We thank the two anonymous reviewers for their valuable comments and constructive suggestions on the manuscript. Below, we explain how the comments and suggestions are addressed and make note of the revision in the revised manuscript.

Reviewer #2

This is a very end-to-end modeling analysis of the effects on surface air temperature and snowpack (SWE, fraction and runoff) in several regions of the western U.S. It includes a comparison of modeled near-surface atmospheric BC concentrations and mixing ratios of BC and dust in snow against observational datasets.

I have no fundamental problems with the analysis. The paper should be accepted after addressing the issues noted below. It could use some editing for English but overall is well-written, if a bit long, in part due to being repetitive in some places. I have enclosed an annotated version of the paper showing the small edits I think are needed for better English/readability.

Reply: We thank the reviewer for detailed review and helpful comments. The text, tables, and figures are revised as the reviewer suggested. We have also included the edits made by the reviewer.

The following issues need addressing:

Pg 8, lines 153-154: "…CLM4 explicitly represents the snowpack (snow accumulation and melt)…" Does it also represent sublimation?

Reply: Yes, CLM4 also represents sublimation. We clarify this in the revised manuscript: "…CLM4 explicitly represents the snowpack (accumulation due to snowfall and frost, loss due to sublimation, and melt)…".

Pg. 8, lines 160-162: I think it should be explicitly pointed out that SNICAR includes the effects of feedbacks to the snowpack (grain size, melt) that are driven by albedo reduction with LAA deposition.

Reply: We thank the reviewer for the comment. We add the statement in the revised manuscript: "It should be mentioned that SNICAR includes the effects of feedbacks to the snowpack (grain size, melt) that are driven by snow albedo reduction due to LAA deposition."

Pg. 9, lines 172-173 and Figure 1b: "As shown in Figure 1b, the high-resolution grids resolve well the variations of terrain in the Rocky Mountains." First: Is Figure 1.b at 0.125deg resolution? That's not clear. The figure caption just says 1b shows the terrain height within the region that is modeled at 0.125deg res – but not that the terrain height data shown in the figure is itself at 0.125deg resolution. Second: The figure just shows terrain height – there's nothing to indicate whether the terrain height at 0.125deg res "resolves well" the terrain (e.g. an actual comparison of terrain height at 0.125deg res vs at some much high res) so I'm not sure what the basis is for this assertion.

Reply: We thank the reviewer for pointing out this. Figure 1b shows the terrain height used in VR-CESM, and the resolution is same as the variable resolution grid (i.e., the resolution is 0.125 degree only in the region surrounded by dashed lines and increases gradually to 1 degree outside of the region). Comparisons with United States Geological Survey (USGS) 3km data is shown in Wu et al. (2017), and the results reveal that the topography data used in VR-CESM resolves well the variations of terrain in the Rocky Mountains. We clarify this in the revised manuscript: "Figure 1b shows the spatial variations of terrain height for the variable resolution grid used in VR-CESM. Compared to United States Geological Survey (USGS) 3km topography data (Lauritzen et al., 2015), the topography data used in VR-CESM resolve well the variations of terrain in the Rocky Mountains (see Figure 2 of Wu et al. (2017))." (Section 2) and "(b) Terrain height (m) in the western U.S. for the variable resolution grid used in VR-CESM. The refined region at a resolution of 0.125° is surrounded by dashed lines." (Figure 1 captions).

Pg. 10 lines 200-201: It is not clear here that the a-posteriori tuning factor is determined as part of this study, or if it was done as part of a previous study and you are just applying an additional adjustment factor here, based on the high-resolution model fields.

Reply: We thank the reviewer for the comment. Before we conducted VR-CESM simulation presented in this study, we had run a test simulation using VR-CESM, which shows that surface dust concentrations are overestimated in North America. Therefore, we reduced the tuning factor (T) accordingly and conduct VR-CESM simulation for this study. We clarify in the revised manuscript: "Due to the large uncertainty in modeled dust emission, the dust emission scheme also adopts a tuning factor (*T*) to simulate the reasonable dust emission amount. Our test simulation shows that with the increase of model resolution, VR-CESM produces much higher dust concentrations compared to the observations (section 3) in North America if *T* used in the standard CESM with quasi-uniform 1° resolution is used. Therefore, for VR-CESM simulation in this study, *T* is reduced by a factor of 2.6 to produce the similar magnitudes of near-surface dust concentrations as the observations, as will be shown in section 4.1".

