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Interactive comment on "Examining the atmospheric radiative and snow-darkening effects of black carbon and dust across the Rocky Mountains of the United States using WRF-Chem" *by* Stefan Rahimi et al.

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We thank the reviewer for their helpful and insightful comments. We have done our best to address each concern. Major (minor) comments are in yellow (green), and responses follow. The authors present a modeling study on the impact of black carbon (BC) and dust on the regional climate of the Rocky Mountains. Using WRF-Chem they examined the radiative impact of BC and dust in the atmosphere and the impact

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on the snow pack via the modification of snow albedo and snow melting. They performed a series of simulations with all processes or with some processes eliminated. WRF-Chem simulations were limited to the period from February to July 2009 after spin-up simulations with WRF without chemistry. The simulations give important information on the contrasting radiative impacts of BC and dust in the atmosphere and the snow. For example, they confirm the larger radiative impact of BC compared to dust despite the orders of magnitude higher concentration of dust. The results also demonstrate the different regimes in four specific regions of the Rocky Mountains. The authors continue to discuss the potential impact of BC and dust on hydrological processes and specifically on the timing of the run-off. I have major concerns concerning this part of the manuscript, which in my opinion is less developed and less convincing. Therefore, I recommend major revisions before publication of the manuscript in ACP. Reply: We thank the reviewer for the positive comments. The manuscript has undergone significant revisions. 4 Figures have been moved to the supplement. Of note, the acronym "BCD" has been changed to "light-absorbing particles" (LAPs) for better consistency with the literature. 5 appendices are now used to house more technical descriptions that weigh the paper down. Sec. 5.4 is now included along with Table 5 which highlights changes in meltout date. Major comments (MaCs) MaC1: In the simulations the CNT run only kicks in after 01/02/09. However, at this date approximately 60 % of the snow has on average already been deposited (Fig. 2c), but the BC and dust loading of this part of the snowpack is not known from the NOCHEM runs. How is this treated? Does the snowpack consist of a lower part of clean snow with layers including BC and dust on top? If yes, what is the impact on the simulations? How was this taken into account for calculated parameters (e.g. for the BC and dust in-snow burdens)? Reply: This is an important detail. Snow at and beneath the surface was not initialized to a "clean" state. Originally, CNT was found to underpredict SWE substantially compared to measurements when initialized on 1 September instead of 1 February. Due to limited computational resources, a new modeling design was applied that saw the surface energy and hydrological fields from NOCHEM applied to the 1

February restart file, and in-snow BC and dust amounts were copied from the original WRF-Chem simulation to the new restart file where snow was present. This detail has now been included in the text: “We restart our WRF-Chem simulations on 1 February 2009 00:00 UTC using surface energy and hydrological fields from the NOCHEM restart file but in-snow LAP fields from the original WRF-Chem restart file.” Specifically, LAP concentrations were used in all levels of the LSM where snow was present, and snow in the branch simulations was not initialized to a clean state. MaC2: The authors claim in ch. 5.2 that since changes in simulated precipitation are at most 0.3 mm d-1 and SWE anomalies can be larger than 10 mm, the induced changes in SWE cannot be attributed to precipitation changes. I don’t find this a convincing argument. Assuming that early in the winter season the solid precipitation increased by the given upper limit only for a period of a month and if all further processes remain unchanged, the resulting SWE for the rest of the winter season would increase by 9 mm. Therefore, the impact of precipitation changes in the simulations should be analyzed and discussed in more detail. Reply: This point is well taken and understood, and this portion of the manuscript has been clarified to reflect this comment. ARI-induced precipitation modifications are generally less than 0.1 mm/d on average, not 0.3. However, it does appear that some correlation exists between the timing of ARI-induced precipitation and runoff anomalies. ARI-induced precipitation anomalies (now Fig. S4) correlate better with the ARI-induced runoff time series (Fig. 11c) than precipitation/runoff anomalies from SDEs (Figs. 9c, d, respectively). Furthermore, it seems as though snow changes are influencing runoff on a longer time scales than is precipitation. This is true for both SDEs and ARIs. Addressing this point, the following changes have been made: 1. All statements suggesting that precipitation changes modulating runoff changes are negligible compared to SDEs have been removed from the manuscript, and the relevance of precipitation changes has been included for both ARI (Sec. 5.2.2) and SDEs (Sec. 5.1.2). In Sec. 5.2.2, the following paragraphs now read as follows: “SWE (runoff) increases (decreases) from April onward due to LAP ARI across all four subregions prior to mid-May. These ARI-induced runoff changes are occurring in the

