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Geoscientific
Model Development
Discussions

Interactive comment on "A module to convert spectral to narrowband snow albedo for use in climate models: SNOWBAL v1.0" *by* Christiaan T. van Dalum et al.

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Referee comment response on the manuscript: A module to convert spectral to narrowband snow albedo for use in climate models: SNOWBAL v1.0 by C.T. van Dalum et al.

We would thank the reviewers for their constructive comments that have improved the accuracy of the calculations and the clarity of the paper. For an easier version to read of the referee comment response, see the supplemented pdf file, where the following colors are used: in black the comment, in orange the response, in blue the changes.

Review #1

Comment 1: A more appropriate reference than Dumont et al.(2014) for P1, L21-23 should be sought. The Dumont paper was really about the potential impact of impurities on snow albedo and hence melt, and subsequent research suggests that its conclusions were not correct (Polashenski et al., 2015).

You are right and we have changed the reference to Van As et al, 2013.

Comment 2: The study mentions that a spectral approach is important for examining the impact of impurities upon snow albedo in the abstract, but later on (page 14, L13-17), states that the effect of dust is not considered as TARTES uses the delta-Eddington approach (P5, L7) as opposed to Mie scattering. This seems a fundamental issue and so I wonder if the study would benefit from an additional paragraph, perhaps in the introduction, which outlines the different optical approaches and what they permit in terms of albedo modelling. Reference to Cook et al. (2017) may be useful here.

We have added extra explanation in the introduction to include what you have mentioned. Page 2: Impurities mostly affect the reflectivity for near-UV and visible light, while snow metamorphism mostly affect the reflectivity for near- IR light (Tedesco et al., 2016). The grain radius of impurities determines the scattering regime. The typical grain radius of soot and humic-like substances (HULIS) are small compared to shortwave wavelengths, while the typical grain radius of dust is not small. Consequently, an albedo model has to be compatible for Rayleigh scattering to incorporate soot and HULIS, and for Mie theory for dust and biological material (Tegen and Lacis, 1996; Cook et al., 2017). Page 15: The effect of dust is not considered, because Mie scattering is not implemented in this version of TARTES âĂČ

Review #2

Comment 1: TARTES is based on a simple approximation. Therefore, it should not be a problem to run it for multiple wavelengths. Please, give the estimation of time needed to produce the spectral albedo as shown in Fig.1. Please, explain in the paper why you need to make the calculations of snow albedo at hundreds to thousands wavelength using TARTES. I guess, the spectral snow albedo as shown in Fig.1 can be calculated on a fixed spectral grid (say, 30-50 wavelengths) and supplemented with a simple interpolation routine to derive it on yet another grid needed for the integration with the solar irradiance as shown in Fig.1. Could you show errors of this simple approach suggested by me in the paper.

It would likely be possible to reconstruct the spectral albedo of a snowpack for a certain SZA with a limited number of wavelengths, as suggested. Within the SNOWBAL framework, calculation time is of lesser importance. Therefore, we did not see the necessity to optimize the spectral albedo calculations. The time it takes for TARTES to compute an albedo increases quite significantly with the number of wavelengths taken. For example, for a 50 layer snowpack, the Python version of TARTES only takes 0.007 seconds to compute a spectral albedo for 12 wavelengths, while it takes about 0.268 seconds for 1000 wavelengths, which is about 38 times longer.

The reason not to adapt this approach for modeling albedo in an RCM is that an albedo curve can only be appropriately used if the spectral distribution of incoming irradiance is available with sufficient spectral detail too. And that is not the case as RACMO provides the irradiance in 14 rather wide bands. Hence, even with an efficiently derived fully spectral snow albedo, it would not be possible to estimate the snow albedo accurately within RACMO as sub-band energy fluxes are essential but unavailable. Therefore, we discarded this idea in an early phase of the project. Page 6: this would lead to a significant numerical burden, although it is likely possible to parameterize this spectral curve using in the order of thirty well-chosen spectral albedos. Page 6: However, RACMO2 does not compute sub-band energy fluxes. Hence, even with an efficiently derived fully spectral snow albedo or with smaller spectral bands, it would not be possible to estimate the snow albedo accurately within RACMO2 as sub-band energy fluxes are essential, but unavailable. Therefore, we discard this approach.

