The importance of the snow spectral albedo and integrated water vapour

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The revised version of the discussion paper: “A module to convert spectral to narrow-band snow albedo fro use in climate models: SNOWBAL v1.0” by C. T. van Dalum, W. J. van de Berg, Q. Libois, G. Picard and M. .R. van den Broeke will here be reviewed. I was anonymous reviewer #3 of the original submission, but will here not be anonymous since this review contains orginal material.

This review has been challenging to make since the authors on one hand have made a revised manuscript which addresses the issue of atmosphere surface coupling of snow spectral albedo at a level, which is higher than what has been included so far in other climate and weather models. On the other hand, there are still major issues with the manuscript, and the authors failed to follow many of the advices I gave in my first review. These are listed below with the same comment numbers as in the first review.

1b: It is an error that the authors have run their simulations with a spectrally constant albedo of 0.5. In my review I suggested to run with actual snow spectral albedos in stead - for instance from TARTES (Libois et al., 2013). The authors have not done so and replied:

> “Allthough you are right that the chosen background albedo in DISORT is not very elegant, it turns out to have only a very limited impact on the representative wavelength and consequently on the narrowband albedo, . . . ”
>
> “In Figure 4, we show that even if extreme values like 0 or 1 for the DISORT surface albedo are taken, that it matters insignificantly on the end results.”

Here it appears that the authors have only tested running with spectrally constant albedos of 0 and 1. They fail to understand that it is the spectral variations of the albedo that are important for the representative wavelengths that they seek to determine. In Fig. 1 the importance of accounting for the snow albedo spectral variations is illustrated.

These spectra are computed with the libRadtran/DISORT package (Mayer & Kylling, 2005; Stamnes et al., 1988), which is also used by the authors. The 1