presence of near-zero and nearly trendless precipitation (Fig. S4) and snowfall (not shown) anomalies. The simulation of these features suggests that the main driver of runoff changes, at least from April through mid-May, is depressed snowmelt from LAP ARI surface dimming. ARI-induced precipitation changes do impact runoff, however. For example, decreased precipitation from mid-May through 1 June (Fig. S4) correlates with decreased runoff during the same time period across Greater Idaho and the Northern Rockies (Fig. 11c). Following 1 June, runoff anomalies become less negative and even positive across the four subregions, a pattern opposite to that of LAP SDE (Fig. 9d). BC ARI tend to drive a majority of the runoff decreases prior to 1 June and promote increased runoff deeper into the summer. Dust ARI on the other hand has the opposite effect on runoff to that of BC ARI, increasing runoff through mid-May and decreasing runoff after 1 June across the Northern Rockies. Comparatively, although SDE- and ARI-induced precipitation anomalies are of similar magnitude across the four subregions, the relative impact of LAP ARI-induced precipitation changes on runoff anomalies is larger than that of LAP SDE because the overall SWE changes associated with LAP SDE are larger. Larger snow (and subsequent runoff) changes occur due to LAP SDE, making the relative contribution of LAP SDE-induced precipitation changes to the total runoff changes smaller. Snowmelt and precipitation-specific runoff contributions were not output and thus cannot be explored further in this study."
In Sec. 5.2.1, the following paragraphs now read as follows: "SDE-induced anomalies in SWE (Fig. 9b) and precipitation (Fig. 9c) change runoff by fractions of millimeters per day across the four subregions. Here, runoff is defined to be the sum of surface and underground runoff from the model output; runoff from glaciers and lakes is neglected. Driven primarily by BC SDEs, runoff is mostly increased through late June across all four subregions. Maximum simulated precipitation anomalies are generally less than 0.1 mm d-1 (Fig. 9c), while runoff anomalies are typically an order of magnitude larger (Fig. 10d). The largest increase in runoff occurs across the Northern Rockies (5.5 mm d-1, a 90% change from CNT), which is characterized by the largest reductions in SWE. During July, negative anomalies in runoff manifest, with the largest

reductions simulated across the Northern Rockies (5.5 mm d-1, July mean ∼1%) and the Southern Rockies (4.5 mm d-1, July mean ∼2%). Smaller runoff reductions of ∼1 mm d-1 (2%) are simulated across Greater Idaho, while runoff increases of 1 to 2 mm d-1 (< 5%) are simulated across the Utah Mountains in phase with precipitation increases across this subregion (Fig. 10c). Although Qian et al. (2009) and Wu et al. (2018) emphasized results across basins, the dipole signature of runoff increases followed by runoff decreases is consistent with our results, despite primarily examining SDEs at higher elevations in this study. SDE-induced precipitation perturbations of greater than 0.1 mm d-1 are not simulated until mid-May, but runoff increases due to SDE are simulated beginning around 1 April. In the absence of a coherent trend in SDE-induced ice (not shown) or overall precipitation (Fig. 10c), we surmise that, at least initially, SDE-induced runoff anomalies are mainly driven by the enhanced melting of SWE and not SDE-induced precipitation changes. By mid-May, runoff increases across the Northern Rockies are relatively maximized, even as near-zero or slightly negative precipitation anomalies due to LAP SDE are simulated. There are however some correlations between the runoff time series and precipitation anomalies. For example, a local minimum in the runoff anomaly time series (Fig. 9d) is simulated around 1 June which correlates with negative precipitation anomalies of 0.3 mm d-1 across the Northern Rockies. In effect, this negative precipitation anomaly is depressing the enhanced runoff signature induced by LAP SDE-induced snowmelt. During mid-June, precipitation increases in excess of 0.4 mm d-1 correlate with an increased positivity to the runoff anomaly time series (Fig. 9d) across Greater Idaho and the Northern Rockies." 2. Fig. S4 has been included showing ARI-induced perturbations to precipitation. MaC3: In general, the seasonal SWE average is in my opinion not an appropriate parameter since it includes the history of the precipitation. Solid precipitation early in the winter season has a larger impact on the average than later precipitation. The same is true for the simulation: if the SWE is modified early in the simulations the impact on the SWE average is larger than for later modifications. This leads than to confusing statements that the simulated SWE is larger than the observed SWE (e.g. ch 3.2), while

