acp-2021-158

Shift in seasonal snowpack melt-out date due to light-absorbing particles at a high-altitude site in Central Himalaya

Author’s response

Based on the very different reviews given by the two referee’s we have organized this response by first addressing Edward Bair (referee #2), followed by point-by-point replies to the questions and general comment raised by referee #1.

In the response to Edward Bair, we first provide a more general response followed by a bullet form, where we have attempted to identify specific criticisms made by the reviewer.

On behalf of my co-authors,

Jonas Svensson

Response to review by Edward Bair

General response

The reviewer suggests rejection based on flaws in the methodology, which cannot be overcome with revision. Else, the reviewer had suggested updated and more sophisticated modeling to account for light-absorbing particles (LAP) variability, while addressing all the pitfalls with in-situ measurements of snow albedo.

These statements by the reviewer summarizes the difference in strategy suggested by the reviewer and the one chosen in the manuscript. The objective of the study was to partly work with an undetermined set of data (e.g. no LAP or microphysical time series) and make assumptions about key processes and variables in order to phenomenologically reproduce observed variability in snow albedo and to quantify the impact by LAP on snowmelt. We are confident that we have achieved precisely this, and that each simplification required is stated in the manuscript and can be traced. Different strategies do not necessarily make either methods flawed.

Instead of a detailed simulations of the snow seasons, we have broken down and simplified a very complicated process into a function (parameterization) of only two variables. These are the average *deposition rate* of LAP and the total amount of snow precipitation. We are aware of detailed simulations of the impact on snow albedo by LAP (referenced in manuscript), but we are not aware of a study that synthesize the complicated processes into only two key variables. Hence, we therefore cannot agree with the reviewer that our method should be outdated.

At the same time the reviewer finds the conclusions not supported by previous research and that the conclusions are unreliable. We agree that our final relations are parametric and simplify the real world as much as possible, but this does not necessarily make them unreliable. It is further specifically mentioned that the estimated 13 days decrease in melt-out days (MOD) for the scenario of EC_{eq}=100 ng g^{-1} and PP=400 mm is unconvincing. It is not clear from the review if this number is perceived as an over- or underestimate. For completeness, equation 11 provides a range to the estimate of 13 days, which is about +/- 4 days.

If we compare our estimate of 13 days to Zhang et al. (2018) (who uses a different methodology; and is referenced in our manuscript) they found a decrease in the range of **1.3-1.8** days for 40 mm snow water equivalent (SWE) and **3.1-4.4** days for 100 mm SWE (on the Tibetan Plateau), our estimate may appear high. Compared to Skiles et al. (2012) (referenced in manuscript) who found a decrease in the range **21 to 51** days (for Colorado snow), our estimate may appear low. The estimated number of days based on equation 10 depends strongly on the amount of precipitation. More snow yields a longer season, which gives the LAP more time to influence the MOD. Below we try a new comparison, but use more appropriate precipitation values. For simplicity we assume the same deposition rate of LAP for all sites. Recall that one of the key findings of our study is that MOD is relatively insensitive to this parameter over a large range of values.

Using SWE values reported in Zhang et al. (2018), our equation 10 and 11 yields the corresponding ranges **0.9-1.7** days for 40 mm SWE, and **2.3-4.3** days for 100 mm SWE (compare with Zhang et al. estimates). At the start of the melt seasons, the cases in Skiles et al. (2012) (their Figure 7) range from about 500 mm to 1000 mm SWE. If we assume 15% loss due to sublimation, equation 10 gives a range **19 to 38** days (compare with Skiles et al. estimates). In the manuscript we never claim that equation 10 is general and can be applied universally. Nevertheless, it captures the order of magnitude estimates conducted in other studies rather well. Hence, we must disagree with the reviewer that the estimated 13 days for the base case in our study is an unconvincing conclusion. On the contrary, it is anchored well with other studies, but the methodology to arrive at these values is completely new and novel. The main scientific advancement is through its simplicity, which helps gaining insights into the key mechanisms controlling the MOD due to LAP deposition.