Pg. 13, line 257: Are the 80 and 94 stations "totals" all stations in existence or the total number of stations from which data are used in this analysis?

Reply: 80 and 94 are for the stations from which data are used in this analysis. We clarify this in the revised manuscript: "In total 80 and 94 stations are selected for BC and dust observations, respectively, in the western U.S. (Figure 2).".

Pg. 13, lines 270-271: Two important things you need to clarify here: First, that you used the snow mixing ratios from the full snow column (not, e.g., just the surface snow mixing ratios) Second, you need to clarify how the column mixing ratio was calculated. Did you average the mass mixing ratios, or calculate the masses of BC throughout the snow column and of snow (SWE) through the whole column, then calculate the mixing ratio from that?

Reply: We thank the reviewer for pointing out this. We clarify this in the revised manuscript: "For comparison with model simulations, we derive observed BC mass mixing ratios (C_{BC}) in the whole snow column at sites #1-12 and #16-17 by dividing total BC mass throughout the snow column by total snow mass throughout the snow column. At sites #13-15, the averages of C_{BC} for all the aged snow samples (from various depths and columns) were reported by Doherty et al. (2016) and are used here.".

Pg. 15, lines 299-300: Important: The dust in snow may have a much larger size distribution than the typical tropospheric dust size distribution. Dust >10microns can be lofted from the surface but will not travel very far because they will rapidly dry-deposit to the surface (i.e. to the snow!), so they don't contribute much to the atmospheric dust but can contribute significant mass to deposited dust. For dust deposited to snow, this will of course be the case more so the closer the snow is to the dust source.

Reply: We thank the reviewer for the comment. We agree with the reviewer, and will highlight the importance of large dust particles (>10 μm) in snow. We more extensively examine previous observational studies, and find that in addition to atmospheric dust particles, Reynolds et al. (2016) also measured the dust mass in snow, which show that the in-snow dust mass is mainly from dust particles with diameter > 10 μm (consistent with the size distribution of atmospheric dust particles). This can directly support the existence of large dust particles (>10 μm) in snow. We add the observational evidence of dust particle size in snow from Reynolds et al. (2016): "According to the observations of Reynolds et al. (2016), the mass concentration of total suspended particles (TSP) both in the atmosphere and in snow is mainly from particles with diameters larger than 10 μm in the Utah-Colorado region.". We also point out the importance of large dust particles (>10 μm) in snow, but they are omitted in the model: "Compared to previous studies based on field observations, our estimation of dust-induced SRE is generally one-order of magnitude smaller in the Southern Rockies, which is ascribed to the omission of larger dust particles (with the diameter >10μm) in the model. This calls for the inclusion of larger dust particles into the model to reduce this discrepancy." (Abstract).

Pg. 16, lines 333-334: Important: "Overall, the model captures the magnitudes of observed near-surface BC and dust concentrations: : :" Here and in several other places assertions such as this are made, which give the impression that agreement is much better than in fact it is. In fact the correlation is not very good (R-squared of 0.3), and being within a factor of 5 is not necessarily representing mixing ratios well…Instead, please just state quantitatively what you found, e.g., that "the modeled concentrations are generally within a factor of 5 of the observed concentrations, and the two are moderately correlated (R-squared 0.3). Averaged

across all comparison points, the model concentrations are a factor of 1.8 lower than the observed concentrations."

Reply: We thank the reviewer for pointing out this. We have revised the analysis and state quantitatively the comparison results: "The modeled concentrations are generally within a factor of 5 of the observed concentrations, and the two are moderately correlated (the correlation coefficients (R) being 0.56 and 0.47 for BC and dust concentrations, respectively). Averaged across all comparison stations, the modeled BC concentration is a factor of 1.8 lower than the observed concentrations, and the modeled dust concentration a factor of 1.4 higher.". We also read through the manuscript, and revised the statements like this.

Pg. 18, lines 364-365: I don't think it's really shown – except in a very hand-waving way, but certainly not quantitatively – that the model "does reasonably well" in simulating the spatial variations in surface atmospheric BC. So I'd omit this sentence and let Figure 2 speak for itself, unless you want to add an analysis showing quantitatively how well spatial variations are represented.