Fig. 2c clearly show lower maximum SWE values in the NOCHEM and CNT runs. The positive bias probably stems only from the delayed snow melting in the simulations. Maybe, anomalies are better analyzed using SWE values at several specific dates? This could show the negative bias in spring and the positive bias in summer in the simulated SWE. Reply: To clarify, SWE is underpredicted by CNT and NOCHEM compared to point SNOTEL observations, but SWE is overpredicted and underpredicted by CNT when compared to the spatial distribution from the UA product (Fig. 4; mostly overpredicted at higher elevations). Because CNT was not run for the full model year, it is impossible to take into account the SWE reductions due to LAP effects occurring prior to 1 February. CNT's overprediction of SWE at high elevations compared to UA occur where driving observations (e.g., SNOTEL) are scarce. Because the UA gridded product is driven by observations, the high modeled SWE bias at higher elevations may be artificial, as indicated in Broxton et al. (2016). More generally, CNT simulates less snow than NOCHEM, meaning that the model that includes aerosol effects (CNT) integrates a solution more dissimilar to SNOTEL observations than NOCHEM. This is due to the fact that the atmospheric and land surface parameterizations in NOCHEM, some of which are empirically based, already partially account for these processes implicitly simply by virtue of the inclusion of SDE and ARI in the measurements for which the parameterizations were originally developed. Our goal here was not to show that CNT was closer to observations than NOCHEM but rather to discuss the physical changes in Rocky Mountain weather and hydrology due to SDE and ARI in high-resolution simulations, which has not been previously studied in this manner across this region. The differences between CNT and NOCHEM are vast and are beyond the scope of this study. As a sanity check, we wanted to ensure that the CNT results were comparable to a more commonly used counterpart without chemistry (NOCHEM), and we wanted to ensure that both simulations compared reasonably well with observations. Additional text comparing CNT and NOCHEM is now provided in Appendix A4. We also note that internal model variability may obfuscate more coherent agreements between NOCHEM and CNT, as well as lead to seemingly strange SWE anomalies, especially in light of the reviewer's point. MaC4: Fig. 2c demonstrates further that the dynamics of the snow melting are not reproduced by the model independent if it includes BC and dust or not. Including BC and dust seems to shift the melt-out dates of the snow by a couple of days, but the simulated melt-out still appears to be delayed on average by more than 20 days compared to the observations. Moreover, observed melting rates are significantly higher than simulated melting rates. This should be discussed in more detail. This bias leads for example to large simulated impacts of BC and dust in the snow on temperature, SWE and run-off in July, for which the observations show no or rather little snow on the ground. Reply: Thank you for this comment. Indeed, both CNT and NOCHEM deviate from SNOTEL observations. Both underpredict SWE and melt out snow too late (by ~20 days). Unsurprisingly, CNT simulates less SWE than NOCHEM due to the explicit presence of BCD effects in said simulation, but NOCHEM simulates a superior SWE curve (Fig. 2c) than CNT compared to observations. While the root differences between CNT and NOCHEM are beyond the scope of this paper (now mentioned in Appendix A4), as are WRF's/CLM's slow melt out deviations from observations, the ramifications of poorly simulated snow dynamics should at least be mentioned in the manuscript in context with potential weaknesses in observations. Appendix A4 reads: "The goal of this study is to quantify the impacts of LAP SDE and ARI on WUS weather and hydrology. This aim does not align with examining root causes of differences between CNT and NOCHEM, and its scope does not necessarily focus on WRF's overall deficiencies in simulating seasonal snow dynamics. Nonetheless, we do note that significant technical differences exist between NOCHEM and CNT which lead to their different results. First, upon grid-cell saturation, NOCHEM's number concentration of activated aerosols is prescribed in the microphysics scheme to be 250 cm-3, while CNT's is calculated online accounting for the local aerosol characteristics. This difference is most certainly leading to differences in the simulated snow yields through changes in the precipitation efficiency of clouds (not examined), with CNT simulating a smaller wet precipitation bias than NOCHEM compared to SNOTEL observations. An additional notable difference between CNT and NOCHEM is the coupling of chemical species' optical properties to the radiation code in CNT; this process is entirely neglected in NOCHEM and is also most certainly contributing to differences in solutions between the two results. More generally, WRF without chemistry (NOCHEM) has traditionally been developed to emulate the observed planet as closely as possible even though the model itself is free of explicitly simulated and physically based chemical processes, both in its atmospheric component and its land surface model. This study is an example of an instance where the inclusion of chemistry into the model (CNT) does not necessarily improve model performance. In fact, it appears that the presence of chemistry in CNT actually worsens our results compared to NOCHEM, as NOCHEM simulates SWE values closer to SNOTEL (Fig .2c) than CNT. Additionally, WRF (and other models) has traditionally showcased difficulties in simulating the evolution and timing of seasonal snow dynamics (Caldwell et al., 2009; Wu et al., 2017), and our study does not attempt to explore why these deficiencies exist. Here, both simulations simulate a melt-out date ~20 days later than is observed by SNOTEL. The differences between CNT and NOCHEM, as well as their deficiencies, should be kept in mind when interpreting the results of the study, and an evaluation of their differences is beyond the scope of this study."

