

Interactive comment on “Estimation of superficial snow specific surface area from spectral albedo time-series at Dome C, Antarctica” by G. Picard et al.

Anonymous Referee #2

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General comments: The paper is a breakthrough in the observation of long-term surface properties of snow and spectrally resolved albedo over snow. In my view, the paper was not easy to read, as the technical details specific to albedometers and the snow measurements appear Methods, Results and Discussion - a structure which seems to me not very friendly for the reader. A structure where the instrument details, then the snow measurements, and then a discussion would be easier to follow. I suggest to reorganize the paper into two main sections: - Theory, background, instrumental details - Observations, interpretation and comparison

I also found the title not very relevant, a title as "Development and long-term calibration of a new albedometer measuring vertically integrated snow surface SSA " would be

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much more clearly describe what you can expect in this paper.

A second important point concerns the "vertically integrated SSA" (my term). As is clearly shown in Fig. 15, the SSA determined via a spectrally resolved albedometer is in a complex way vertically integrated. As a snowpack is mostly horizontally layered, and often with very strong changes in surface SSA within millimeters (surface hoar, glaze, windcrusts, new snow,...) this point should be considered already in section 2, not in the very end of the paper.

I also found it a bit disturbing that no independent direct measurements of the SSA via stereology or micro-CT were performed. This would make the interpretation significantly more plausible and less speculative, especially concerning the role of the very surface.

As a very significant paper, I would like to suggest to the authors that they reconsider the structure.

As a final point, the peer-review will not be able to validate the method (see p20, L8), I can only check if the methods and procedures are reasonable!

— Further points p 1 L10 The sentence "The comparison of the retrieved SSA with independent measurements made with an optical device operating at 1310 nm confirms the presence of a sharp and recurrent vertical gradient of SSA in the uppermost centimeter at Dome C, which challenges the assessment of the absolute accuracy from independent measurements." seems to me overstated. If the gradient is "sharp" can not be determined by the methods used: either near-infrared photography in a profile would be necessary, or micro-tomography. What is obvious and correct that the upper snow layers are at times of higher SSA than the averaged SSA observed with Autosolex. This is not surprising concerning the calculated penetration depth! The same is valid for p17 L34ff.

p 3 L 27 ff references for the "manual devices"?

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p18 L11 ff: Which spectral irradiance was used ($\text{W m}^{-2} \text{nm}^{-1}$) for the calculation of the averaged penetration depth?

p22 L19 reference to Libous 2013 seems incomplete

p38 Fig. 14 The two datasets are from different depths (as shown in Fig. 15), so they are not really comparable. These data only show that the surface has almost always a higher SSA than the deeper "sampling" Autosolex.

Interactive comment on The Cryosphere Discuss., doi:10.5194/tc-2015-213, 2016.