Comment 2: Please, change: 'geometric asymmetry parameter' to 'geometrical optics asymmetry parameter' (see also p.21). To be more clear, please, acknowledge in the paper that the total asymmetry parameter g=(1+g_G)/2 for nonabsorbing particles. Please, explain in the paper why B/(1-g_G) must be equal to the corresponding value for spheres.

In the ART formalism, snow optical properties depend on snow SSA, g, B and snow density (all single scattering properties can be derived from these properties). Practically, people have been using spheres to represent snow. The parameters B and g can be computed for spheres. This approach proved quite successful for albedo computations (which is why it is widespread), but much less for transmittance or penetration depth simulations. Albedo depends on B/(1-gG) while penetration depth depends on B*(1-gG). Libois et al., (2014) developed a method to determine B, and demonstrated at the same time that g cannot be determined based on optical measurements - it is coupled to SSA. This means g must be assumed somehow. The relative success of spheres means that any shape such that B/(1-gG) equals that of spheres should be quite efficient for albedo simulations. Hence the best estimate of g would be such that B/(1-gG) equals the value for spheres. Page 5: …and grain shape is determined by a geometrical optics asymmetry parameter gG, with the total asymmetry parameter g = 1/2 (1+gG) for non-absorbing particles, and an absorption enhancement parameter B. Page 5: The parameters B and g can be computed for spheres and prove to be quite successful for albedo calculations (Gallet et al., 2009, Grenfell and Warren, 1999), but much less for transmittance or penetration depth simulations. Libois et al., (2014) demonstrate that g cannot be determined based on optical measurements, because it is coupled to SSA, and must be assumed somehow. The relative success of spheres for albedo calculations, which depends on B/(1-gG), means that any shape such that B/(1-gG) equals that of spheres should be quite efficient for albedo simulations. Hence, the best estimate of g would be such that B/(1-gG) equals the value for spheres.

Comment 3: Fig.11, TARTES albedo drops at high SZA. Please, explain the reason for this. I guess, this is not a correct behavior. Are you aware of experimental results which confirm such a drop in albedo? I would suggest to make a plot of BBA as function of cos(SZA).

To clarify what we meant, we have added an extra line in Figure 11 that shows the albedo of TARTES weighted with the energy flux for a fixed angle at SZA = 53 degrees. The point is that the albedo of TARTES increases with SZA, but that this effect is negated if the spectral albedos of TARTES are weighted with DISORT. The energy fluxes provided by DISORT shift more toward longer wavelengths for which the albedo is low. Therefore, if the spectral albedo calculated by TARTES is converted to a broadband albedo, the albedo does not increase as quickly as one would expect if you would use a fixed energy flux (compare the solid black line with the dashed line in Figure 11). We have changed our method from using DISORT to the pseudo-spherical SDISORT, for which the effect is less pronounced, but still visible. We have also tried to clarify this more clearly in the text: Page 19: This spectral shift is not or not sufficiently included in PKM and RACMO2 (Figure 11). For high SZA, DISORT models a clear spectral shift towards longer wavelengths, limiting the increase of the broadband albedo. If this effect is left out (black dashed line) the broadband albedo is much higher. Hence, the difference between the black solid and dashed line indicates this albedo decrease is not induced by the RW-approach, but by general red-shift in the incoming radiation. Page 20: The spectral albedo of TARTES is weighted with energy fluxes derived with DISORT (in black, solid line) or with the energy fluxes valid for a SZA of 53 degrees (black, dashed line) to compute a broadband albedo. Page 23: For clear-sky conditions during winter, i.e. large SZA, we show that the spectral shift towards larger wavelengths has substantial impact on the albedo, resulting in an albedo decrease.

