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Atmospheric Chemistry and Physics
Discussions

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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Response to reviewer RC2

We thank the reviewer for their helpful and insightful comments. We have done our best to address each concern.

Based on model simulations, the authors examine the skill of high resolution WRFChem on the impact of snow albedo darkening and radiative forcing over western USA. They evaluate the model simulation with various observations. The authors also

discussed the radiative differences between BC and dust and intercompared two different pathways (snow direct radiative effect and atmosphere direct radiative effect) of aerosol effect on surface water budget. The spatial and temporal variation of radiative effects are also discussed. The experiments and results are interesting and suitable for publication in ACP after major revision to address following concerns. 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, and other text has been moved to the supplement. Sec. 5.4 is now included along with Table 5 which highlights changes in meltout date. Major comments (MaCs) MaC1: More discussion on role of dust in the manuscript is needed along with clear explanation and analysis. A) why is dust induced SWE positive over Northern Rockies in Figure 8f,which is inconsistent with the fact that dust-ISRE is positive (Figure 7d) and dust-induced change in albedo is negative (Figure 8i). B) More aerosols always have a negative radiative effect at surface as it either scatters or absorbs incoming radiation at surface. Why is dust surface RE in Figure 12i positive? Although, the authors have tried to explain this by stating the differences in aerosol microphysics, I feel it is not clear. C) Also, this is found over the entire domain not only over the brighter snow surfaces as discussed latter. Therefore, explain in detail line 545 to 550. D)Detailed analysis and discussion should be done to explain why on doubling the dust concentration changes the sign/magnitude of dust induced perturbations on various variables nonlinearly (compared to that with initial dust). For example, in figure 14e, peak dust SDE in may end is ∼1.5 Wm2 and the corresponding dust-induced reduction in SWE SWE is ∼7% for Northern Rockies. This is very different from the situation in Figure 10 and 11. Dust SDE in Figure 11 is ∼1W/m2 and the corresponding dust-induced reduction in SWE is ∼ 2%. Please explain and discuss this nonlinearity.

Reply: We respond to this comment in segments below.

Why is dust induced SWE positive over Northern Rockies in Figure 8f, which is inconsistent with the fact that dust-ISRE is positive (Figure 7d) and dust-induced change in albedo is negative (Figure 8i).

Reply: We believe this peculiarity was the result of internal model variability. Simulations integrate independently even though they are initialized to the same file. Small differences in simulated physics (i.e. certain physical effects were disabled in certain experiments) led to small changes in storm timing/location in the different experiments. At some points characterized by positive SWE anomalies, even a single storm difference, specifically a difference that saw less snowfall in CNT, was enough to yield a March-June mean anomaly that was positive. Regionally averaged, it is clear that BC and dust SDEs contribute to SWE reductions (see Fig. 9b). Furthermore, dust SDE (and ARI) anomalies were assumed to be the linear difference between BC+dust SDE (ARI) and BC SDE (ARI) effects. This linearity assumption may have partially contributed to the positive SDE anomalies. To affirm our suspicious that internal model variability was driving strange SWE anomalies, we examined correlations between SWE, temperature, precipitation, cloud, and circulation anomalies with terrain height; they were unrevealing, and no coherent signature was found. It is believed that these positive SWE anomalies would most likely be uncommon if we conducted our experiments over many years and averaged over climate-relevant time scales. As such, we have added the following to the Sec. 5.1.1:

“We note that there are areas where LAP SDE leads to increased SWE amounts across a small fraction of gridcells (Fig. 8d). We believe this to be the result of internal model variability rather than a physical manifestation (Bassett et al. 2020). Examination of several grid points where the March-through-June mean SWE anomalies were positive revealed that fine-scale storm location and intensity differences between, for instance, CNT and noBCSDE were leading to positive SWE anomalies (not shown). We expect these positive SDE-induced SWE anomalies to be more uncommon if averaged over climate-relevant time scales. As will be shown in the next section, SDE SWE anomalies are negative when averaged regionally.”

Internal model variability must also be considered when interpreting ARI-induced anomalies. As such, the following sentence was added to Sec. 5.2.1, P2: “As with SDEs, internal model variability may be responsible for unintuitive SWE anomalies, but this issue was not examined in this study due to limited computational resources.”

More aerosols always have a negative radiative effect at surface as it either scatters or absorbs incoming radiation at surface. Why is dust surface RE in Figure 12i positive? Although, the authors have tried to explain this by stating the differences in aerosol microphysics, I feel it is not clear.

