Interactive comment on "*In situ* continuous visible and near-infrared spectroscopy of an alpine snowpack" *by* Marie Dumont et al.

Anonymous Referee #2

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General comments

The paper presents continuous snow spectral albedo observations over the range 350-1050 nm from an Alpine area. The dataset is extremely valuable and unique, as very few continuous spectral albedo datasets have been obtained so far in the world. The dataset allowed the examination of the diurnal and seasonal evolution of some snowpack properties that affect the albedo (namely SSA, snow impurity content, surface slope, and presence of liquid water) and that can be retrieved from the albedo data thought the inversion of snow albedo schemes. This ambitious goal is of high relevance for remote sensing applications.

The dataset is extremely complex, with uncertainties arising from a large number of sources. The authors have well taken into account this complexity, and have devel-

oped an elaborated method to retrieve the snow properties from the albedo spectra. The study is very innovative, and opens the path for a better exploitation of albedo observations. However, in my opinion the presented methodology is not illustrated clearly enough. It is sometimes hard to follow the reasoning behind the proposed method, or even understand what exactly the method does, and consequently it is difficult to interpret the results. In several parts the text is too cryptic and condensed. A better readability of the paper is a necessary condition for the adoption of the proposed methodology by the scientific community.

I therefore recommend the authors to do a major revision, which should mainly consist in reformulating most of the text in the method section to better clarify the content and provide all the necessary details to help the reader to easily follow the reasoning behind the various steps.

Detailed comments

p.5, line 5 "…thus impose the observed unfolding of the meteorological conditions that is essential to represent dust event". This is an example of too cryptic and condensed text. Could you please rewrite to make the content clearer?

p.5 Sect. 3.1. The content of this section should be formulated much more clearly, and with a more strict logic. In my understanding the following steps were done: 1. ALADIN-Climate forced by Era-Interim data was applied to calculate optically relevant atmospheric quantities (aerosol, ozone, water vapor) in the 50x50 km grid cell that includes Col de Porte, for the whole observational period. Did the authors used a single model cell or made some sort of weighted average? The authors write that the model was used to calculate "mean atmospheric conditions at Col de Porte" (line 16). Does this imply some averaging in time or space was done? Please clarify. The atmospheric profiles obtained from the ALADIN-Climate runs were considered representative of Col de Porte. This is not an obvious passage, as in a mountain area the 50x50 km resolution can be too low and strong differences can exist inside the grid. This problem clearly

appears in Fig 10, but the authors should discuss this issue already in the methodology, and explain the limitations and benefits of the chosen approach. 2. The obtained atmospheric profiles and the locally measured T2m were fed into SBDART to calculate the ratio between Swdir and SwTOT as a function of the cloud optical thickness τ (for the whole observation period?). These results were used to produce a regression equation of τ vs Swdir/SwTOT. 3. The regression equation was applied to calculate the actual τ from the observed Swdir and SwTOT. 4. SBDART was then applied again to compute hourly direct and diffuse irradiance using the derived τ. Is my interpretation correct? If it is so, please describe these steps in this logical sequence. Also, how well the Swdir and SwTOT modelled with SBDART matched the observations? In other words, what is the uncertainty in Eq 1, and how does it propagate to the calculation of the Swdir and Swdiff spectra?

p.6, lines 19-20 "the surface slope is small and local enough not to modify significantly the solid angles under which the incoming and reflected radiations are measured with respect to what would happen for an horizontal surface" Very tortuous sentence, quite difficult for me to understand. Can it be made clearer? Sect. 3.3 is quite hard to follow. The equations to calculate the effect of slope on diffuse radiation and on measured albedo are presented in Appendices A and B, but without explaining many passages, so it is too laborious for me to check them, and it is difficult for an interested reader to apply them without a full understanding. The authors refer to some literature for the details (Dumont et al, 2011, Wang et al 2016), but they need to report in the paper the key concepts and passages to make the paper self-sufficient. For instance, what is the physical meaning of the parameter "K"? Probably also a schematic drawing of the angles of the tilted and horizontal surface would help to understand the equations.

p. 7, line 5: "…and an horizontal surface". Should instead be a "tilted surface"? At the beginning of Sect. 3.3.1. the authors could add a paragraph introducing the strategy applied in the method and the purpose of the various steps.

p.7, line 17: "A seasonal value of A is estimated…" Why do the authors need to cal-

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culate a seasonal value? I can understand it after seeing Fig. 3, but this figure is introduced only in the Result section, so here the authors need to explain why A can vary and why they need to choose a single value. The reader can then better understand the sentence about the propagation of errors (lines 21-22).

p.7, line 19: "To avoid undetermination problem between A and Cimp…". This is a too cryptic and compact sentence, please explain.

p. 7, lines 27-30: I don't understand this paragraph. First of all, how the (albedo?) spectra are calculated (line 27)? I suppose using the retrieved SSA and Cimp. And which model was applied? If Cimp is retrieved with the optimization method, why the discrepancy between observed and calculated albedo (using the retrieved optimal parameters?) should be related to Cimp? In my view, the discrepancy can be attributed to a large number of approximations and assumptions in the applied model.

p.8, lines 4-6: I recommend the authors to explain the motivation of Step 3, which looks unnecessary if they know in advance the slope and aspect of the surface. Also, I would explain here why the slope can change (with precipitation and snowdrift).

p.8, lines 9-10: How is the filtering of the spectra done? Do the authors remove the spectra that show a specific features in the moving window average?

p.8, lines 18: "…only slightly lower than the ideal value of one". This is not a correct expression, as in my understanding A=1 is not an "ideal value" of A, but rather it indicates that the instrument had an ideal response. So, the concept would better be expressed by stating that a value of A close to 1 indicates that the measurement apparatus has caused only a small deviation from the ideal response of the instrument.

p.8, line 20: please remove "presence of"

p.8, lines 25-26: "… diurnal cycle with higher values in the morning and lower values in the afternoon". From Fig 4a I see that K is higher in the afternoon! Please consider redrawing Fig 4a, with just 24 hours in the x-axis to show better the diurnal cycle, and

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maybe the time series of K in just two selected days, one with good agreement and one will poor agreement.

In Fig. 4b, please replace the symbol "âůę" with "(degrees)" in the y-labels for slope and aspect.

p. 9, line 2: "The seasonal evolution of slope and aspect seems to be related to snow evolution". It has to be, what else could cause a change in slope and aspect? So, I would replace "seems to be" with "is".

p. 9, line 21: "have been done under" should perhaps be "have been obtained under".

p. 11, lines 14-16: The text is too difficult to follow. The authors need to provide some interpretation, and also explain the applied relationship between SWE and snow density.

p. 12, line 1: "2014-04-10" should perhaps be "2014-04-02"?

p.12, line 8-9: "The grey area corresponds to the 15% uncertainty estimated in Picard et al. (2016a)". Please clarify what this uncertainty refers to. In Fig 9c it appears as a constant value throughout the observational period rather than a percentage of SSA (which varied in the range ∼5-60 m2/kg).

p.12, lines 24-29: I did not understand this paragraph. The authors write "If the RMSD is larger than the RMSD over the whole spectrum, Cimp is represented in red". What is the relevance of this? And what is the reasoning behind the application of this criterion? The message of this paragraph is totally unclear to me.

Fig 9: In panel (a), I think that morning and afternoon symbols are inverted.