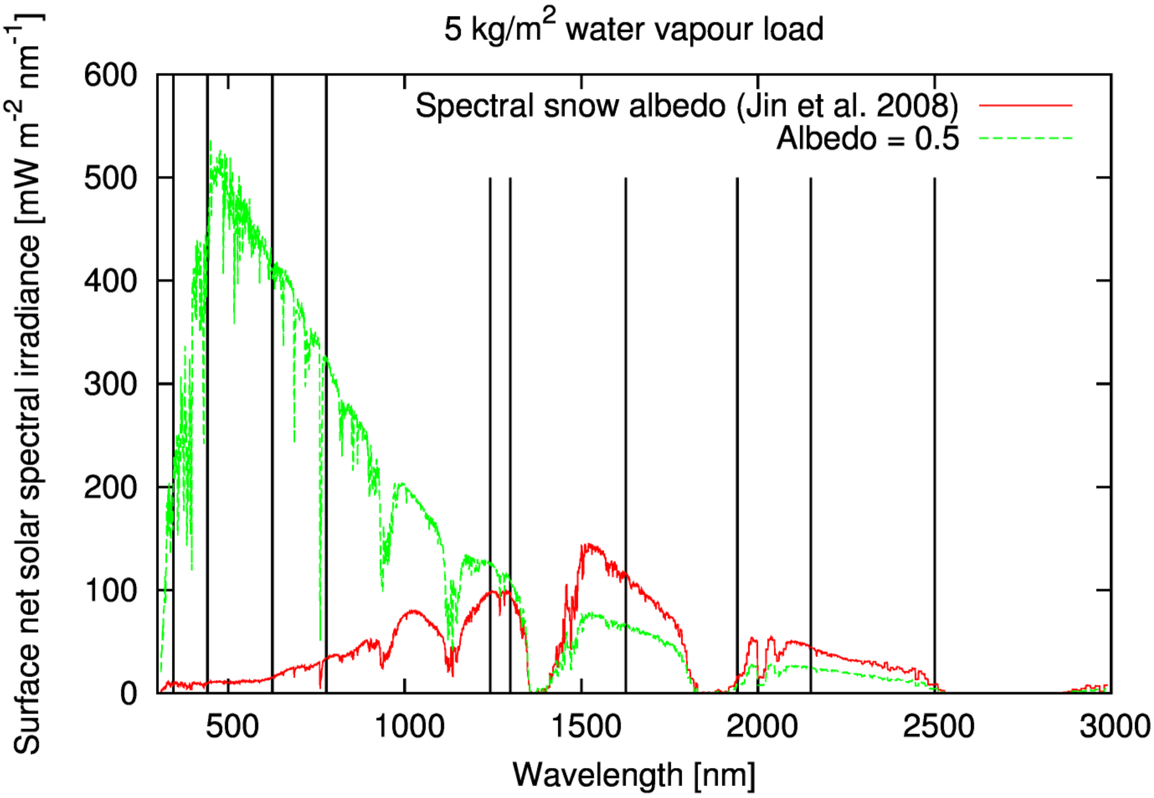

Figure 1: Surface net solar spectral irradiances computed for an altitude of 1.0 km, a solar zenith angle of 56°, and an atmospheric water vapour load of 5 kg/m^2. The green curve shows the net irradiances for a constant surface albedo of 0.5. The red curve shows the net irradiances for the Jin et al. (2008) albedo spectrum of aggregate snow particles. The vertical lines show the wavelength band divisions between the spectral bands of the IFS 14 band shortwave radiation scheme.

nm resolution solar spectrum of Kurucz (1992) is used together with the psedo-spectral LOWTRAN/SBDART option (Pierluissi & Peng, 1985; Ricchiazzi et al., 1998). The ozone cross sections are given by Bass and Paur (1985). The default aerosols of Shettle (1989) are used with the AFGL subarctic summer atmosphere (Anderson et al., 1986). The atmospheric water vapour load is scaled to 5.0 kg/m^2.

In one of the simulations shown in Fig. 1 (the green curve) a constant spectral albedo of 0.5 is used. In the other simulation (the red curve) a typical snow albedo spectrum is used. Here a spectrum from Jin et al. (2008) for aggregated snow particles is used. Note that the net solar spectral irradiance at surface level is shown rather than the downward solar spectral irradiance, since it is the net irradiance that determines the snow melt. The 14 IFS shortwave (solar) spectral bands are marked in Fig. 1 with black lines - or at least those of these spectral bands that are within the spectral range from 300 nm to 3000 nm. The major difference from using an actual spectral snow albedo, rather than just assuming the snow to be grey, on both the full spectrum and the band representative wavelengths is clear to see; using a constant albedo, as the authors have done is wrong. This needs to be fixed!

1c: In my initial review I questioned using the AFGL "subarctic winter" standard atmosphere (Anderson et al., 1986). I pointed out that the clear sky spectrum changes quite a lot depending on the gases and aerosols present and suggested for instance using atmospheric profiles from the Copernicus Atmospheric Monitoring System (CAMS) dataset. The authors replied:

> "Using atmospheric data profile from the CAMS reanalysis for Greenland might be more typical, but will not result in any significant change in the results."

