Review 2

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 re-organize the paper into two main sections:

- Theory, background, instrumental details

- Observations, interpretation and comparison.

We have reorganized the paper to address first the most generic and theoretical points and second the instrument details. After the introduction, a section addresses the theory with 1) a description of the SSA retrieval algorithm, 2) the vertical representativeness of the retrieved SSA and 3) the formulation to investigate the impact of instrument artifacts on SSA uncertainty. The next section is focused on the instrument description, processing description and illustration/uncertainties, and stability. The next section presents the time-series of SSA and the last one provides conclusions and future works. This follows the two points proposed by the reviewer except that the “Theory” and “instrumental details” are split in two sections because they are indeed different and represent 90% of the article.

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

We propose the following title which include the term “time-series” which is the main novelty of the instrument.

“Development and calibration of an automatic spectral albedometer to estimate near-surface snow SSA time-series.”

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.

This has been moved in Section 2 as suggested.

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.

We share this feeling but we do not have such independent measurements at Dome C and we believe it is very difficult to collect near surface snow and apply X-ray tomography (or stereology) techniques to obtain results with a sufficient accuracy in the typical Dome C conditions of high SSA and high vertical gradient in the topmost layer.

We have participated in several intercomparison campaigns over the past years where many measurements techniques have been combined. These intercomparisons took place in Alpine conditions where SSA is lower, snow is more homogeneous and the vertical gradient is probably much smaller than at Dome C. Only in such ideal conditions and after a few attempts, we were able to claim that a reasonable check (or “cross-validation”) of the optically-based SSA measurements has been achieved (not published yet). We hope such attempts could be done at Dome C in the future.

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!

Yes. The sentence is removed.

- 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.

We remove this sentence from the abstract because the paper does not address the issue of the gradient with sufficient details.

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

done

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

Albedo calculations from which we deduce the penetration depth are independent of the irradiance.

p22 L19 reference to Libous 2013 seems incomplete

corrected

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.

This has been addressed with the reogranisation of the article. The vertical representativeness is first described in Section 2. Then the comparison with Asssap in Section 4 only concludes that the SSA is always higher at the surface than slightly deeper.