Comment 4: Please, change ’assymetry’ to ’asymmetry’

Done

Review #3

Comment 1a: DISORT is here run with 6 streams. Have you tested if that is enough to get the desired accuracy? If not, you should do so! Quote from Stamnes et al. (2000): "For strongly forward-peaked phase functions it is difficult to get accurate intensities with fewer than 16 streams, and even with 16 streams accuracy can be poor at some angles. Thus, careful users have been forced to use 32 or even 64 streams to be sure of getting 1% accuracy" Stamnes et al. here speak of intensities and not fluxes for which less streams are required, however, I expect that 6 streams are too little also to obtain very accurate fluxes. Tests should be done particularly for high solar zenith angles (SZAs), as these often occur in Greenland.

We thank the reviewer for this suggestion. After some tests, we conclude that the impact of the number of streams taken is very limited for this study. Nonetheless, we decided to rerun DISORT, so we now use 32 streams and some of the other suggestions that you have made. All results and figures now include DISORT run with 32 streams, even though the results and conclusions are hardly altered. Page 5: Thirty-two streams, i.e. computational polar angles, are used to solve the radiative transfer equation (Stamnes et al., 2000).

Comment 1b: In the DISORT simulations the surface broadband albedo is set to 0.5. In the supplementary scripts, it can be seen that this is done regardless of wavelength with the "albedo" input option in libRadtran. Here, the "albedo_file" input option should have been used, in which spectral albedos can be specified. In order to make the atmospheric and snowpack radiative transfer computations consistent, the TARTES spectral albedos should be used in this albedo file. As shown by for instance Nielsen et al. (GMD, 2014), the downward fluxes at the surface are not independent of the albedo. Given the complex variations of spectral irradiances and albedos shown in Fig. 1, it seems important to run DISORT with the TARTES albedos. Fig. 1 is a very illustrative figure by the way! An even better representation of the surface reflectance could be obtained by running coupled DISORT simulations for both the snowpack and the atmosphere. This can be done by adding the spectral inherent optical properties of the layers of the snowpack as the lowest model layers in DISORT. In this way the full BRDF of the snow surface will be properly represented and coupled with the atmospheric simulations, which cannot be done with the two-stream TARTES simulations.

Although 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, as we have stated in section 2.3: “The surface broadband albedo of DISORT is set to 0.5, but as shown later, the sensitivity of both the surface broadband albedo and the aerosol load to the results is low”, as well as in Figure 4. 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 result. One could introduce a more sophisticated surface albedo for DISORT as you propose, but in the end it will matter little, because it will certainly be in between the two extreme cases shown in Figure 4. Also, uncertainties in what surface albedo file to use if a more sophisticated profile is used.

Comment 1c: The "subartic winter" atmospheric profile is used. The reference describing the details of this is missing and should be added. I assume that this is one of the AFGL standard atmospheres of Anderson et al. (1986). Additionally, the "rural aerosols" of Shettle (1990) are used. How representative are these profiles for Greenland? Could typical atmospheric profile data from the CAMS reanalysis be used instead? The clear sky spectrum can change quite a lot depending on the gasses and aerosols assumed to be present. You should mention this uncertainty in the method chosen.

The uncertainties on the narrowband albedos of aerosols are small and are shown in Figure 4 for the extreme case of no aerosols, as well as for subarctic summer. More profiles have been tested, but omitted for clarity, because the weighted RMSE is similar. 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. The subarctic winter is indeed one of the AFGL standard atmospheres, and the reference is added

accordingly: Page 6: For the runs presented here, a subarctic winter atmospheric profile is chosen, which is one of the Air Force Geophysics Laboratory (AFGL) standards (Anderson et al., 1986).

Comment 1d: When inputting liquid and ice clouds to DISORT you assume these to have effective/equivalent radii of 10 μm and 20 μm, respectively. Here the former number is reasonable, but 20 μm is a very low number for typical ice clouds, where I would suggest using 50 μm instead. Also, you make these look-up table values a function of the cloud optical thickness rather than the cloud liquid water path (LWP) and ice water path (IWP). Since the cloud optical thickness is proportional to LWP+IWP and approximately inversely proportional to the effective/equivalent radii, similar relative changes in these cloud properties cause similar changes to the cloud optical thickness.