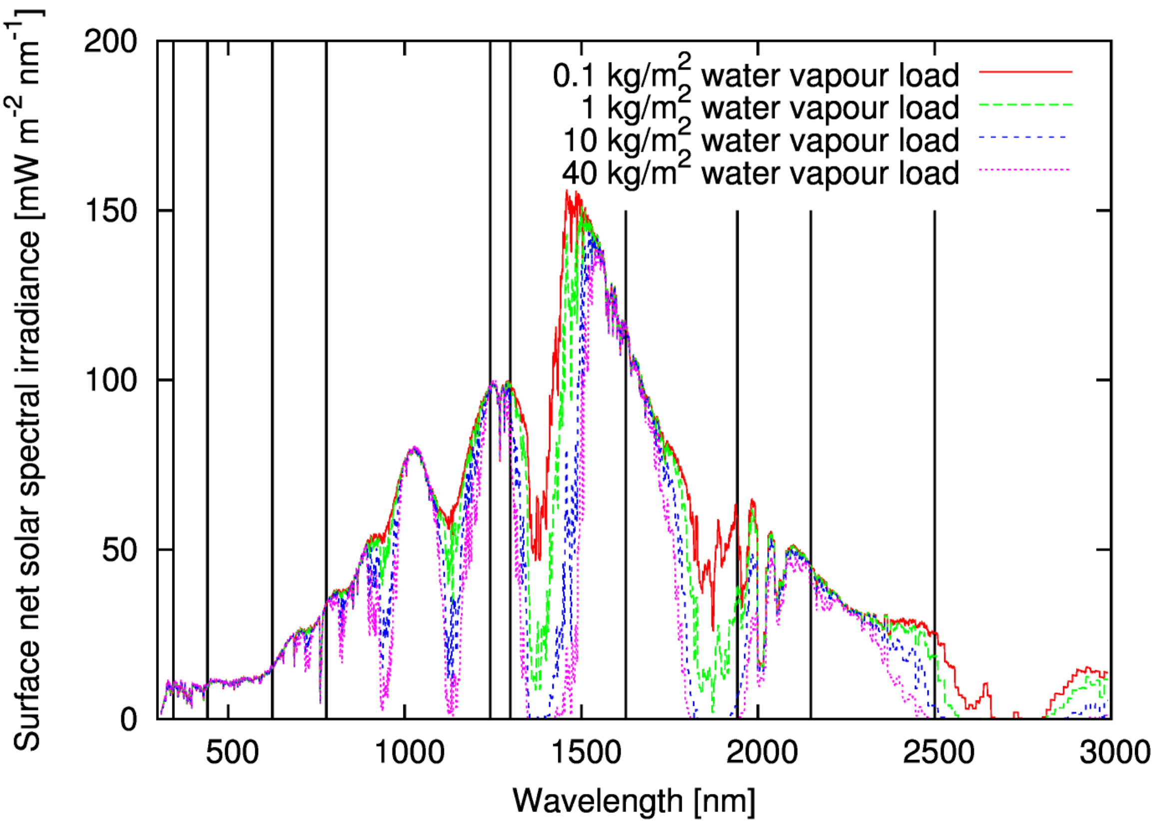

Figure 2: Surface net solar spectral irradiances computed for varying atmospheric water vapour loads. The Jin et al. (2008) albedo spectrum of aggregate snow particles is used. Otherwise, the computations and the figure have been made as Fig. 1.

That conclusion is just wrong and something must be wrong with the methodology of the authors when they find no significant change in their results for different atmospheric profiles. Here it suffices to test the solar spectral irradiance variations as a function of the atmospheric water vapour load, since water vapour is the primary gas affecting the solar irradiance. In Fig. 2 simulated surface net solar spectral irradiances are shown for atmospheric water vapour loads of 0.1 kg/m^2 (red curve), 1 kg/m^2 (green curve), 10 kg/m^2 (blue curve) and 40 kg/m^2 (magenta curve). The Jin et al. (2008) snow albedo spectrum is used. Again the IFS shortwave spectral band boundaries are marked on the figure with black lines. It is clear that if representative wavelengths are to be chosen for several of these bands, these must be different when the water vapour load changes.