The main event is not that temperature and snow depth can be used together with assumptions about LAP concentrations in order to mimic snow albedo. The main event is that we indeed can mimic snow albedo, investigating which adjustments are necessary. Indeed, a complex model driven by observed times series of snow microphysics and LAP concentration, with full meteorology and turbulence, could arrive at a better agreement between observed and derived snow albedo. However, this is not the objective. In contrast we try to simplify as much as possible. In the end we have gained an understanding that the key parameters to capture the observed variability in snow albedo is an idea about EC and dust concentration (representing LAP) in new snow, the temperature (representing pristine snow albedo) and changes in snow depth (representing precipitation and ablation). By being able to mimic snow albedo also gives us the ability to calculate radiative fluxes over the season, without detailed temporal observations of snow microphysics or LAP surface concentrations. Based on this success we can answer the scientific question**: what is the contribution to the number of days with snowmelt from a <u>given change</u> of LAP in the seasonal snow for the study region?** We have discovered that LAP deposition is important for reducing the number of melt days, but at the same time the amount of precipitation is key since this will modify the length of the season and the time that the LAP will absorb solar energy.

Even if the reviewer finds our method flawed and our conclusions unreliable he must agree that we, with a minimum of information and a maximum of in-sights, are able to mimic the snow surface albedo for two snow seasons extending over several months in time. From there on the energy calculations and MOD would essentially be conducted in a similar manner even if the albedo was calculated using a much more complex framework. The end results would be similar and appear as Figure 6. That is, at the end of the season about 1/3 of the total SW energy absorbed by the snow is due to LAP. **This is the central number, and this is what the reviewer should challenge**. The breakthrough with our methodology is that in comparison to a complexed framework, we only must consider variation in key parameter and not every possible factor that may influence the results.

Simple parametrizations clearly can work in some conditions, while in other environments they may fail. For example, in Alaska a snow albedo parametrization works well during snow melt and low-precipitation environment, but badly for snow fall at a high frequency and greater amounts (e.g. Mölders et al. 2008).

More specific responses

- Clearly snow albedo is strongly dependent on the microphysical properties of snow, but we can unfortunately not claim to be the first in using the observed temperature as parametric variable for snow albedo. For instance air temperature has been used for albedo, as stated (with specific references) in Meinander et al. (2013), "in several models, such as CAM 3.0 (Collins et al., 2004), ECHAM5 (Roeckner et al., 2003; Roesch and Roeckner, 2006), and in the ECWMF model (ECWMF, 2010), snow albedo decreases with temperature (either linearly or exponentially). The basic parameterizations have the potential to be improved. According to Pedersen and Winther (2005), snow depth-dependent parameterizations perform better during the snowmelt period than temperature-dependent parameterizations." As seen above, temperature relations can be made more or less involved, but we chose to derive a relation for our dataset where the brightest albedo at each temperature interval is fitted to a simple curve. Strictly speaking it is the derived specific surface area that is fitted in order to relate to the microphysical structure of the snow. In relation to this, see also Table S1.

- We absolutely agree that snowmelt is driven by the net energy flux, where the radiation fluxes are by far the largest. This is what we show in Figure 7. What we also show is that our flux observations, and flux estimates, are not good enough to use directly. We therefore need a different variable that indicates when the snowpack has surplus energy that can be used to melt the snow. In Figure 7 we show that Ts can serve as this threshold variable. Again, this is nothing unique and in other studies the air temperature above zero (more or less involved formulations) is often used (see references in manuscript).

- We are not explicitly simulating LAP and snow interactions, but it is certainly included in our analysis. The pristine snow albedo is calculated from the temperature relation discussed above. The reduction in this albedo is described by equation 4. This equation requires information about r_e and the surface concentration of LAP. The effective radius comes from Eq. 3, 2, and 1. The LAP surface concentration is estimated from equation 6, which uses the snow depth time series. As snowmelt (SD decreases) the LAP concentration increases. The net change (excluding effect from the ground) in albedo due to LAP is then given by equation 7. A test on how well this parameterization works, is provided in the comparison shown in figures 4 and 5 (see also scatterplot S3). The non-linear indirect effect from LAP on snow microphysics is not included in our formulation. However, the combined optical effect from LAP and grain size is diagnosed through the temperature relation.