Reply: We thank the reviewer for pointing out this. We have deleted this sentence in the revised manuscript.

Pg. 19, lines 384-386: "This indicates that BC and dust accumulate within the snow column…". BC and dust will be added any time snow is added, but this doesn't make the MIXING RATIO at the surface higher, so this statement is misleading. It's not clear what point you're trying to make here.

Reply: We thank the reviewer for pointing out this. We agree with the reviewer, and identify the reason for larger BC/dust mixing ratios in snow in spring than in winter is the larger BC/dust deposition. Therefore we delete this statement and clarify in the revised manuscript: "This is due to larger deposition of BC/dust in spring than in winter, resulting from larger northward transport of BC/dust in spring (Figure not shown). Larger dust deposition in spring can also be partly explained by the larger dust emission in this season.".

Pg. 19, lines 388-391: "As observations only sampled the snow in one day …. Are given for the comparison." I don't understand what you are trying to say in this sentence; please re-write for better clarity.

Reply: We thank the reviewer for the comment. I use this sentence to state that the observation is based on short-term measurement (one day or tens of days), and how the model results are specifically derived for a fair comparison. In the revised manuscript, we delete this sentence as we already describe how to derive the simulation results for comparison with the observations in Section 3.

Pg. 19, lines 393-394. "The model reproduces reasonably the magnitude of observed BC-in-snow mass mixing ratios at most of the stations". Again, this judgement of "reasonably" is not really justified. As with the comparison to atmospheric concentrations, please just let the data speak for itself, and give quantification of agreement (R-squared; agree within a factor of XX; mean bias…)

Reply: We thank the reviewer for the comment. Following the reviewers' suggestion, we compare the simulated results with the observations more in the revised manuscript: "Simulated BC mixing ratios range from 8.3 ng g^{-1} to 30.6 ng g^{-1} at these sites, which are in the range of observations. Despite this, simulated BC-in-snow mass mixing ratios differ from the observations by a factor of up to 4 at some stations. Averaged across at the 17 sites, the simulated BC mass mixing ratio is 35% larger than the observed value.".

Pg. 20, lines 399-400. I don't see how this "indicates the northward transport of BC"

Reply: We thank the reviewer for pointing out this. We delete this statement in the revised manuscript.

Pg. 20, lines 405-207: "When snow is melted completely, BC-in-snow mixing ratio will be zero, but the model will average the simulation results at different time steps to derive the mean result." I am not clear what is being said here. IMPORTANT: Does this mean the average mixing ratio includes zeros where there is no snow present? If so, this is a problem, as this will incorrectly bias the average model

mixing ratios low. Modeled snow BC (or dust) mixing ratios should only be averaged across locations where snow is present. Please clarify.

Reply: We thank the reviewer for pointing out this. Yes, in the previous manuscript the monthly mean results from the model include "zero" values in some days where there is no snow present. We agree with the reviewer that this will incorrectly bias the average model mixing ratios low. To be consistent with the observations, in the revised manuscript, we use the daily BC mixing ratios when snow is present (snow water equivalent ≥1mm) and average the simulated results on the same date (month/day) as the observations for comparison. As expected, the modeled in-snow BC mixing ratios are larger compared to the monthly mean results at the stations where snow layers are thin (e.g., sites #9, #10, #15, and #16). In the revised manuscript, we clarify that: "As our simulation period (1981-2005) does not encompass the years 2013 and 2014, we will use the daily simulation results of C_{BC} on the same month/day (or months/days; Table 1) when the observations were made (i.e., we will ignore the exact year) and compare them (means and standard deviations) with the observations. At each station, daily simulation results are used only when snow is present (i.e., daily mean snow water equivalent ≥ 1mm). 1 mm is chosen to be consistent with the minimum snow-layer thickness in observations." (Section 3) and "Simulated BC mixing ratios range from 8.3 to 30.6 ng g^{-1} at these sites, which are in the range of observations. Despite this, simulated BC-in-snow mass mixing ratios differs from the observations by a factor of up to 4 at some stations. Averaged across at the 17 sites, the simulated BC mass mixing ratio is 35% larger than the observed value.." (Section 4.2).

