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

Anonymous Referee #1
Received and published: 22 December 2016

Authors responses are enlighten in blue. Proposed changes in the manuscript are reported in bold.

General comments:
This paper provides a very sound study on the determinations of several snow surface proprieties such as snow specific surface area (SSA), effective light-absorbing impurities content and presence of liquid water, based on spectral albedo measurements. The authors well describe the Methods and theoretical framework for analyzing the aforementioned effects depending on snow albedo. The paper also builds up on recent studies in this field (e.g. Picard et al.). The methodological framework of handling the albedo data is quite elaborated including atmospheric model outputs and using several assumptions. I assume that these methodological framework is constructed to investigate the relationship between snow albedo and the snow cover proprieties.

The authors are thankful for this useful review of the manuscript. All comments have been accounted for, responses and proposed modifications are described below after each comment.

To my opinion, the authors should analyze and present a bit more of the raw albedo data in order to explain the methods used. For me it is unclear why the authors are estimating the ratio of the diffuse and direct irradiance. To my knowledge, the albedo is defined as the ratio of the incoming and reflected global radiation. Both are affected by either direct sunlight or anisotropic reflectance depending on solar zenith angle (and cosine error of the entrance optics etc.). I assume that the authors take into account theses effects by using atmospheric model outputs and the methods describes in the paper.

The direct to total spectral irradiance ratio is required to perform the cosine response correction. It is also required to analyse the snow surface parameters and the effect of slope on the albedo measurement. The effects of the cosine receptor angular response and of SZA are taken into account in the methodology. The effects of the anisotropy of reflected and diffuse incoming radiations are neglected (see details in the response to the specific comment 3).

However, it would be very useful and maybe simpler, to analyze the diurnal course of the snow albedo depending on solar zenith angle (SZA), when a minor change of the snow surface proprieties can be assumed. Most likely the spectral albedo shows a dependence on SZA – also depending on the wavelength selected, however, this dependency should be similar for all days of the season. If such a relationship can be found, all data of the days can be normalized to a reference SZA and then used for the comparison with the specific snow surface proprieties such as snow specific surface area (SSA), effective light-absorbing impurities content and presence of liquid water. This is in particular important for days with partly direct sun impact and partly cloudy in order to select either only direct sun albedo or cloud covered albedo. In this respect, it would be very useful the show the diurnal course of fully cloud albedo and to compare these effects with direct sun albedo. Due to this method, all SZA depending effects (e.g. cosine error of the entrance optics, slope of the surface) may be removed, which may allow to find a clearer signal between snow cover proprieties and spectral albedo.

In summary: I suggest to analyze and present some data of daily albedo depending on SZA and

different cloud conditions and to discuss shortly these effects in regard of SSA, LWC and impurities just based on the measurements before presenting the analysis including model calculations.

Thanks for these thoughts on the methodology. "Most likely the spectral albedo shows a dependence on SZA – also depending on the wavelength selected, however, this dependency should be similar for all days of the season." The dependency on the SZA (ignoring the effect of snow properties) is not exactly similar for all days of the season because of the changes in the diffuse to total irradiance ratio, i.e. changes in atmospheric profiles and cloudiness and also because of the changes in the slope and aspect of the surface (due to precipitation, wind transportation and melt).
In response to this comment, we have added the figure below that shows some examples of the raw albedo diurnal cycle for a cloudy day and a clear sky day.