Reply: The surface radiative effects (REs) for BC+dust, BC, and dust are shown in Figs. 10g, h, and I, respectively. RE values are the sum of the shortwave and longwave REs. The key difference between BC and dust is that dust aerosols can downwell longwave energy. For dust aerosols of the right size and number concentration (Tegen and Lecis, 2012), this downwelled longwave energy can compensate for and even exceed solar dimming, yielding positive surface RE values (Fig. 10i). BC on the other hand is not a good attenuator of longwave energy because of its relatively small sizes. BC is however a very effective scatterer/absorber of incoming solar energy. Thus, BC dims the surface (Fig. 10h), yielding a very different surface RE compared to dust. BC-induced dimming dominates over dust-induced longwave warming, yielding a negative surface RE across the domain (Fig. 10g). The following discussion has been added to Sec 5.2.1 P3: “Specifically, the key difference between BC and dust is that dust aerosols can downwell several W m-2 of longwave energy. Depending on the dust size and number concentration, this downwelled longwave energy can dominate over the solar dimming, yielding positive surface RE values (Fig. 10i; Tegen and Lecis (2012)). BC meanwhile is not an effective attenuator of terrestrial energy because of its relatively small sizes. BC is however a very effective scatterer/absorber of incoming solar energy. Thus, BC dims the surface (Fig. 10h), yielding a very different surface RE compared to dust (Fig. 10i). BC-induced dimming exceeds the dust-induced longwave

RE, yielding a negative surface RE across the domain (Fig. 10g).”

Also, this is found over the entire domain not only over the brighter snow surfaces as discussed latter. Therefore, explain in detail line 545 to 550.

Reply: These lines provide an explanation of why the negative SWE anomalies from dust ARI tend to be maximized atop high-albedo surfaces of the Northern Rockies, especially since BC dimming dominates the LAP surface RE. Downwelled terrestrial radiation contributes positively to surface REs everywhere (Fig. 10i). Meanwhile, BC dims everywhere, with dimming decreased across high-albedo surfaces (Fig. 10h) by Eq. (3). Depressed solar dimming by BC over high-albedo surfaces coupled with ever-present downwelled terrestrial radiation by dust leads to a “less negative” total (BC+dust) RE at the surface and subsequently increased snow melting regionally averaged. Ultimately, the microphysical differences between the two aerosol types dictate in what wavebands they attenuate. The examination of these aerosols’ microphysical nature is beyond the scope of this manuscript. Clarification to these lines are made: “For atmospheric dust (and BC) particles residing over the high-albedo surface of the Northern Rockies, this means that there will be a higher chance of shortwave absorption at the surface through a larger S_totalˆâĘŞ. Together with dust longwave warming (Figs. 10i and 11d), dust ARI contribute to snowpack reductions across the Northern Rockies. SWE reductions are most prominent across the Northern Rockies subregion where smaller, more scattering dust particles are present. The physical process described here is similar to that noted in Stone et al. (2008) who examined the atmospheric REs of wildfire smoke across northern Alaska’s high albedo surface.”

Detailed analysis and discussion should be done to explain why on doubling the dust concentration changes the sign/magnitude of dust induced perturbations on various variables nonlinearly (compared to that with initial dust). For example, in figure 14e, peak dust SDE in may end is ∼1.5 Wm2 and the corresponding dust-induced reduction in SWE SWE is ∼7% for Northern Rockies. This is very different from the situation in Figure 10 and 11. Dust SDE in Figure 11 is ∼1W/m2 and the corresponding dust-

induced reduction in SWE is ∼2%. Please explain and discuss this nonlinearity.

Reply: Unfortunately, we cannot estimate the efficacy of dust SDE SWE reductions without further experiments being conducted. Four specific factors must be considered when evaluating the effects of increased dust emissions (Fig. 13 and Sec. 6). 1. The original dust ARI (SDEs) are considered to be linear differences of the noBCSDE (noBCARI) from the noSDE (noARI) experiment. However, DTF=2-CNT anomalies represent linear+nolinear effects of increased dust emissions, 2. We cannot explicitly determine whether SDE or ARI enhancement associated with increased dust emissions is driving DTF=2-CNT anomalies; undoubtedly both dust SDE and dust ARI increase in DTF=2, 3. Internal model variability effects, and 4. Increased dust emissions can impact cloud properties (indirect effects), which are not a focus of this study. We acknowledge the well-documented nonlinearity between snow-albedo and the mass concentration of impurities (e.g. Hadley and Kirchstetter 2012; Flanner et al. 2007; Painter et al. 2009; Wiscombe and Warren, 1980)