We do not agree with your statement that the effective radius of ice clouds that we 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 (Stubenrauch et al., 2013; Fitzpatrick et al., 2004; Mahesh et al., 2001; Fu, 1996, Key et al., 2002, King et al., 2004). Therefore, we have taken 20 μm to work with. We have added these references in the paper: Page 6: . . .which are realistic radii for clouds in the Arctic (Stubenrauch et al., 2013; Fitzpatrick et al., 2004; Mahesh et al., 2001; Fu, 1996, Key et al., 2002; King et al., 2004)

Regarding cloud optical thickness, see response of comment 2.

Comment 1e: In the DISORT experiments a range of cloud ice water path of up to 5 kg/m2 is simulated. This is at least 10x more than a realistic maximum value for clouds over Greenland. Cloud liquid water paths of up 40 kg/m2 are also simulated. This is also an order of magnitude higher that cloud water paths that can occur even in the tropics. I suggest that the simulations are done for more realistic ranges.

We are aware that the last elements for both IWP and LWP are very high, but we decided to do this for two reasons. Firstly, we also want RACMO2 to be able to work with glaciers in lower latitudes, therefore we have extended the lookup tables to larger values than would be necessary for Greenland. Secondly, we want RACMO2 to always have some representative wavelength, even if it represents a non-physically high LWP or IWP. RACMO2 is therefore not expected to reach such high values for Greenland. Also keep in mind that this paper is to give an expression on how to couple spectral to narrowband albedos, and the lookup table shown in Figure 9 is only an example of how a final lookup table would look like. If our method is applied to another model, other values of IWP and LWP might be necessary. The following has been added in the paper accordingly: Page 17: . . . of the most similar conditions. The lookup table of Figure 9 contains high values of IWP and LWP to allow RACMO2 to be run for lower latitudes and to ensure that RWs are always calculated, even if RACMO2 would produce unusually thick clouds.

Comment 1e continued: Also, the results shown in Fig. 7 are run for a LWP of 0.5 kg/m2. Is that a typical LWP for clouds over Greenland? Please update your experiments to more typical values

As you have requested, we have changed Figure 7 for LWP = 0.5 kg m2 to 0.1 kg m2, which occurs more often in Greenland and is consistent with Figure 6. However, the conclusions do not change. The following has also been changed in the paper: Page 13: In this figure, a LWP = 0.1 kg m2. . . Page 14, caption Figure 7: . . .keeping LWP = 0.1 kg m2 constant.

Comment 1f: In the supplementary scripts it can be seen that the rte_solver disort is used in the libRadtran/DISORT experiments. Here, the rte_solver sdisort (Dahlback & Stamnes 1991) should be used instead. The regular disort/disort2 solver is designed for a plane-parallel geometry, where the atmosphere curves. sdisort is a pseudo-spherical disort solver, which accounts for the atmospheric curvature. In particular for high SZAs using disort will cause errors. This is also likely to explain the discrepancies seen for high SZAs in Fig. 11.

You are right and we have rerun DISORT, but now with SDISORT (and 32 streams). We have updated all figures accordingly. However, the results are still similar as before and the conclusions remain unchanged. Figure 11 is the only figure that changed somewhat. See Review #2, Comment 3 for the changes in the accompanying text. Other changes include: Page 5: . . . at a given angle. The pseudo-spherical variant SDISORT (Dahlback and Stamnes, 1991) also accounts for atmospheric curvature, which is particularly relevant for high SZA. SDISORT is used in this paper and is called DISORT from now on unless stated otherwise. Page 6: SDISORT does not provide reliable fluxes for clouds with LWP or IWP > 1.0 kg m -2, hence the regular DISORT solver is used instead for these cases.