- At the latitude of about 30 degrees, representing our study area, the range in solar zenith angle is from about 40 degrees to about 20 degrees between December and June for the second season, which is the longest. Based on the calculations by Gardner and Sharp (2010) (their figure 9), “old dirty snow” would decrease the albedo with a few percent if the same snow were assumed for the entire period. Nevertheless, this potential systematic change in snow albedo is much smaller than the variability caused by the LAP, which is the main concern in this study. Cloudiness would actually be more important for our site, which is indirectly accounted for in the paper.

- The surface under the footprint of the AWS is assumed to be flat and homogeneous, which is a fair assumption given the surroundings. As the location of the measurement site is part of the Himalayan mountain range, this is of course not true for the regional topography.

- As can be seen from Figure 4 occasional new-snow events reach albedos above 0.9, which is what it is, given all the uncertainties. It is unclear what the reviewer intends with his comment about “physically impossible”. It is only five values (1x95, 1x91 and 3x90) of all the data that belong to this category. What is unacceptable to the reviewer in order to warrant rejection? Also, snow albedo that is >0.9 do exist in the literature (e.g. Warren and Wiscombe 1980 where it shows that the visible albedo for clean snow is >0.9, consistent with the extremely small absorption coefficient of ice in this spectral region). Reported albedo measurements above 0.9 exists also in the literature (e.g. Järvinen & Leppäranta 2013).

- The occasions where the albedo is less than 0.4, belongs to the category when the ground albedo influence the signal. This effect is considered in Equation 8 and depicted in Figure 2. Since the ground albedo is less than 0.2, it follows by necessity that the albedo must at some point pass through 0.4 when melting occurs.

- Unfortunately, the reviewer missed a very central point of our study, which were among those potential suggestions by the reviewer. As mentioned already above, we are not assuming a constant LAP concentration at the surface. The concentration is enriched every time the snow is defined to melt (see equation 6). Hence, the concentration towards the end of the seasons is given by the deposition during the whole season. In other words, the concentration is approaching PP (mm)x100 ng g^{-1} / d. Recall that d is set to 4 mm. If we take our base case of 400 mm, the concentration at the very end of the season is 400*100/4=10000 ng g^{-1}. That is very dark snow, but only for a short time period.

- The assumption of a constant concentration in new snow at a concentration of 100 ng g^{-1} is probably an excellent choice. It is based on the findings in Svensson et al. (2021), but it is not the enriched melt layer that interests us. It is the young snow that is closest to the new snow conditions, which is what we need. The EC deposition is given by 50 ng g^{-1} in young snow and it is the 50/50 mixture assumption that brings the concentration up to 100 ng g^{-1}. A 20/80 mixture would bring the base case up to 250 ng g^{-1}. From Figure 5, we note that the cumulative net SW radiation absorbed by the snow is possible for the 300 ng g^{-1} case, but is better represented by the 100 ng g^{-1} case. In the end, this will not influence the conclusion on the sensitivity of MOD due to LAP and answering the scientific question: **what is the contribution to the number of days with snowmelt from a given change of LAP in the seasonal snow for the study region?**

- The added absorption from the increasing LAP concentration and increasing insolation is considered, which is perhaps best illustrated in Figure 5, but also in Figure 7. It can be good to recall that the study concerns two cases of seasonal snow cover, with MODs on 22 May and 2 June, respectively. On this topic the reviewer may revisit the results presented in Figure 6, which shows that the fraction of SW radiation absorbed by LAP (due to albedo reduction) at the end of the season. The two seasons are very similar for a given LAP concentration in new snow. At the end of the seasons, case two will present much higher surface concentrations due to more deposition, but the fraction absorbed SW will not change very much. The other interesting feature is the square root dependence by the relations, which in a linear presentation as in Figure 6 highlights the non-linear relation by the absorbed SW fraction and LAP concentration in new snow. Note that this is at the end of the season, and that it is the net result from all processes.

