2023-05 Sandells et al. Review 2023-05-13

General comments:
The objective of the study is not focussed on snow science, but on the search for a better use of microwave radiometer data from satellites in weather forecasting, especially in polar regions. Snow and ice surfaces produce the variable background of the atmospheric emission to be looked for. Different disciplines, dealing with atmosphere and cryosphere join here in a highly intricate way. Fortunately, the authors use up-to-date models (ARTS, SMRT) for simulating sensor signals at frequencies near 89, 118, 157, 183 and 243 GHz, i.e. at wavelengths roughly between 1 and 3 mm. Since snow-structure parameters cover a similar range, volume scattering by snow is dominant and can be highly variable. The selection of these frequencies, however, is not optimum for snow. It was based on the atmospheric properties to be sensed. The atmospheric window channels at 89, 157 and 243 GHz are highly sensitive to liquid-water clouds, to water vapour, and with increasing frequency also to ice clouds, the 118 GHz channels are used for temperature profiling around an O_2 line, and the 183 GHz channels are used for humidity profiling around a strong line of water vapour. Only the wing channels around these lines played a role here. In a future study, window channels at lower frequencies that are optimal for snow should be used as well. When reading the manuscript, it appears that the focus was more on snow than on the overall aspects. No information was given on the atmospheric opacity range at the given channels. Because the airborne system was optimised for the atmosphere, it is not surprising that the results were only suboptimal. Imaging microwave radiometers optimised for the surface use conical scanning with constant incidence angle. The authors found a way out of this problem by limiting the study to radiance from nadir direction. However, this limitation was a trap for various raisons as shown below.
Discrepancies between surface and aircraft observations and changes in observations between different flights were interpreted by small-scale heterogeneity and by temporal variations. Radiometric data presented were taken at an unspecified aircraft altitude. Also, data taken with a surface-based instrument on a sledge at 89 GHz were transformed to flight altitude. I cannot understand how the setup measured the snow surface in nadir direction without distortion by shadowing of the sky and by its own radiation towards the footprint. It is much better to observe at a sufficiently large nadir angle (50° to 60° off nadir, as conically scanning satellite instruments do). Then the distortion by the instrument and its setup can be negligible, and additional information by the difference between vertical and horizontal polarisation can be obtained, e.g. to separate between specular and diffuse scattering. This would help in quantifying the effect of the ice lens. Transformation to nadir direction could still be done approximately by combining SMRT and ARTS. The surface-based instrument should also be used to measure the downwelling sky radiation (tipping curves for calibration, and to determine the zenith opacity of the atmosphere). I missed information on such measurements. Indeed, atmospheric and surface radiation are linked in many ways!

Special comments:
1) Figure 1: I miss geographic location and altitude range. It is unclear how rugged this terrain is, how steep the slopes and therefore how large the topographic effects are.

2) Line 132: How large is the difference between the Rayleigh-Jeans equivalent TB and the physical TB based on the Planck function (especially at the highest frequency used)? Give

some typical examples. The difference between the two starts to diverge with increasing frequency and decreasing temperature.

3) The use of SSA in Table 2 and elsewhere: It would be easier for the reader to get the correlation length in mm (Eq. 1) than SSA in kg/m^2 because the wavelength is in mm, too.

4) The identification of snow pits in Figures 2, 4, 5, 7, and Table 2 is cumbersome when changing between text parts, Tables and Figures. Please use simple numbers from 1 to 29. You still can add things like C2, such as 1-C2 for Pit 1. This is much clearer than A0C2 because all pits are now called A0...

5) Line 179-180: What is the thickness range of the observed ice lens? How does SMRT treat its effects? Do coherent reflections between the upper and lower boundary play a role?

6) Figures 3, 8, 10: Text labels and numbers are too small.

7) Line 204 and 210 "Atmospheric correction": This term is irritating. Tb was adapted to flight height, not corrected. Only errors can be corrected in my understanding.

8) Line 232 (and elsewhere, including the abstract) "Anisotropic atmosphere": What do you mean? An atmosphere that contains anisotropic particles, such as ice crystals? Or charged particles in a magnetic field? Or do you mean anisotropic radiance? It is clear, that radiance varies with direction, even in a plane-parallel atmosphere. Therefore, the tipping-curve method has been used for a long time. But this does not mean that the atmosphere is anisotropic.

9) Line 240: "nadir ground-based TBs". See General comments, above. Measurements may be distorted (mostly enhanced) by the effects mentioned there.

10) Figure 7: I do not understand the box symbols. The key is incomplete.

11) Figure 8: Why not without atmosphere, or with a time-constant atmosphere. It is not clear if the changes are due to the atmosphere or due to the surface.

12) Figure 9 is very helpful because it shows the weather history. Its information could be used to better interpret the radiometer data. The temperature remained below freezing. No changes are expected for the ice lenses. Temperature-gradient metamorphism with slow changes only. The figure also indicates that time series of radiometric measurements at the same temporal resolution might be valuable.

13) Table 4: Text unclear. I don't see any "effect of thin...". I only see numbers. Please clarify. Why are they all negative?

14) Line 330: "This suggests that emission from the atmosphere may dominate ... at 183 GHz". It appears to me that the author did not check the actual brightness temperatures & opacities involved.

15) Figure 10 is very helpful. It is the only part where we clearly see the influence of the atmosphere. However, the analysis and description should be improved, e.g. on Line 333: "the atmosphere reduces the RMSE of the base simulation medians". I cannot see any RMSE in this figure. Do you mean the widths of the distributions shown? Please don't call this an error. And certainly not of the medians. Later, on Line 338, you mention something with respect to the distributions. I was unable do understand this text.

16) Line 345: Upper frequency limit of IBA: There is no fixed limit. The error of the approximation just increases with increasing frequency (and is larger for scattering in backward than in the forward hemisphere). "radius" should be defined, or else replaced by "correlation length".

17) Line 350: "Underlying topography": Do you really mean topography, here? Or dielectric properties of the underlying ground? The topography, in terms of slope steepness, orientation, and the solid angle of open sky above the surface point is relevant at all frequencies. The discussion that follows seems vague and not enough specific to the situations of the study.

18) The discussion on Lines 366 to 370 indicates that the selection of sensors used was not optimal. A mapping sensor with sufficient spatial resolution would have been more helpful, even if it is at a single frequency, such as 89 GHz. As an alternative a movable radiometer on a sledge would also give information on the spatial variability. Of course, this is no argument against the mentioned snow micropenetrometer. Both together would be excellent.

19) Finally, I am surprised about the large standard deviation of all simulated TB values. What is the reason? And how can you get more specific results that better focus on the actual situations?