Comment 2: Page 21, lines 14-15: "However, this version of the IFS code embedded in RACMO2 does not explicitly model any cloud content properties nor includes a parameterization of the optical depth." That is incorrect! Since you have not included the RACMO2 source code in your supplementary material, I cannot tell what "the IFS code embedded in RACMO2" entails, but I am very familiar with the IFS radiation scheme and SRTM as used in cy33r1. In this the cloud optical thickness is parameterized. In fact it is computed for each spectral band in each 3D model grid box.

You are right that the cloud optical thickness is calculated in the IFS radiation scheme for each spectral band. We were not aware of this. Therefore, we decided to investigate the possibility to use the cloud optical thickness instead of LWP and IWP. However, after an extensive analysis, we concluded that the uncertainty that arises when using the cloud optical thickness is likely higher then if our LWP/IWP approach is applied. Our findings are described in a new section 3.5, and we have added the results in Figure 8.

In short, we tried the following methods to use the cloud optical thickness instead of LWP and IWP. Firstly, we tried to manually set the cloud optical thickness τ in DISORT to a realistic interval (King et al., 2004). Then we defined τ the same for every wavelength such that the cloud has a certain τ. With this method, spectral effects are neglected and therefore it is omitted. Secondly, we let DISORT calculate the optical thickness for the prescribed clouds used in the lookup tables. This would, in theory, allow us to reduce the lookup tables from LWP and IWP to τ. However, it turns out that the type of cloud, i.e. liquid or ice, does have an effect on the spectral distribution computed by DISORT. In addition, the altitude of the cloud and the cloud effective radius have an impact on the spectral curve of DISORT and consequently on the calculated RWs (Note that the impact of cloud effective radius is also assessed in section 3.5). The extra figure (figure-3.png) illustrates how the representative wavelength RW alters considerably for band 8 for ice clouds, liquid clouds and everything in between.

Therefore, one would still require to make a distinction between ice and water clouds and has to define other cloud properties like the altitude. The difference between ice and liquid clouds is mostly caused by the various possible grain shapes and orientations of ice grains (King et al. 2004; Wyser and Yang, 1998). Also, a distinction has been made between ice and liquid water clouds in the IFS code, suggesting that is necessary to treat them differently.

However, it might still be possible to use cloud optical thickness in some form. Therefore, we have tried the following method. First, we derived RWs as function of the cloud optical thickness (τ) as estimated by DISORT, using pure ice clouds and the IWPs as listed in Figure 9. Pure ice clouds were used as this type of clouds is most common in polar regions. Next, we derived τ with DISORT for all other cloud combinations in Figure 9 and linearly estimated the RWs for all these combinations using the RW-τ relations for pure ice clouds. Finally, we compared the subsequently derived narrowband albedos with the true narrowband albedos. Figure 8 shows that the weighted RMSE and bias of this τ-approach is too high to be a viable option. Besides that, the quality of the liquid cloud optical thickness parameterization by Slingo (1989) in the IFS part of the ECMWF model version used in RACMO2 is limited and outdated (Hogan and Bozzo, 2018; Nielsen et al., 2014), and is updated in later iterations of the ECMWF model, but not available yet for RACMO2. Furthermore, the ice cloud parameterization by Fu 1996 (which is used in RACMO2) is not so reliable for thicker clouds above surfaces with a high albedo (Nielsen et al. 2014). To conclude, after a thorough investigation, using the cloud optical thickness as an alternative to LWP and IWP performs not as well as we have hoped for, so we decided to keep working with the method already described in the manuscript.

These considerations were incorporated in the manuscript in the following way: Page 15: 3.5 Cloud properties

LWP and IWP are chosen to represent the effect of clouds on the RW. In addition, microphysical properties of clouds such as the cloud effective radius r_e are known to impact the incoming radiation (Nielsen et al., 2014). We have chosen a realistic value of r_e, but in practice r_e will vary for each instance. Although the potential effect of r_e on the RWs is larger than BC and HULIS, it is still low (weighted RMSE < 0.01) for both clouds with small and large r_e, i.e. r_e,ice, r_e,liquid = 15, 5 and 30, 30 μm respectively (Figure 8). These values for r_e are on the lower and upper end of the probability range one could expect for the Arctic (King et al., 2004). Consequently, the typical weighted RMSE and bias is lower than indicated in Figure 8 and there is no need to make RWs dependend on r_e.