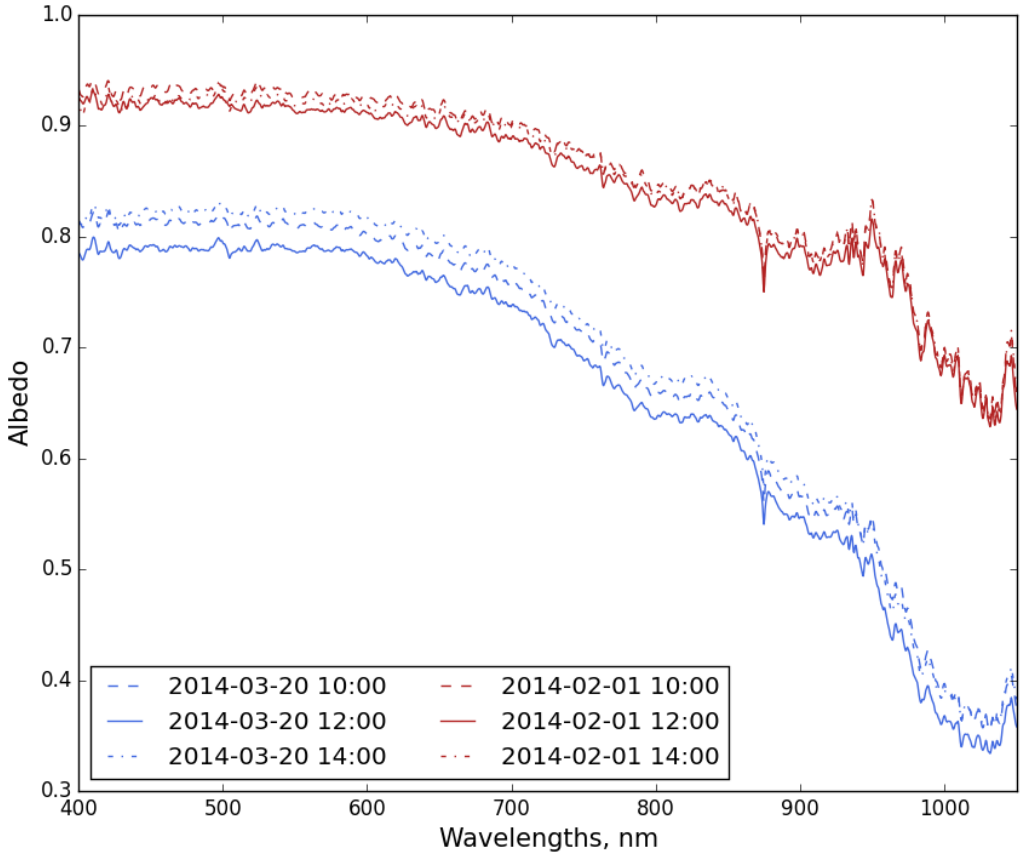

Figure 3 : Raw measured albedo for a clear sky day (blue lines, 2014-04-03) and for a cloudy day (red lines, 2014-02-01) at 10:00 (dashed lines), 12:00 (solid lines) and 14:00 (dash-dotted lines).

A short description of the Figure has been added page 7 line 12.

Figure 3 illustrates the raw measured albedo diurnal cycles for a cloudy day (red lines) and for a clear sky day (blue lines). As expected from Eq. 8, the diurnal cycle is more pronounced for the clear sky day, the albedo evolution being non-symmetric with respect to solar noon probably due to both slope and changes in snow properties effects.

The submitted paper is well written and organized and the methods and data are fully described. The paper can be published with minor revision requested.

Specific comments:

1 - Section 2.1: Some more details about the spectral albedo measurements are needed:

Agree. The details listed below in blue have been added in section 2.1. (see below)

- What is the height above snow surface of the two entrance optics, was the height constant over the season? Or was the height changing with growing snow cover? What would be the effect of the different hights.-

The height of the upward looking head is 2.4 m above the ground (with no snow) and 2.1 m for the downward looking head. The height is changing with the growing snow cover, the main effect of this varying height is the change in the surface seen by the sensors.

These details have now been added in the text page 3 line 28 :
"one albedo head. **The upward (downward) optic is set up 2.4 (2.1) m above bare soil. The height of the albedo heads and consequently, the field of view of the sensor, are thus varying with snow depth.** The device …"

What is the effect when the entrance optics where changes (from up to down and vice versa)? Was that tested? This would give a hint of the expected uncertainty of the albedo measurement, including all effects such a cosine error.

The entrance optics are cross-calibrated and this is taken into account in the collector cosine response correction (please see response to comment 2 for details and proposed modifications in the paper).

- Spectral resolution of 3 nm: Do you mean the spectral bandwidth or the spectral sampling rate?

We mean spectral bandwidth.
Page 3 line 29 has thus been modified as follows:
"350-1050 nm range with an effective spectral resolution**, i.e. spectral bandwidth,** of 3 nm."

- Are the domes heated?
Yes, and ventilated
This detail has been added page 3 line 31 : "Consequently, the device was cleaned up manually after each snowfall **although both the upward and downward looking domes are heated and ventilated**. The snow surface … "

2 - Page 4, line 4: (iv) correction of the angular response: How did this correction applied. Was the correction applied for both entrance optics? Are the entrance optics similar in respect of the cosine error?

As stated in the text (page 4 lines 3-5), the correction of the collector angular response is performed exactly as extensively detailed in Picard et al., 2016a (section 3.3.4). The correction is applied only to the direct component as the diffuse component has already been calibrated at the cross calibration step. The correction is thus applied only to the upward looking entrance optics. The cross calibration step consists in measuring successively the upward and downward channels under the same illumination conditions (see section 3.3.3 in Picard et al., 2016a) and allows accounting for the differences in the two entrance optics cosine errors. The cosine errors of the two entrance optics are of the same order of magnitude.

The text of the paper was consequently modified as follows page 4 line 4 :

'…(iv) collector angular response. **The correction of the collector angular response was applied in two steps (i) cross-calibration of the two entrance optics under the same illumination conditions and (ii) cosine response correction on the direct component of the incident radiation as detailed in Picard et al., 2016a (sections 3.3.3 and 3.3.4).** The corrected spectra ….'

3 - Page 6, line 18; Can the reflected radiation on snow be assumed as isotropic. Is there any reference for this assumption? Most likely the reflected radiation also shows a dependency on SZA.

The radiation reflected by the snowpack is not isotropic (e.g. Dumont et al., 2010) and the anisotropy of the reflected radiations indeed varies with SZA: the anisotropy is stronger for higher SZA. The anisotropy of the reflected radiation impacts the cosine correction as detailed in Carmagnola et al., 2014. The effect of this anisotropy on the spectra correction is of second order as long as the cosine response correction is small, which is the case for our device.

This is discussed page 7 lines 8-12 and also in the conclusion page 14 line 3.

4 - Page 7: line 6. Maybe there is a strong dependence of the scaling factor depending on SZA. Was this analyzed? Maybe this explains the distribution of the scaling factor A in Figure 3.

The distribution of scaling factor A in Figure 3 was obtained during fully cloudy days, the SZA should not have much impact on the spread. We didn't analysed in detail how A can varies with SZA, indeed we assume that most of the variations of the signal with SZA are due to slope and albedo effect (Eq. 8). Due to the number of unknowns in Eq. 8, it is quite difficult to disentangle the effect of SZA on A, K and albedo.
To account for the referee comments we modified the text page 8 lines 1-2:
"Note that by using a seasonal A value for every spectra, we assume that the measurements artefacts are the same under cloudy and clear sky conditions **and for varying solar zenith angles.**"

Smaller issues:
The abstract basically describes the intention and results of the study – no changes.
The text is well written and no major typos have been detected so far..

Thanks !