March-through-June cloud fraction differences between DTF=2 and CNT were found to be < 2% and generally positive, so (4) is discounted as a possibility for increasing snowmelt in DTF=2. (3) is probably not the issue here, as a regional average somewhat smooths noise associated with internal model variability, hence the prominence of negative SWE anomalies in Fig.13e across the Northern Rockies. The in-snow dust as a percentage is increased the most in DTF=2 relative to CNT across the Northern Rockies compared to any other subregion due to the fact that CNT simulates relatively low top-snow dust amounts of 2-4 mg m-2 (Fig. 6b). These amounts are increased by 50-80% in DTF=2 by small dust particles emitted to the southwest. Clearly, the ratio of the top snow dust amount in DTF=2 to CNT is maximized across the Northern Rockies, even as the increase in ISRE is maximized across the Utah Mountains. This exemplifies the nonlinear relationship between snow impurity amount, ISRE, and SWE reductions. It is thus believed that because of increased dust ARI-induced warming in DTF=2 (see Sec. 5.5, Eq. 3, Fig. 13b), in addition to the larger fractional change in snow impurities (and thus a stronger dust SDE), larger SWE reductions are simulated in DTF=2 than CNT across the Northern Rockies compared to the Utah Mountains. Of course, the assumption of linearity (1) in estimating the original dust SDE/ARI complicates this comparison, as DTF=2-CNT anomalies include linear and nonlinear effects. In any case, the main point of conducting the DTF=2 experiment was to assess the impacts of the simulated low dust bias, not to assess the effects of our linear assumption in assessing dust effects. We cannot completely address this comment without conducting further experiments. The following paragraph has been added to Sec. 6:

“While it can be seen that increased dust emissions have consequences on simulated meteorology, it cannot be determined whether a majority of changes in meteorological variables are due to enhancements in dust SDE or dust ARI without conducting further experiments. We did identify small increases in cloud amounts (by less than 2%; not shown). In-snow dust burdens, as a percentage, were increased the most across the northern subregions, although ISRE perturbations in DTF=2 were smaller compared to the southern subregions (Fig. 13a). However, perturbations to the surface RE were generally positive across high-elevation areas of the northern subregions, especially the Northern Rockies (Fig. 13b). Evaluating enhancements in dust SDE in the DTF=2 experiment is complicated by the nonlinear relationship between snow impurity amount and radiation absorption (Flanner et al. 2007, 2012; Painter et al., 2007; Hadley and Kirchstetter 2012; Wiscombe and Warren, 1980). DTF=2 enhancements of dust effects over CNT comprise linear and nonlinear ARI and SDE, whereas earlier computation of dust ARI and SDE were subject to a linearity assumption, further complicating the comparison of DTF=2-CNT anomalies with previously computed dust anomalies (Secs. 5.1, 5.2). We emphasize the limitations of our assumptions in quantifying dust effects, and call for further studies of dust SDE and ARI across this region.

MaC2: A relative issue is this paper is very long which dilutes the main findings. I strongly suggest the author to significantly shrink the length of this paper by moving relatively minor parts/figures to supplementary and organization better to highlight the

prime results.

Reply: Following revisions, the paper has a higher word count, but it has been significantly reorganized to present a less diluted product. Four figures have been added to the supplement. Also, 1 supplemental subsection and 5 appendices have been created to highlight less important details of the manuscript.

MaC3: Evaluation of snow cover duration should be included in the manuscript as authors report temporal shift in SWE as a main result

Reply: As can be seen if Fig. S1, CNT (and NOCHEM) melt out snow far too late compared to SNOTEL, especially across the Western WY, Utah, and Colorado (note these are regional averages of SNOTEL point observations, not our defined subregions). For our defined subregions (i.e. Greater Idaho, Northern Rockies, Utah Mountains, and Southern Rockies), melt out dates are also overpredicted by CNT be multiple weeks, which is consistent with the CNT/SNOTEL bias. In fact, meltout (SWE = 0 mm) is simulated only in Greater Idaho in CNT; all other subregions have simulated SWE on 1 August. Because our simulations were not run long enough to explicitly capture meltout on regional scales, we did not initially include these results in the manuscript.

With this is mind, we did evaluate snowmelt timing by comparing 1 August SWE in CNT with other experiments across our four subregions. For instance, if SWE = 15 mm in CNT and 18 mm in noBCD, we would find the day in noBCD that had a SWE value of 15 mm and compute the lag time. We found this lag time and used it as a proxy for meltout shifts for subregions in which we did not actually simulate melt out. These results are presented in Table 5. Section 5.4, “Changes in meltout timing”, has been added to discuss meltout changes.