An alternative to the approach described in section 3.3 is the use of the cloud optical thickness τ instead of LWP and IWP to calculate RWs. This would be a valid approach if the spectral distribution is not altered considerably differently for ice clouds than for water clouds, as otherwise it would result in different RWs. Some differences between ice and liquid clouds are observed and are mostly caused by the various possible grain shapes and orientations of ice grains (King et al., 2004; Wyser and Yang, 1998). Still, a method using τ could be used if the uncertainty is small enough, but a choice regarding what type of clouds to calculate τ for, i.e. ice clouds, liquid water clouds or a combination, and its cloud properties has to be made nevertheless and will inevitably lead to uncertainties. We tested this "τ -approach", hence derived RWs as a function of τ for pure ice clouds, and linearly interpolated RWs for a given τ for liquid water clouds or a combination of liquid water and ice clouds. The approach performs reasonably well (Figure 8), but the spread is large. If the statistical analysis of the “τ -approach” is limited to common LWPs and IWPs in the Arctic (< 1.0 kg m-2, see Figure 9), the RMSE is rather high (blue box and dark orange median in Figure 8), especially compared to the other parameters considered. Therefore, we decided not to use the cloud optical thickness as leading parameter to compute RWs. Page 14: Other factors controlling the narrowband albedo… Page 16: The blue box shows the 25th to 75th percentiles for cloudy conditions if limited to LWP and IWP < 1.0 kg m-2, with the dark orange line indicating the median. Low and high concentrations of BC and HULIS are considered, but the blue box is omitted for clarity. Page 16: The impact of cloud effective radius r_e is evaluated for small and large values, i.e. r_e,ice, r_e,liquid = 15, 5 and 30, 30 μm respectively. Finally, the τ-approach is shown, as is described in section 3.5. Only cloudy conditions are considered for the cloud effective radius and τ-approach. Page 22: Other properties can possibly affect the spectral albedo of snow, e.g. cloud top height, but their effect is deemed negligible compared to the other known uncertainties. Page 22: Using the cloud optical thickness instead of LWP and IWP to calculate RWs does not have the desired result, as a distinction between ice and liquid water clouds still have to be made. Moreover, the quality of the liquid cloud optical thickness parameterization by Slingo (1989) in the IFS part of the ECMWF model version used in RACMO2 is limited and outdated (Hogan and Bozzo, 2018; Nielsen et al., 2014), and is updated in later iterations of the ECMWF model, but not available yet for RACMO2. In addition, the ice cloud parameterization by Fu (1996), which is used in RACMO2, is not so reliable for thicker clouds above surfaces with a high albedo (Nielsen et al., 2014). Therefore, the use of cloud optical thickness in RACMO2 to determine RWs would result in an additional uncertainty on top of the described uncertainties in section 3.5. Hence, the option to use cloud optical thickness instead of LWP and IWP has been dismissed

Comment 3: Page 9, line 10: "The broadband albedo, which is for direct radiation close to 0.78 for most SZAs..." The broadband albedo of snow can be quite different

from 0.78 depending on atmospheric and snow conditions. Please correct this line to reflect this!

You are right that it is not clear what we meant. We have changed it accordingly: Page 9 The broadband albedo, which is for direct radiation and for the atmospheric and snow conditions described in the method section close to 0.78 for most SZAs (except for high SZA), and is used to compute a RMSE for each band. âĂČ

Minor comments: - Abstract, line 5: "... Integrated Forecast System atmospheric..." –> "... Integrated Forecast System (IFS) atmospheric..." Also, add the version number 33r1!

Done

- Abstract, line 10: "... 14 spectral bands of the ECMWF shortwave..." –> "... 14 spectral bands of the IFS shortwave..."

Done

- Page 1, lines 22-23: "... the melt-albedo feedback, e.g. Dumont et al. (2014)." –> "... the melt-albedo feedback (e.g. Dumont et al., 2014)."

