Interactive comment on "MEMLS3&a: Microwave Emission Model of Layered Snowpacks adapted to include backscattering" *by* M. Proksch et al.

Anonymous Referee #1

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General Comments The topic of this article is a sophisticated model for simulating microwave emission and scattering of snow, which very well balances theoretical issues and practical handling. Such simulation models are essential when developing remote sensing technologies for continuous retrieval of snow properties over larger regions. Although the article is directly related to snow covers on land surfaces, it is useful also for snow on sea ice and for snow/firn of the ice sheets. The paper is very well written and structured and should definitely be published. I have only a few minor comments and questions.

Specific comments

Page 2611, line 4: assumption of Lambertian reflectance: The intensity of surface scattering from ice and snow may reveal a dependence on the observer's zenith angle.

Is the assumption of Lambertian reflectance (i. e. the apparent brightness of a surface does not depend on the observation angle) realistic in all cases? If not, what does this mean for the model simulations? (The text on page 2612 lines 15-19 implies that the Lambertian behavior is a consequence of the dominance of volume scattering?) Please comment.

Page 2612, line 9: How is the constant S_0 determined?

Page 2613: Equation 9 is the "geometric optics" (GO) model (Ulaby p. 983). The Physical Optics or Kirchhoff formulation (Ulaby p. 925-926) includes both GO and the scalar approximation.

Section 2.2. For a reader not familiar with passive radiometry one should mention how two different values of T_{sky} can be obtained in real measurement scenarios? (Referring to section 4.1.3 – use of measurements acquired at different air temperatures?)

Page 2619, line15: Wasn't the snow temperature directly measured during the field measurements?

Page 2620, line 1: referring to the selection of the value of the frozen ground permittivity in line with Rautiainen et al. (2012) – the title of the Rautiainen-article suggests that the permittivity is valid at L-band, whereas here simulations are carried out at much higher frequencies?

Page 2621, Fig. 7 and Fig. 8: the latter repeats the results for CT-input already shown in Fig. 7? I would suggest to remove the CT-result from Fig. 7 and to mention that Fig. 8 specifically shows the "best" result. Why did you select an incidence angle of 50° for this examples? Most satellite radar measurements are typically in the range between 25° and 40°.

Page 2622, lines 3-5: The variations of the backscattered intensity due to fading can be estimated. Considering that the number of independent looks seems to be close to 40, the variations of the backscattering coefficient should be rather small. Can the

authors definitely exclude variations of snow and ground properties? Looking at the results e. g. shown in Figs 9 and 10, this should be discussed more carefully.

Page 2624: I am somewhat uncertain whether I agree that the simulations are in "good" agreement with the SnowScat observations. Maybe "reasonable" is a better word, considering the results shown in Figs. 9 and 10, and also the problems with unknown input parameters (q, m, microstructure correlation function, frozen ground characteristics...)

Discussion: It would be useful to get an impression concerning the individual magnitudes of the surface/interface and the volume contribution (in particular for the case of dry snow).

Is the correlation length really a much better characteristic for quantifying the snow microstructure? The advantages of this approach are well described in the discussion (p. 2627) but I wonder whether situations may occur in which the knowledge of grain size distributions and grain cluster characteristics are better quantifiers of the snow microstructure, at least with regard to microwave scattering (theoretical scattering models based on the permittivity's correlation function also have limitations).

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