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 #1

This study describes a novel use of a variable resolution configuration of the Community Earth System Model (VR-CESM) to explore impacts of light absorbing aerosols in snow of the Rocky Mountains. Previously, these and other mountain ranges could not be adequately resolved in coarse resolution GCM studies to quantify impacts of aerosols on mountain snow. The configuration applied here represents the Rocky Mountains with 0.125 degree horizontal resolution, constituting a substantial improvement over previous global and even regional model simulations. This is a very thorough end-to-end study including evaluation of simulated atmospheric and in-snow aerosol concentrations against observations followed by analysis of radiative forcings, temperature response, and hydrological response to the presence of black carbon and dust. Overall, I find this to be an excellent, logically-organized, and well-written study. I have only minor comments.

Reply: We thank the reviewer for his/her detailed review and encouraging comments. The text and figures are revised as the reviewer suggested.

General comments:

The authors point out that simulated dust-in-snow concentrations are 1-2 orders of magnitude lower in the San Juans than measured by Skiles and Painter (2016) and simulated radiative forcing from dust is about an order of magnitude smaller. These are substantial biases. The authors also mention that dust particles larger than 10um are not included in the simulations, but comprise a majority of dust mass in measurements from this region. Is there good reason to believe that these model biases persist (and are of similar magnitude) throughout the study area, or do the authors believe these biases are somewhat unique to the San Juan Mountain area? If the former, I would consider mentioning in the abstract the omission of particles larger than 10um as a potential source of systemic bias in dust-in-snow SRE throughout the study area.

Reply: We thank the reviewer for the comments. We explore more extensively these biases and associated dust size distribution. It has been recognized that dust particles with the diameter larger than 10 μm can transport regionally for hundreds of kilometers, especially under favorable weather conditions, such as strong winds. Observations made by Reynolds et al. (2016) show that airborne dust mass concentration are mainly contributed from larger particles (diameter>10μm) in Utah-Colorado region. We mention that they also show large portion of dust mass in snow is from larger particles in Colorado, which we didn't mention in the previous manuscript. This supports for the importance of larger particles in snow. As the stations used in Reynolds et al. (2016) are widely distributed across the Southern Rockies, it is reasonable to believe that large portion of larger dust particles (diameter >10μm) exist in atmosphere and in snow across the Southern Rockies.

For the surface radiative effects (SRE), we have included the comparison of our results with another study, Skiles et al. (2015), which shows SRE by dust is smaller but in a similar magnitude (32-50 W/m^2) in Grand Mesa (~150 km to the north of SBBSA) compared to that in SBBSA in San Juan Mountains (50-65 W/m^2). Compared to their estimations, our simulated SRE by dust (reaching up to 2-5 W/m^2) is one magnitude smaller at both stations. Therefore, these model biases of SRE may persist throughout the Southern Rockies. In the revised manuscript, we have mentioned the persistence of biases in Southern Rockies in Abstract: "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 particles (with the diameter >10μm) in the model. This calls for the inclusion of larger particles into the model to reduce this discrepancy".

In Greater Yellowstone region and Northern Rockies which are farther from the dust source regions (Figure 1), there is no available observation for dust size distribution in atmosphere and in snow. It is possible that dust particles with diameter>10μm may still exist, but their mass concentrations should become smaller than in Southern Rockies and the biases of SRE caused by omission of dust particles with diameter>10μm should be smaller as well. We discuss this in Section 5: "Note that such bias in SRE may become smaller in the Greater Yellowstone region and

Northern Rockies which are farther from the dust source regions than Southern Rockies.".

The study acknowledges that use of a coarse resolution (1.9x2.5 degrees) BC emissions inventory could have biased the simulation, which was conducted at ~16 times higher resolution. In fact, the native resolution of the emissions inventory produced by Lamarque et al (2010) was 0.5 degrees (see abstract of that paper), so in fact finer resolution emissions could have been applied in this study. I do not suggest that the runs be conducted again, but I mention it so the authors are aware that higher resolution versions of their emissions data exist.

Reply: we thank the reviewer for pointing out the native resolution of the emissions inventory produced by Lamarque et al (2010). Although the native resolution is 0.5º ×0.5º, this dataset is further processed to be at the resolution of 1.9º×2.5º for its adoption in standard CESM model (at ~1º or ~2º). It is desirable that we directly process the dataset at its native resolution (0.5º×0.5º) for CESM model, which can benefit our high-resolution simulation to resolve more spatial variations of BC emissions. We plan to do this in the future. We have clarified in the revised manuscript in Section 2: "We note that BC emission data is natively at a resolution of 0.5º×0.5º (Lamarque et al, 2010). However, it is processed to be at a relatively coarse resolution of 1.9º×2.5º for adoption in standard CESM, which is used in this study." and "It is desirable to adopt BC emission at its native resolution for our high-resolution simulation. The sensitivity of our simulation results to the resolution of BC emission will be analyzed in a separate study.".

Please describe in more detail which version of the modal aerosol model (MAM) is applied here. i.e., is MAM3, MAM4, or MAM7 used? How, briefly, are black carbon and dust treated in this version of MAM/CESM?

Reply: We thank the reviewer for the comment. We use MAM3 in this study. We have described MAM3 and the treatment of BC and dust within MAM3 in the revised manuscript in Section 2: "Here, we use the 3-mode version of MAM (MAM3). These three modes are aitken, accumulation, and coarse modes. In MAM3, BC is treated in the accumulation mode. BC particles are instantaneously mixed with sulfate and other components in the accumulation mode once they are emitted. Dust particles with the

diameter range of 0.1-1 μm and 1-10 μm are emitted into the accumulation mode and coarse mode, respectively. Airborne aerosol particles are then transported by winds and delivered back to the land surface by both dry and wet deposition, as described in Liu et al. (2012). ".