Modeled dust mixing ratios are also averaged when snow is present: "For the simulation, we will calculate mean C_{dust} for May-June from daily C_{dust} on the days when snow is present (i.e., snow water equivalent ≥10 mm).". Snow water equivalent of 10 mm is equivalent to snow depth of 30-100 mm, which is comparable to the snow-layer interval of 3 cm in the observation.

Pg. 21, lines 425-427: This could also be due to compensating errors in BC deposition and snowfall.

Reply: We thank the reviewer for the comment. We add this explanation in the revised manuscript: "Another reason for the inconsistency of BC mass mixing ratios in snow and near-surface BC concentrations in the atmosphere may be related to the compensating errors in BC deposition and snowfall."

Pg. 21, line 439: Are the TSP numbers mass concentrations or number concentrations. I'm pretty sure it must be the former, but it would be good to specify.

Reply: They are mass concentrations. We have added "mass" before "concentrations".

Pg. 23, line 461: I think it would be good to point out that this amplification in spring is due in part to feedbacks

Reply: We have mentioned feedbacks in the revised manuscript: "This is because of the stronger solar insolation and larger albedo reduction due to snow aging, aerosol accumulation within snow, and feedbacks in spring."

Pg. 23, line 470: Note the correction in the annotated .pdf: SRE is a function of MIXING RATIO not MASS.

Reply: We have changed "mass values" to "mixing ratios".

Pg. 24, lines 496-497: "For the contribution of different aerosols, BC-induced springtime SRE is larger than dust-induced SRE in the five regions." This is a repeat of sentence on pg 23, lines 464-465.

Reply: We delete this sentence in the revised manuscript.

Pg. 25, lines 512 and 518: change "around the mountains" to "adjacent to the mountains" or "surrounding the mountains". "Around the mountains" could be misinterpreted to mean in the mountains. (Ah, the joys of English!)

Reply: We have changed "around the mountains" to "in the regions adjacent to the mountains".

Pg. 26, lines 524-525: I'd think it is SWE that's a stronger determinant here. Low snow fraction = lower area over which forcing is exerted, but with lower SWE the snow albedo feedback (via exposure of the underlying surface) occurs more readily.

Reply: We thank the reviewer for the comment. We agree with the reviewer and have changed this sentence to "For example, winter and spring snow water equivalent is mostly above 50 mm on the high mountains (see Figure 8 of Wu et al. (2017)).".

Pg. 26, lines 532-533: "… which is likely related to the large-scale circulation change due to the aerosol SDE." Nowhere is it shown that the aerosol SDE induces a largescale circulation change. You either need to show this here, as part of this analysis, or point to a reference where this is shown. It's not clear where this assertion is coming from.

Reply: We thank the reviewer for the comment. This assertion is based on our simulation results and we apology for not showing the results in the previous manuscript. In the revised manuscript, we have added the analysis of simulation results and clarified the assertion: "The increase of snowfall in Figure 9 is likely related to the large-scale circulation change due to aerosol SDE. Figure 10 shows wintertime tropospheric temperature and zonal winds in CTL and NoSDE simulations and their difference. In the NoSDE simulation, we have turned off the SDE not only in the Rocky Mountain region, but also in other regions of the globe. Due to aerosol SDE, temperature is increased in the high-latitudes of northern hemisphere (Figure 10c), which can reduce the meridional temperature gradient, thus leading to the weakening polar jet stream north of 50 ºN (Figure 10f). This suggests a shift to a more meridional wind pattern in winter, which can enhance the broader meanders and thus the formation of winter storms (Wu et al., 2017). Enhanced winter storm activity further reduces surface temperature to the north of Rocky Mountain region as well as in the northern part of Rocky Mountain region (Figures 8a and 10c). This, together with the increased temperature in the southwestern U.S. and southern part of Rocky Mountain region, increases meridional temperature gradient and leads to stronger westerly at 30-45ºN (Figure 10f). Stronger westerly at 30-45 ºN favors the water vapor transport from the Pacific Ocean. The enhance of winter storm activity and water vapor transport may lead to the increase of precipitation (mainly in terms of snowfall in winter). In spring, the change in temperature and zonal winds is similar to that in winter, but with a northward shift of the patterns as a result of northward movements of the polar jet stream and westerlies in spring (Figure not shown). Therefore, the change of snowfall is likely a result of circulation change induced by SDE from both the Rocky Mountain region and remote regions. It is worth isolating the impacts of SDE from the Rocky Mountain region and remote regions (e.g., high-latitudes) in the future."