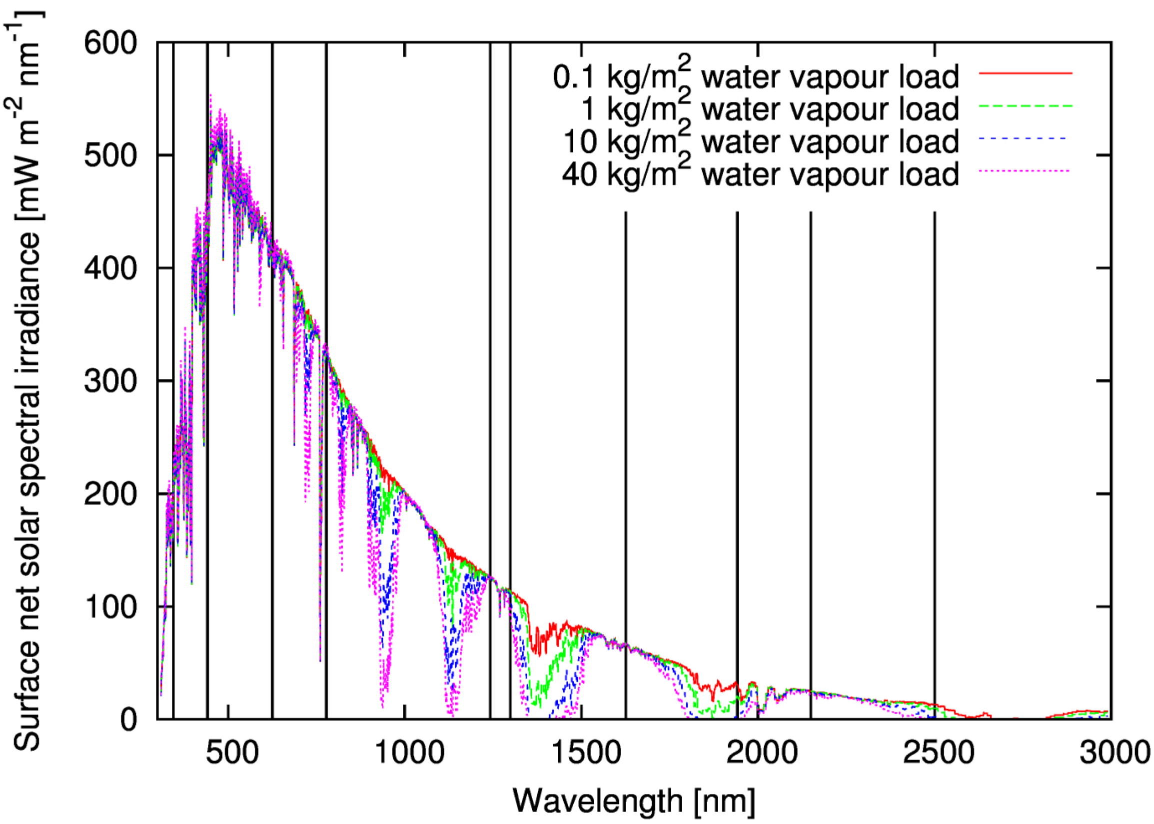

Figure 3: As Fig. 2 but for a constant surface albedo of 0.5.

This is also the case if a constant spectral albedo of 0.5 is used as shown in Fig. 3, so something is wrong when the authors find no significant changes when they run their simulations with a different atmospheric profile.

In Fig. 4 the cumulative distribution function of hourly atmospheric water loads from the month of July 2012 for a region covering Greenland is shown. In the figure the water vapour loads of the 6 AFGL atmospheric profiles are also marked