Specific comments:

line 91: "except that" -> perhaps "except when" (grammar issue)

Reply: Done.

line 113: "by comparing against" -> "in comparison with"

Reply: Done.

line 132: "for for"

Reply: We have deleted a redundant "for".

line 216: "all aerosols except BC (dust) as Flanner et al." -> "all aerosols except BC (i.e., only dust in this case) as in Flanner et al."

Reply: Done.

line 217: "the five regions" -> "five regions"
Reply: Done.

line 293: "snow samples at a depth of 30cm" - Here, I suspect you mean "snow samples through a depth of 30 cm" (i.e. samples collected from 0 - 30cm depth).

Reply: We change "snow samples at a depth of 30cm" to "snow samples in the top 30 cm of the snow column".

line 301: "is mainly contributed from" -> "consists mainly of"

Reply: We change “is mainly contributed from” to “is mainly from”. We think that “consists mainly of” may apply to the number of particles, but not to the mass of particles.

line 309: "cycles" and "cycle" -> "circles" and "circle"

Reply: Done.

line 324: "although it is much weaker" -> "although they are much weaker"

Reply: Done.

line 398: "Rockiest" -> "Rockies"

Reply: We agree with the reviewer, but this sentence is not used in the revised manuscript (we recalculate simulated BC-in-snow concentration by using daily results instead of monthly-mean results, and the analysis of comparison results is re-written.)

line 405-408: The description of how a monthly-mean BC-in-snow concentration differs between the model and reality in cases where there is no snow during part of the month is unclear to me. Please elaborate a bit on this description, and if necessary describe any associated implications more clearly.

Reply: We thank the reviewer for pointing out this. In the previous manuscript, we derive the simulated BC-in-snow concentration from monthly model result. The monthly result is an average of the results for all the timesteps during the month. If snow is not present (e.g., completely melted) in some days, BC-in-snow concentration is set to zero in these days and the monthly mean accounts for these “zero” values during the month. For observations, only the BC samples within snow (snow depth ≥ 5 mm or snow water equivalent ≥ 1 mm) are analyzed and they are not “zero”.

To be consistent with the observations, in the revised manuscript, we use daily model output and derive the in-snow BC concentrations (with snow water equivalent ≥ 1 mm) on the same date (month/day) as the observations. By doing this, we eliminate the influence of “zero” values in the monthly model results. We clarify this in the

revised manuscript: "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, simulated daily mean C_{BC} is used only when snow is present (i.e., daily mean snow water equivalent ≥ 1mm) in the simulation. 1 mm is chosen to be consistent with the minimum snow-layer thickness in observations." (Section 3).

lines 498-502: In the discussion of dust SRE I would acknowledge again that the results do not include particles larger than 10um, which may/probably constitute the majority of dust-in-snow mass.

Reply: We thank the reviewer for the comment. We add the discussion: "Note that dust-induced SRE shown here doesn't take into account dust particles larger than 10 μm, which can constitute the majority of dust-in-snow mass (Reynolds et al., 2016). Therefore, our estimations of dust-induced SRE may be biased low."

line 525: "This suggests that snow on the high mountains is less susceptible to the aerosol SDE" - I would re-word this. A higher snow cover fraction does not necessarily imply lower aerosol SDE. Quite often it is the opposite.

Reply: We thank the reviewer for the comment. We agree with the reviewer and re-word this sentence: "Local aerosol SDE may also induce substantial impacts on the surface temperature and snowpack on the high mountains, but these impacts may be canceled out by the increase of snowfall (Figure 9f)."

line 543: "ratio of surface air temperature change to SRE" - I suggest emphasizing that the efficacy is defined here in terms of *local* delta T and SRE.

Reply: We change "ratio of surface air temperature change to SRE" to "ratio of local surface air temperature change to SRE over a specific region".

line 559: "... which corresponds to a fraction of ..." - The meaning of this fraction (apparently a fraction of snow cover fraction) is confusing. I suggest using the

terms "absolute" and "relative" to differentiate the two, or maybe just removing the relative fractions because I don't really think they are necessary.

Reply: We thank the reviewer for pointing out this. Following the reviewer's suggestion, we remove the relative fractions.

line 565: ".., the runoff includes the surface runoff and sub-surface runoff" - Is subsurface runoff examined at all in this study? If not, I would brielfy mention that here.

Reply: We thank the reviewer for the comment. We only show the total runoff in the previous manuscript. We combine surface runoff and subsurface runoff, as they both will flow to river and become discharge. Following the reviewer's suggestion, we clarify this and briefly mention the simulation results of surface/surface runoff in the revised manuscript: "Our simulations show that the spatial distribution and seasonal evolution of surface runoff and sub-surface runoff are generally similar to total runoff (Figure not shown), and surface runoff and subsurface runoff accounts for 30-40% and 60-70%, respectively, of annual total runoff in the mountains. Here we only show the simulation results of total runoff, as both surface runoff and sub-surface runoff will flow to rivers and become discharge.".

Figure 3 caption: Please define "cold season"

Reply: We add "(winter and spring)" after "cold season".

Figure 5: There is a lot of white space in this figure. I think the axis ranges could be narrowed a bit.

Reply: Now we have narrowed down the horizontal and vertical axis ranges.

Figure 8 caption: There are two references to "bottom row". The first should be "middle row"

Reply: We thank the reviewer for pointing out this. The first "bottom row" is changed to "middle row".

Figure 14: Does this depict surface runoff only, or total runoff (including the sub-surface component)?

Reply: We also show the simulation results of total runoff in the previous manuscript. We combine surface runoff and subsurface runoff, as they both will flow to river and become discharge. We clarify in the revised manuscript by change "runoff" before "total runoff including surface and subsurface runoff".