Pg 26, line 540: "around 0.003-0.17degC". "Around 0.003" is a bit silly, since "around" implies approximate, but then you give 3 decimals of precision. Instead, say "around 0 to 0.17deg C" or (probably better) "around 0 to 0.2deg C".

Reply: We have changed "around 0.003-0.17 °C" to "around 0-0.2 °C".

Pg. 30, lines 624-626: I don't understand what you are trying to say here, regarding the aerosol SDE being "more significant" in July.

Reply: We stated aerosol SDE in July is more significant in terms of the larger relative change of runoff (the ratio of absolute runoff change to original runoff). To clarify, we have deleted "more significant" in the revised manuscript: "Runoff is relatively smaller in July versus in previous months, and aerosol SDE can reduce the runoff by 0.04 (8%), 0.17 (23%), and 0.06 mm day^{-1} (16%) in the three regions, respectively".

Pg. 31, line 634 "the model also reproduces observed distributions of near-surface atmospheric BC and dust…" vs pg 31, line 638 "BC concentrations are mostly underestimated" Which is it?? The former implies the modeled and observed values agree; the latter shows they do not.

Reply: We thank the reviewer for pointing out this. The former indicates the general spatial patterns are similar for simulation and observations, such as larger BC concentrations in West Coast/Southwester U.S. and smaller BC concentrations in Rocky Mountains/Great Plain. The latter applies to the comparison of BC concentrations in the Rocky Mountains. To clarify, we have deleted the former statement and only emphasized the comparison in the Rocky Mountain region: "Here we show that the model simulates similar magnitude of near-surface dust concentrations at most stations in the Rocky Mountain region compared to IMPROVE observations. The model tends to underestimate near-surface atmospheric BC concentrations mostly by a factor of 1.5-5 in the Rocky Mountain region.”.

Pg. 31, lines 641-645 (e.g. “closely related”). This is a rather optimistic qualitative statement about how the model does. As noted earlier, better is to just state quantitatively what the model vs. obs bias and correlation were.

Reply: We thank the reviewer for the comment. In the revised manuscript, we have deleted “Simulated aerosol-in-snow concentrations are closely related to the distributions of both snowpack and near-surface atmospheric aerosol concentrations.” and state quantitatively the comparison result: “Simulated BC-in-snow concentrations ranges from 2 to 50 ng g^{-1} in the Rocky Mountain region, and they are 35% larger than the observations for the average at the 17 sites.”.

Pg. 33, lines 674-675 “reproduces observed magnitudes” What does this mean? What is the metric here? Averaging across all sites? Please quantify.

Reply: We thank the reviewer for the comment. We mean the magnitudes of simulated BC-in-snow concentrations are comparable to at most stations (i.e., in the range of observations). To clarify, we have quantify the comparison result: “however, overestimates BC-in-snow concentrations by 35% for the average across the 17 observational sites.”.

Pg. 679: As noted earlier, the snowpack dust size distribution may skewed towards even larger sizes than the atmospheric distribution, which already has significant mass>10microns.

Reply: We thank the reviewer for the comment. In the revised manuscript, we have added the observation evidence from Reynolds et al. (2016) for significant contribution of larger particles to total dust mass. We have also emphasized the importance of larger particles in Abstract.

Figure 3: The yellow color for Utah and Nevada is pretty much invisible. Please use a different color.

Reply: We have changed the yellow color to black color in the revised manuscript.

Figure 5: Please state in the caption what the dashed lines represent.

Reply: We have added in the caption "The 1:1 (solid) and 1:5/5:1 (dash) lines are plotted for reference."

Figure 8: The black crosses are really difficult to see against the dark blue. Maybe try bright yellow, at least in panels c) through f)?

Reply: We thank the reviewer for the comment. We try bright yellow, but it looks a little messy. Therefore, we change the color scheme by not using the dark blue and keep the black crosses. The revised Figure 8 looks more clear now.

Please also note the supplement to this comment: https://www.atmos-chem-phys-discuss.net/acp-2017-799/acp-2017-799-RC2-supplement.pdf

Reply: We thank the reviewer for the edits, which improves the manuscript. We have includes these edits in the revised manuscript.