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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.

Anonymous Referee #1

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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 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.

Major comments: 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)?

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.

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.

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.

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.

Minor comments: Concerning the impact of a modified snow pack 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.

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.

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.

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.

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?

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.

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