...the melt-albedo feedback (e.g. Van As et al., 2013)."

- Page 2, line 4: "... solar angle" –> "... solar zenith angle"

Done

- Page 2: You need to add spectral band definitions of what you mean, when you refer to "near-UV" and "near-IR". …is highest for near-ultraviolet (near-UV, 300-400 nm), visible and near-infrared (near-IR, 750-1400 nm) radiation…

- Page 2, lines 13-14: "... the ratio of upwards to downwards shortwave radiative flux integrated over the solar spectrum." –> "... the ratio of upwards to downwards shortwave radiative flux on a horizontal surface integrated over the solar spectrum."

Here you should also add explanations of the direct ("black sky") albedo, which varies as a function of the SZA, and the diffuse ("white sky") albedo. Both of these are used as input variables to SRTM in the IFS radiation scheme.

... the ratio of upwards to downwards shortwave radiative flux on a horizontal surface integrated over the solar spectrum.

Also, a distinction has to be made for the albedo of direct radiation, which varies as a function of the solar zenith angle (SZA), and of diffuse radiation. Although broadband albedo. . .

- Page 2, lines 19-20: "For example, a buried dark impurity layer will only significantly affect near-UV albedo." This I disagree with. Water has minimal absorption at the UV/violet spectral boundary, and the absorption is also very low in the UV, blue and green parts of the spectrum. Thus, these parts are also significantly affected by underlying impurities.

Done

- Page 2, line 21: "... the thermal regime" –> "... the snow heating rates"

Done

- Page 2, line 30: The RRTM_SW (also known as SRTM in the IFS code) references are placed after "RACMO2" in this line. They should be moved back to where RRTM_SW is referred to!

Done

- Page 4, lines 1-2: "RRTM_SW computes flux profiles for clear-sky and total-sky conditions on hourly intervals." –> "RRTM_SW computes instantaneous flux profiles for clear-sky and total-sky conditions."

Done

- Page 4, lines 14-15: "... specific surface area (SSA)..." This is the same acronym that is used for single-scattering albedo, an essential radiative transfer variable, which can be confusing. You should consider using something else.

In this manuscript, single-scattering albedo is not used, while specific surface area is an often occurring term. The abbreviation SSA is well defined and often used in other work, like Gallet et al. (2009) and Libois et al. (2014), so we decided to keep this acronym.

- Page 5: lines 31-32: "The effective droplet radius for ice and water clouds ..., which is a realistic radius for clouds" –> "The effective droplet radius for ice and water clouds ..., which are realistic radii for clouds"

Done

- Figure 12: This is a very nice figure, however, it is very difficult to distinguish cloud covers of 0 and 1. Please expand this part of the figure, so that these data are not hidden by the graph axes!

Done

Please also note the supplement to this comment:
https://www.geosci-model-dev-discuss.net/gmd-2018-175/gmd-2018-175-AC1-supplement.pdf

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Interactive comment on Geosci. Model Dev. Discuss., https://doi.org/10.5194/gmd-2018-175, 2018.