Additionally, we have added Sec. 5.4 which quantifies the changes in meltout date. These results are summarized in newly added Table 5. Because meltout did not occur across 3/4 of our subregions, we present a "lag" time between CNT and their perturbation experiments. It was generally found that LAP effects accelerate meltout by ~3-4 days. MaC5: In the manuscript the hydrological impact is directly linked to surface run-off related to snow melting, without taking into account any detailed hydrological processes like groundwater storage or sub-surface transfer. This should be mentioned in the manuscript and potential impacts should be discussed. Moreover, since the dynamics and the timing of the snowpack melting in the simulations do appear to be biased (see above), it appears likely that the derived run-off is also strongly biased. How reliable are the conclusions concerning shifts in the timing of the run-off? A comparison with observed run-off data like for the atmospheric and snow data would be very helpful to support the conclusions in this part of the manuscript. In my opinion, related to this bias the simulations can at most give relative changes according to run off shifts in the runs with and without BC and dust. In my opinion, the presented shifts in run-off are not realistic and can in its current form not be used to inform local stakeholders. I recommend deleting from the manuscript all results and further parts describing and discussing the derived run-off. Reply: While a complete water table analysis was not conducted in this study, the runoff results in this study reflect the changes in surface + subsurface runoff (mentioned in Sec. 5.1.2 as runoff deviations are presented). We did not output other variables such as runoff from glaciers, or groundwater storage/transfer that would have allowed us to do a complete water budget analysis.

Regarding the reviewer's second point, the bias in simulated snow dynamics may indeed be biasing our results. However, intuitively, one might expect that a warming of the snow would accelerate snowmelt as winter transitions to summer, accelerating runoff. By late-spring and early summer, runoff rates would be depressed as a consequence of smaller snow amounts than baseline. This dipole runoff signature shows up in our sensitivity experiments and is consistent with the results of Qian et al. (2009, 2010) and Wu et al. (2018) focused on the western U.S. and indeed other regions (e.g., Rahimi et al. 2019). Moreover, these sensitivity experiments were run to explore if the findings of previous studies, which were conducted on comparatively coarse resolution grids, still held up at convective-permitting scales within a fully non-hydrostatic atmospheric model coupled with chemistry. Although the necessary output required to perform a complete water budget analysis is not available, the linkages between snowpack changes, precipitation changes, and runoff changes due to LAP effects can still be discussed in context to one another; these variables are fundamental to the local water budget across the intermountain west.

Regarding the reliability of these results, the changes in temperature, snow, precipitation, and runoff are comparable to results in previous studies (mentioned above). Hence, LAP-induced anomalies in these variables can be considered to be at least

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“physically plausible,” if not “reliable”, even if there are aspects of the overall meteorology that are simulated with inaccuracies (e.g., the snow dynamics). This will always be the case however in any form of numerical modeling framework. We believe however for these results to be truly informative for policymaking efforts, these experiments need to be conducted on multi-year time scales to develop a base climatology and smooth out internal model variability.

Finally, we examine how simulated temperature, precipitation, and snow compare to observations due to an abundance of high-resolution observational data products. We then include runoff in our analyses as an extension of our results, as (too our knowledge) runoff datasets are too coarse to capture fine-scale signatures across our domain. Because all products in this study were either point-source observations or characterized by grid spacings < 5 km, no evaluation of simulated runoff compared to observed runoff was performed; we were unable to find such high-resolution runoff datasets. For now, we keep simulated runoff in our analyses without validation.

