2026-07 Gelinas et al. Review 2026-07-07

General comment:

The manuscript describes the analysis of microwave radar data in a partly forested, agricultural area of Ontario, Canada, during the winter season with snowcover.
The study was focused on the influence of the underlying ground, using dielectric-soil and surface-roughness measurements. The description was mainly limited to a single day: March 1, 2023. The temporal variation cannot be assessed.
Relevant information is missing, e.g. if the snowpack was dry during all SAR observations.
Often the wording is not precise, giving rise to irritation. Please be more thoughtful when writing. Still, the results are encouraging and should be published after improvements.

Special comments:

1) Title: "Soil microwave background retrieval" and "snow sensitivity" both are unclear.
It is more about SAR sensitivity on dielectric soil properties and possibly on certain snow properties, which were not presented.

2) Abstract, Line 7: avoid "strong agreement". Only disagreement can be strong or large.
(Actually, according to Figure 3, the time-varying permittivity was rather constant, near 40, during the time when radar observations occurred.)

3) Introduction
Line 21: "(SWE) representing the amount of meltwater contained in the snowpack". A dry snowpack does not contain meltwater.
Line 39: Delete "high frequency" before "Ku band SAR". 13 GHz is near the lower-frequency end of Ku band.
Line 72: Please describe what you mean with "A one-at-a-time sensitivity analysis".

4) Section 2
Line 91: Give the dates of this period: "conducted during this stable accumulation phase"
Line 103: Nothing is visible about "The collected data offers a comprehensive dataset".
Figure 2 is irritating with all the coloured lines without proper description.

5) Section 3
Table 1: Delete the lower part (without measurements). Describe the status of this radar development somewhere else.
Line 151: "... included all of the necessary snow information needed". Nothing is mentioned on possible liquid water content in the snowpack. If wet snow was present, large changes of microwave signatures should have occurred. Please clarify.
Line 154-157: Ice layers and bottom ice crust can strongly influence the situation. The described simplification that was made is hard to justify.
Line 165 to 175: Point clouds. What are: the size of a "point", the distance to its nearest neighbour, the shape of correlation functions, the correlation length in relation to the radar wavelenths? Does vegetation play a role? Answers are relevant for understanding and assessing the results.
Range and variability of RMSH may also be relevant, not only MSS.

Both quantities may be needed for roughness representation. The limitation to MSS must be justified and linked with the scattering model. Furthermore, the influence of refraction and propagation through snow may also influence radar signals, especially for bottom ice. Effects of ice segregation in the soil-ice transition can lead to water fluxes and ice-lens growth.

Equations (2) & (3): Backscatter coefficients: what was used? dB values or actual values? Note that wave superpositions are linear in power (and distorted by speckle noise). The reason for using dB values came from engineering to cover a large dynamic range.

6) Section 4

Figure 3: The large standard deviation in soil permittivity is strange. Is there a sensor problem? What is the problem with the actual data, and why did you use this type of presentation?

Figure 4: Information on frequency is missing. Add it in the key next to CryoSAR, etc. In addition to this view, a spectral representaion would be helpful, too. Also, variation with time could be helpful for the understanding, if linked with Figure 3.

Line 252: "all available frequencies (CryoSar, RCM, TSX)": These are not frequencies. You meant backscatter data at the frequencies of the named sensors. This is a complicated way for readers.

Table 2: Again, frequency information is missing (sensor names are less important).

Line 262: "good accuracy" is not a sound description.

Figure 5 is irritating. In the key, the line strength should be the same as in the graphs. The names of the keys (like Central): what do they mean?

Figure 6 is questionable. Better understanding should be found before putting such a figure in the paper. Do not call MSS roughness.

Figure 7 is even worse, should be deleted. Who knows what Sobol sensitivity is?

Figure 8 should be deleted in my view. What do you think that you and readers can learn?

Figure 9 is interesting, but it is important to better describe what it shows.

7) Section 5

Table 3: Caption is a bit irritating: The values show computed penetration depths for typical values of soil permittivity at the sensor frequencies, isn't it?

Section 5.2 is not very helpful in this form. It gives some insight in the associated uncertainties, missing understanding, and possible sensor problems of OECP. For me, it is also unclear how OECP was operated, manually or in some kind of automatic setting over a certain time. Problems are associated with the extremely large field gradient at the edge of the inner conductor. When it is in contact with the test medium, any heterogeneity of the test medium can excite higher-order modes. This problem was also found in dielectric measurements of vegetation by a similar sensor of El Rayes and Ulaby, 1987 especially at frequencies higher than 8 GHz.

Section 5.3 is not useful in the present form, bad language. Anyway, hard to read.

8) Finally: Without commenting to the end, I am confident that the authors will improve text, figures, tables and insight, leading to better understanding in a revised manuscript.