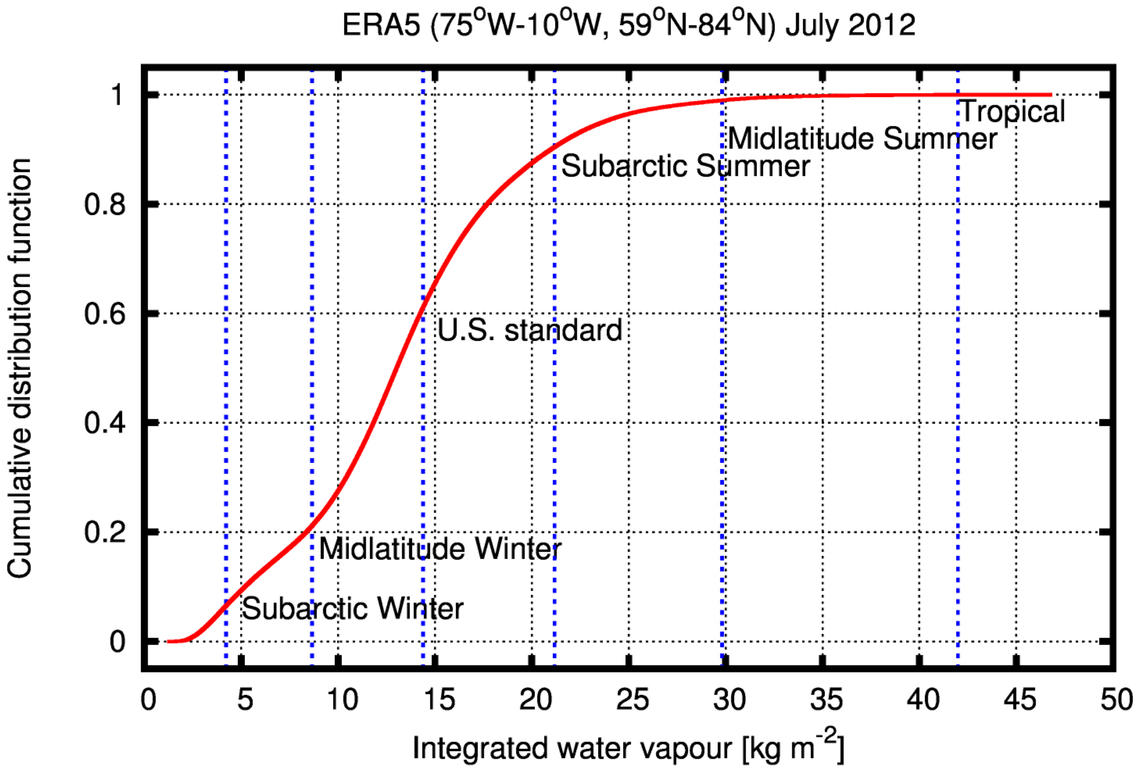

Figure 4: The cumulative distribution function of hourly atmospheric water loads from the ERA5 reanalysis dataset (Hersbach & Dee, 2016) for the month July 2012 and the longitude-latitude region between 75°W to 10°W, and 59°N to 86°N. This region covers Greenland and the surrounding areas. In the figure the water vapour loads in the AFGL standard atmospheres are marked with blue dashed lines.

with blue lines. In order of increasing water vapour load these are: Subarctic winter, midlatitude winter, U.S. standard, subarctic summer, midlatitude summer and tropical. The figure illustrates how variable the atmospheric water vapour load is in the Arctic.

To sum up the above results show that in order to simulate the shortwave (solar) contribution to snow melt accurately, the spectral effects of varying the atmospheric water vapour load must be accounted for!

Here it should be noted that the effects of water vapour on the solar spectrum have been known for a long time (e.g. Abbot, 1911), but there appears to have been a lack of awareness on this issue for snow albedo modelling. Thus, it is also not accounted for in the study of Gardner and Sharp (2010).

1d: The authors disagree with my comment that the effective radius of ice clouds that they assume of 20 μm is too low:

> "We do not agree with your statement that the effective radius of ice clouds that have taken is too low for the Arctic and that we should use 50 μm instead. According to the following sources, the radius of ice clouds in the Arctic is typically between 10–30..."

...and then they list several papers including Fu (1996). Just checking the generalized mean effective sizes (D_{ge}) of cloud ice particles given in Table 2 of Fu (1996) shows this to be wrong. In this cloud ice particles of up to $D_{ge} = 130$ μm are listed, which corresponds to an effective radius of 85 μm. Also, in the source code of cloud ice effective radius used in the IFS cy33r1 radiation scheme, as used in the RACMO simulations (`.../phys_ec/radlswr.F90` lines 432-483), larger cloud ice effective radii are defined depending on the namelist integer variable `NRADIP`. Thus, if `NRADIP = 0` the cloud ice effective radii are fixed at 40 μm; if `NRADIP = 1` the minimum cloud ice effective radius is set to 40 μm; if `NRADIP = 2` or `NRADIP = 3` the minimum cloud ice effective radius is set to 30 μm. Thus, unless the authors have modified the IFS cy33r1 radiation scheme, their claim here is inconsistent with their own source code!

These three major issues need to be addressed properly for the paper to be accepted. The other major and minor issues that I brought up in my first review have been addressed to a satisfactory level.

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