Minor comments (MiCs) MiC1: Concerning the impact of a modified snowpack on the hydrology of the western part of the US the authors refer in the introduction to Serreze et al., 1999 and Hamlet et al, 2007, which are both based on data from the last century. Adding studies on this subject based on more recent observations would be valuable for the readers. Reply: The following citations have been added to reinforce the text based on more recent studies/observations: Fyfe, J. C., Derksen, C., Mudryk, L., Flato, G. M., Santer, B. D., Swart, N. C., Molotch, N. P., Zhang, X., Wan, H., Arora, V. K., Scinocca, J. and Jiao, Y.: Large near-term projected snowpack loss over the western United States, Nat Commun, 8(1), 14996, doi:10.1038/ncomms14996, 2017.

Kapnick, S. and Hall, A.: Causes of recent changes in western North American snowpack, Clim Dyn, 38(9–10), 1885–1899, doi:10.1007/s00382-011-1089-y, 2012.

Mote, P. W., Li, S., Lettenmaier, D. P., Xiao, M. and Engel, R.: Dramatic declines in snowpack in the western US, npj Clim Atmos Sci, 1(1), 2, doi:10.1038/s41612-018-

0012-1, 2018. MiC2: It appears that the used emissions covered 2011, while the simulations covered the first half of 2009. It remains unclear for which year the boundary conditions are valid. Any specific conditions during any of the considered years? The potential impact should be briefly discussed. Reply: This is a very good point. We have made sure to indicate which emissions data are and are not simultaneous to our experimental period. Specifically, anthropogenic emissions are from 2011 inventories (non-simultaneous with our simulation period), but boundary condition and initial condition chemistry from MOZART-4, as well as fire emissions from FINN, are date-time specific to our experimental period. The following modifications to the text have been made in Sec. 2.2: “Anthropogenic emissions from the Environmental Protection Agency’s (EPA) 2011 National Emission’s Inventory (EPA NEI-11; https://www.epa.gov/air-emissions-inventories/2011-national-emissions-inventory-nei-data) are used. These emissions contain location-specific point and area source emissions and are interpolated to a 4-km grid using the open-source software emiss_v04.F (ftp://aftp.fsl.noaa.gov); anthropogenic emissions from EPA NEI-11 are not simultaneous with our experimental time period. Simultaneous biomass burning emissions. . .” Sec 2.3 has also been modified: Reply: “Chemical boundary tendencies are updated every 6 hours beginning on 1 February 2009. MOZART-4 chemical input into WRF-Chem is date and time specific, but we note that in-domain anthropogenic emissions are averaged for the year 2011” MiC3: It would be good to recall in ch. 2.1 how the introduction of BC and dust into the snowpack due to dry and wet deposition is treated in the SNICAR model and if and how BC and dust are preserved in the snow during melting. Reply: The following discussion about SNICAR has been added as an appendix (A1): “Simulated snow modification by the SNICAR model begins with LAP deposition flux (wet and dry) information calculated by the atmospheric chemistry module. As described in Flanner et al. (2012) and Zhao et al. (2014), dust (BC) mixes externally (internally and externally) with falling hydrometeors and is deposited on the snowpack. Upon deposition, LAP is uniformly and immediately mixed throughout the layer. For BC, offline calculated Mie parameters (i.e., asymmetry parameter, SSA, extinction) valid for effective radii of 0.1 mm are used

