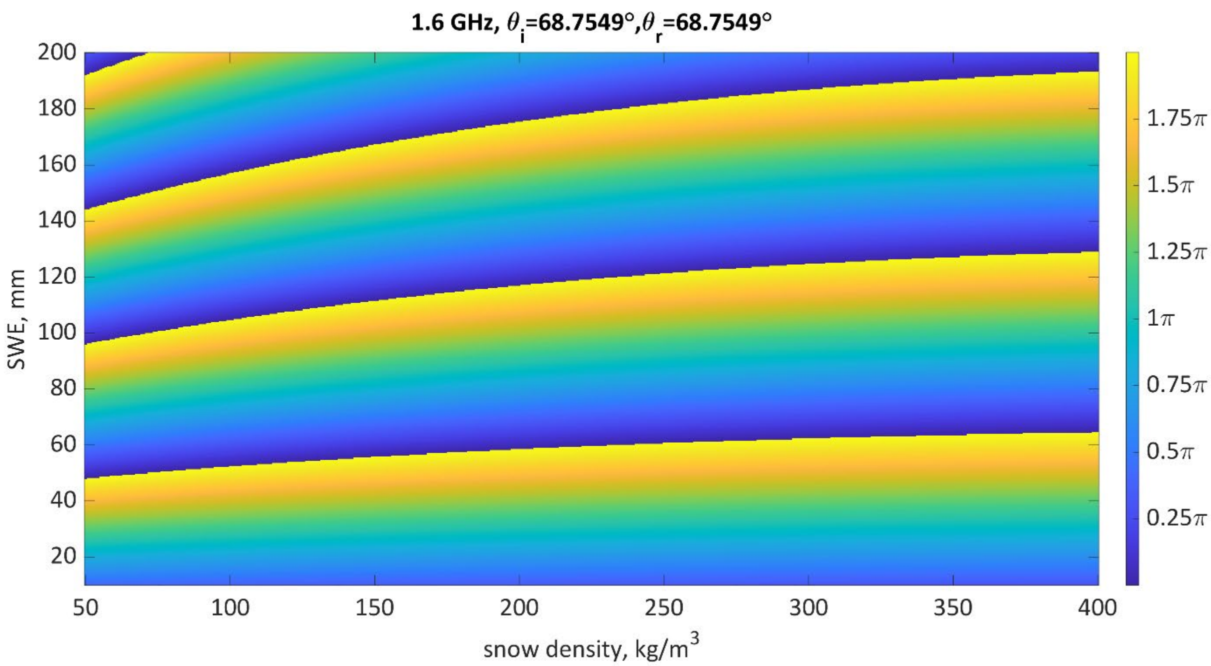
Line 424: "phase unwrapping" is mentioned here and elsewhere. In processing the interferometric phase values, how do you decide when you've gone through a phase cycle? Or more than one?

ESTIMATING SWE DIRECTLY FROM InSAR (instead of estimating depth and multiplying by density)

Rearrange Eq. (A5) to calculate $\varphi_s$ (similar to how Leinss et al. 2015 explain):

$$\varphi_s = \frac{4\pi \Delta d_s}{\lambda}\left(-\cos\theta_i + \sqrt{\varepsilon_s - \sin^2\theta_i}\right)$$

By inspection, two snow terms drive $\varphi_s$ to increase, $\varepsilon_s$ which depends on density $\rho_s$, and $\Delta d_s$. The relationship is nearly linear, certainly linear in $\Delta d_s$ and nearly linear in $\rho_s$. $\Delta SWE = \Delta d_s \rho_s$, so different combinations of $\Delta d_s$ and $\rho_s$ can yield the same $\Delta SWE$. $\Delta SWE = \mathrm{f}(\varphi_s)$ is nearly linear with a weak dependence on density only at combinations of deep snow with low densities.




Thus, a compelling argument for InSAR is its lack of dependence on density, in contrast to lidar for example where the biggest uncertainty is that in density.