- The comment on ablation is one which is valid except the part about being erroneously. We do not have information about precipitation amount or melting, but we have information about the snow depth. More specifically we make use of the change in snow depth. We assume that every time snow depth is increasing this represent precipitation. We know that the same amount must disappear at the end of the season. This can be melt or sublimation. In both cases this will lead to an increase in the surface concentration of LAP. The other possibility as that snow simply increase the density, without melt or sublimation, as pointed out by the reviewer. However, this will not lead to an over- or underestimation of the ablation, only that it is registered prematurely. This was of concern in the initial phase of the study until the data processing commenced. The purpose of Figure 4 and to some extent Figure 5 (checking that observed and estimated endpoints are consistent), was to try to spot potential problems caused by this process. The result would be an underestimation when the snow density increases and a hold towards observed values when that snow actually did ablate. Based on the daily values and the overall noise level, we could not detect a clear problem due to this process. Again, the problem is that the ablation is registered too early, not that we miss it or overestimate it. For a normal snowpack densification, however, we understand that it can be a rather slow process (which can be on the scale of 0.01 a^{-1}, e.g. Granberg et al. 2009).

- The data availability will be arranged with the use of a Finnish Meteorological Institute database, pending acceptance of the manuscript.

References

Granberg et al. 2009, doi.org/10.3189/002214309790794823.

Järvinen & Leppäranta, doi.org/10.1016/j.polar.2013.03.002.

Meinander et al. 2013, doi.org/10.5194/acp-13-3793-2013.

Mölders et al. 2008, doi.org/10.1007/s00703-007-0271-6.

Skiles et al. 2012, doi.org/10.1029/2012WR011986.

Zhang et al 2018, doi.org/10.5194/tc-12-413-2018.

Response to questions raised by Referee # 1

Question 1:

Page 3, Line 104: Authors mentioned that "data was screened for inconsistencies." However, no clear methodology has been provided on how the inconsistencies were screened. What makes a data point inconsistent?

We are referring to data from the AWS that can be considered as obvious outliers. These data points could be the result of either the data logger or some sensors having a temporary malfunctioning, or being affected by external conditions, which may for example arise when snowfall covers the radiation sensor (described in the manuscript text). In the latter example, it would mean that the SW ground radiation is greater than the incoming radiation, resulting in an inaccurate albedo. These are the kind of data points that were removed during the first data screening.

Question 2:

Page 4, Line 106: Paper uses median for albedo and SD, while a daily average for other data. Why so? Does it create any impact on results if authors use the daily average for albedo and SD as well?

The reason for using the median for albedo and SD is due to the fact that the median is less affected by skewedness in the data. If a data set contains a few extreme values, the average will become biased by those values, and not be very representative for the majority of the data set. When it comes to albedo and SD the data should not display a great daily variation, and therefore median is more representative of the central tendency in the data than the average. For other AWS variables, on the other hand, daily variation is likely to exist. A primary example could be displayed in the radiation sensors when you have a cloudy day versus clear sky. In this case it would be the average that is best suited to work with in the calculations.

Question 3:

Page 4, Line110: Paper states, "Using a lower emissivity would result in higher Ts, but will not affect the interpretation of the data." Why so? Some explanation is needed.

As stated in the text, if we were to use a lower emissivity value it would result in a higher Ts. This in turn, would shift the scale in Fig. 7 to a different (increased) numerical threshold used later in the manuscript. It would also shift the sensible heat fluxes, but, since they are expected to be very small, it will not influence our conclusions.

Question 4:

Page 11, Line 309-310: "Compared to other reported values for snow these estimates are high, but are close to those reported for ice." This statement is not entirely clear and needs more explanation on why *such a thing will happen if all parameters are used for snow? Also, provide some references for values reported for snow so that a fair comparison can be made.*

Our estimates are high to generally reported values for snow (as shown in table of Zhang et al. 2006). To clarify the numeric value, we have added "(about a factor of two)" on page 11 line 310 just before the comma. This difference is since our estimates include the full effect of enhanced melt-rate from LAP. We also added the reference to Singh et al. (2000) to the Zhang et al. reference for more numerical examples of DDF in the revised manuscript.