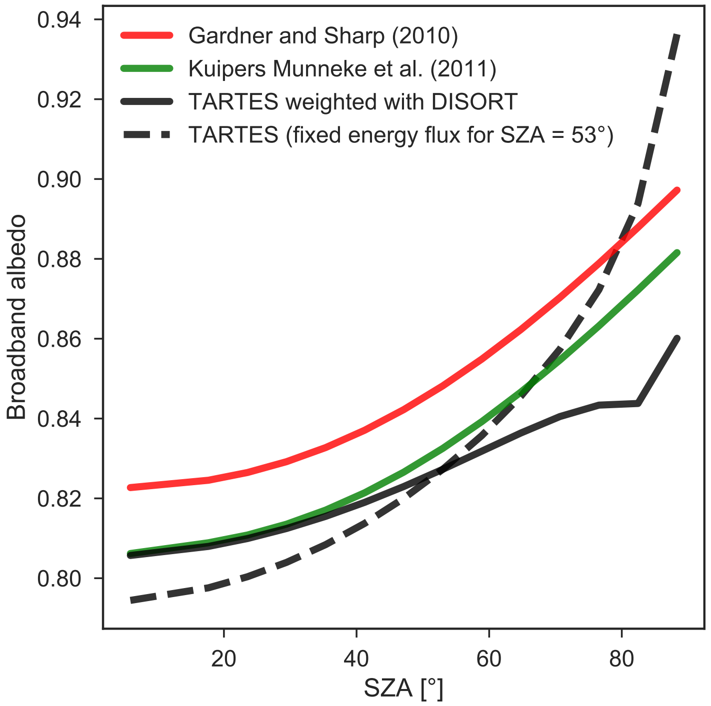

Fig. 1. Updated Figure 11 from the manuscript

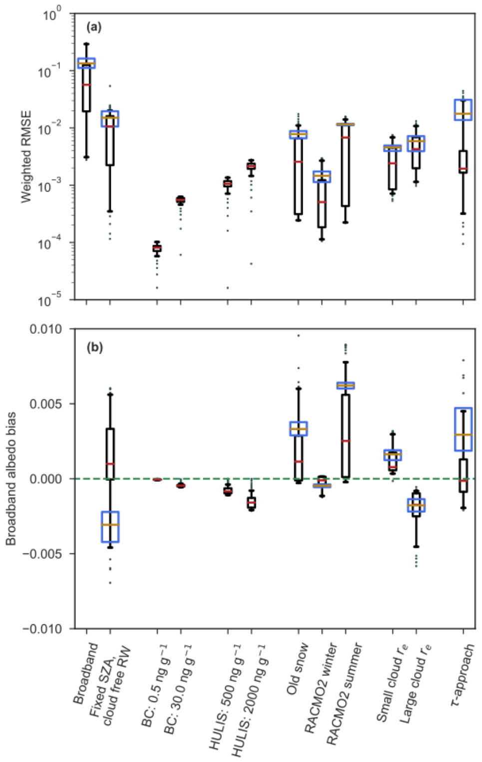

Fig. 2. Updated Figure 8 from the manuscript

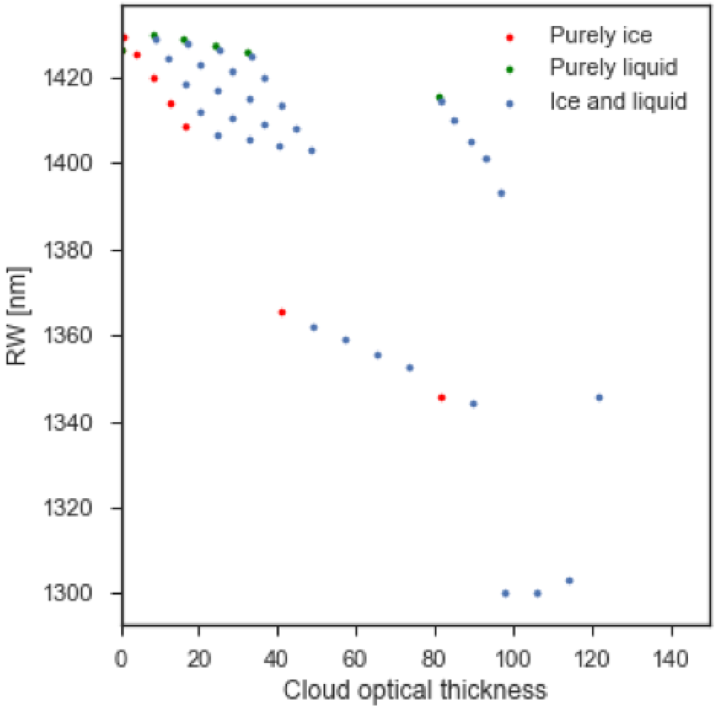

Fig. 3. Extra figure which shows the RW as a function of cloud optical thickness for pure ice, pure liquid and both ice and liquid clouds