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from Chang and Charalampopoulos (1990). These values were used to derive snow absorption enhancement factors for a broad range of snow grain sizes. The mass absorption cross sections of BC are scaled by these factors which are found in a lookup table. For dust, optical properties in snowpack are derived from a combination of the Maxwell-Garnett mixing approximation and Mie theory. An assumed dust composition is used, and its size distribution is defined lognormally with a number median radius of 0.414 mm and a standard deviation of 2. Snow grains are treated by SNICAR as a collection of ice spheres with effective median number radii between 30-1500 mm. Mie parameters for snow are computed in one visible and four near-infrared bands offline. For the final radiative transfer calculations, BC, dust, and snow grains are treated as an external mixture by summing the extinction optical depths for each element, weighting the individual SSAs by the optical depths, and weighting the asymmetry parameters by the product of optical depths and the SSAs (Zhao et al., 2014). More information on the methods used in SNCAR can be found in Flanner et al. (2012). As the snowpack melts, meltwater scavenging of LAP is accounted for in SNICAR. Each layer in CLM4 has a threshold liquid capacity. Once this capacity is exceeded in a layer, the excess liquid is added to the liquid content of the layer beneath. The amount of scavenged LAP in this meltwater is proportional to this excess, the mass mixing ratio of LAP, and a scavenging factor (see Eq. 1; Zhao et al., 2014)." MiC4: In Fig. 2c it appears that the only significant difference between averaged SWE in CNT and NOCHEM occurs in the first half of March. Afterwards, the two curves seem to behave very similar with more or less constant differences. Is the impact of BC and dust in the snow on the simulated SWE only apparent in this short period? For example, the authors could show in Fig. 2c also the difference in SWE from CNT and NOCHEM to clarify this. I would actually expect that the impact is stronger during the melting phase than in March. If this is not the case, this should be discussed. Reply: It is impossible to pinpoint exactly what is driving the differences between CNT and NOCHEM here due to the fundamental differences between CNT and NOCHEM (addressed in MaC4). As mentioned, it was hoped that the CNT results would be somewhat comparable to NOCHEM in terms of simulated temperature, precipitation, and snow properties, hence the motivation for running NOCHEM. NOCHEM was meant to serve not only as a starting point for the control (CNT) and perturbation WRF-Chem hydrological fields but also as another dataset to evaluate the performance of CNT. For examining the effects of LAP SDE and ARI on WUS weather and hydrology, we only use CNT and variations in CNT (perturbation experiments; noSDE, noARI, etc.); NOCHEM is not used in any analyses after Sec. 3. Finally, it is indeed the case that LAP effects induce the largest changes in weather and runoff as the spring progresses. As indicated by time series in Figs. 9, 11, and 12 the largest perturbations to SWE (and other variables) are simulated from April through June (the melting season). MiC5: The data shown in Fig. 2c cover a huge area. It would be useful to show the same curves also for the four selected regions, which exhibit in the simulations different snow dynamics as discussed later on the manuscript. Are there similar differences in observed and simulated SWE in the four specific regions? Reply: Great idea. We have added Fig. S1 to the supplement. As can be seen, CNT simulates slightly less SWE than NOCHEM across all subregions. Furthermore, all CNT and NOCHEM melt out snow too late compared to SNOTEL, except across Greater Idaho. Greater Idaho sees the largest low bias in simulated SWE compared to SNOTEL and melts out snow ~10 days later than is observed, while the Utah Mountains see simulated melt out occurring almost 30 days later than SNOTEL observations. Simulations do a fair job of reproducing the observed timing of maximized SWE in mid-April compared to SNOTEL regardless of subregion. Characterization of the snow melt out discrepancy is now presented in Sec. 3.1. MiC6: The description of the impact of BC on snow metamorphism in lines 383ff appears rather superficial. The presence of absorbers in the snow has multiple impacts on the properties of the snow, which finally contribute to the radiative forcing. More detailed descriptions of the processes can for example be found in Painter et al., 2007 and Flanner et al., 2007. Reply: The snow-aerosol-albedo feedback enhancement by snow impurities was only skimmed in the intro. The paragraph in question has been modified to be more specific about what is happening regarding snow impurities and the enhancement of the snow-albedo feedback: “The additional energy in the snowpack (Figs 7a, 7b, and S3) for a given time increases melting rates, leading to ice crystal growth of the underlying snow at the expense of liquid; larger ice crystals have a lower albedo than smaller ice crystals (Hadley and Kirchstetter, 2012). Increased heat content at the surface can warm the interfacing air via conduction, and this warming in turn melts more top snow, completing this feedback. Fig. 8j shows that mean snow grain radii are mostly enhanced by several microns across snow-covered regions from March through June. This enhancement in the snow-albedo feedback is explored in detail in Flanner et al. (2007) and Painter et al. (2007).”

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Please also note the supplement to this comment:
https://www.atmos-chem-phys-discuss.net/acp-2019-998/acp-2019-998-AC3-

supplement.pdf

Interactive comment on Atmos. Chem. Phys. Discuss., https://doi.org/10.5194/acp-2019-998, 2020.