Question 5:

Page 14, Line 383-385: Authors mentioned "an overestimation of the melting compared to pristine snow." How much overestimation and compared to which data? Provide some references for comparison and quantify the overestimation.

On line 385 in the revised manuscript we clarify what we mean by adding "At the end of the previous section the estimated melt rate for season 1 including LAP was estimated to 9 mm per day, of which 2.89 mm per day was due to the contribution from LAP. Hence, we can understand why the first DDF estimate made in section 4.2 was too high."

General comments:

Minimal references are provided in many places, especially in the Introduction. Including suitable and more recent references make it easier for comparison and improvement made from previous studies. Some references in the manuscript are quite old and do not reflect the current state of knowledge with associated research. Here are few key references, which authors should consider including in the manuscript (List is not exhaustive):

Bond, T. C., et al., (2013). Bounding the role of black carbon in the climate system: A scientific assessment. Journal of Geophysical Research: Atmospheres, 118(11), 5380–5552. https://doi.org/10.1002/jgrd.50171

Flanner, M. G., Zender, C. S., Hess, P. G., Mahowald, N. M., Painter, T. H., Ramanathan, V., & Rasch, P. J. (2009). Springtime warming and reduced snow cover from carbonaceous particles. Atmospheric Chemistry and Physics, 9(7), 2481–2497. https://doi.org/10.5194/acp-9-2481-2009

He, C., Takano, Y., & Liou, K.-N. (2017). Close packing effects on clean and dirty snow albedo and associated climatic implications. Geophysical Research Letters, 44(8), 3719–3727. https://doi.org/10.1002/2017GL072916

Lee, W.-L., Liou, K. N., He, C., Liang, H.-C., Wang, T.-C., Li, Q., Liu, Z., & Yue, Q. (2017). Impact of absorbing aerosol deposition on snow albedo reduction over the southern Tibetan plateau based on satellite observations. Theoretical and Applied Climatology, 129(3), 1373–1382. https://doi.org/10.1007/s00704-016-1860-4

Singh, D., Flanner, M. G., & Perket, J. (2015). The global land shortwave cryosphere radiative effect during the MODIS era. The Cryosphere, 9(6), 2057–2070. https://doi.org/10.5194/tc-9-2057-2015

Stephens, G. L., O'Brien, D., Webster, P. J., Pilewski, P., Kato, S., & Li, J. (2015). The albedo of Earth. Reviews of Geophysics, 53(1), 141–163. https://doi.org/10.1002/2014RG000449

Ward, J. L., Flanner, M. G., Bergin, M., Dibb, J. E., Polashenski, C. M., Soja, A. J., & Thomas, J. L. (2018). Modeled Response of Greenland Snowmelt to the Presence of Biomass Burning-Based Absorbing Aerosols in the Atmosphere and Snow. Journal of Geophysical Research: Atmospheres, 123(11), 6122–6141. https://doi.org/10.1029/2017JD027878.

Regarding references in the introduction, we do not agree that the suggested papers would significantly revitalize our present list of papers. We have however included the reference by Lee et al. (2017) as this is also related to statements made by Edward Bair (referee #2).

On line 76 in the Introduction of the revised manuscript we add "By combining satellite observations and 3D global model calculations, Lee et al., (2017) derived a linear relation for snow albedo (for pixels with 100% snow cover) expressed as function of the land surface temperature (LST), aerosol optical depth (AOD) days after snow fall (DAS). As much as 57% of the variance in albedo could be explained by these variables over the southern Tibetan Plateau".

References

Lee et al. 2017, doi.org/10.1007/s00704-016-1860-4.

Singh et al. 2000, doi.org/10.1016/S0022-1694(00)00249-3.