MaC4: Figure 10: The variability in runoff perturbation should be sum of perturbations in precipitation and that in SWE, But this is not the case in Northern Rockies. The precipitation increase and SWE decrease, both are maximum in June, but the runoff maximum is in May, why? This is not clear and need analysis and discussion.

Reply: Correct, runoff comes from both precipitation and snowmelt, but we only consider the total runoff, and we did not output runoff from snowmelt and precipitation individually. It can be seen in Fig. 9c that precipitation anomalies are negative from late May through early June across the Northern Rockies. Meanwhile, runoff from accelerated snowmelt is presumably positive as runoff due to precipitation is reduced. We thus see a local minimum in runoff from late May into early June that coincides nicely with the negative precipitation anomalies. Runoff is still positive due to enhanced snowmelt by LAP effects.

MaC5: In the text, the study period is mentioned as march through June, but in figure 7 it is February through July. Why? Also, why is the evaluation period different from the period averaged for results. It should be consistent.

Reply: Nice catch! We chose to emphasize the March-through-June time frame in our spatial distribution figures highlighting LAP effects/properties because 1. Aerosol burdens are either increasing or are maximized during this time period, 2. The solar elevation is increasing, and 3. Snowpack is maximized. Thus, aerosol effects emphasized in this study should be maximized during this time period. We also wanted to increase the signal-to-noise ratio. As such, Figs. 7 and S2 showing the spatial distribution of ISRE/RE and aerosol burdens, respectively, have been changed to a March-through-June average, consistent with other aerosol-related figures. Otherwise, the meteorological figures/metrics remain a February-through-July average; their biases do not change significantly when averaged from March through June, so we keep the averages in Sec. 3.

Mac6: I feel BCD is a misnomer and should be better described as LAP, a common term in literature for these light absorbing particles.

Reply: Done

Mac7: The authors discuss the differences in this study to previous modelling studies (Wu et al 2018 and Qian et al.,2009) over the same region in detail. One important

difference between these 3 simulations is that they all are simulated at different spatial resolutions. The observed differences in the results related to surface elevation could also be due to the inherent variability in terrain height and thus snow depth and associated BCD-in-snow concentrations as also shown in a recent study by Sarangi et al.,2019, ACP (https://www.atmos-chem-phys.net/19/7105/2019/). This should be discussed in context.

Reply: This is a very significant paper and reinforces the motivation for this study. As such, the paper has been cited three times in the introduction.

MaC8: What is the difference between ISRE and SDE in the manuscript, it seems to be same and used inter-changeably. Again, what is the definition and formula for calculating surface RE? We don't see good spatial correlation between surface RE and corresponding 2-m temperature in many figures? Why? Please define these terms and calculations clearly in methodology near Section 2.4.

Reply: ISRE is the excess energy absorbed by snow due to impurities [W m-2] and is only physical where there is simulated snow. The SDE describes a physical process which begins with the deposition of LAPs on snow. This slightly darker snow absorbs more incoming solar energy, increases snowmelt, etc. The surface RE is computed using the methods in Ghan et a. (2012) across all grid cells. The calculation of REs is briefly mentioned in Appendix A5 and involves two steps. First, all aerosol effects are accounted for in the radiative calculations for upwelled and downwelled energy. Second, the same section of the code is called again, but with specific aerosols' radiative properties disabled. By subtracting the former with the latter, REs can be computed for different aerosol species. As for the poor correlation between the surface RE and 2-m temperature (T2), the surface energy surplus can be converted to sensible heat, latent heat or it can be conducted away to the overlying air or the snow. Since we emphasize REs over snow covered regions, this excess heat is conducted to the snow by absorbing LAPs. The snow melts isothermally under a phase change and we therefore do not see a high correlation between positive T2 and larger REs, especially across the highest elevations.

MaC9: Include tables like 3 and 4 for all the variables discussed in the manuscript.

Reply: We emphasize the time series of other variables in Fig. 9a-d, 11a-d, 12a-c, S3, and S4. We can convert to a table, but we believe this to be a better way to visualize LAP-induced perturbations to WUS weather.

MaC10: Line 479Âż it should be aerosol

Reply: Fixed

MaC11: Line 560Âżit should be difference

Reply: Fixed

References

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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-AC2-supplement.pdf

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