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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Impacts of absorbing aerosol deposition on snowpack and hydrologic cycle in the Rocky Mountain region based on variable-resolution CESM (VR-CESM) simulations" *by* Chenglai Wu et al.

Anonymous Referee #2

Received and published: 7 October 2017

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

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English/readability.

The following issues need addressing:

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

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.

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.

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.

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?

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?

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.

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."

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.

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.

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 sen-

tence; please re-write for better clarity.

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. . .)

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

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.

Pg. 21, lines 425-427: This could also be due to 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.

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

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

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.

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!)

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.

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 large-scale 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.

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”.

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.

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.

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.

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

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.

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

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

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)?

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

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