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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Impacts of Aerosols on Seasonal Precipitation and Snowpack in California Based on Convection-Permitting WRF-Chem Simulations" *by* Longtao Wu et al.

Anonymous Referee #1

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Aerosols can induce large impacts on the regional climate and hydrologic cycles. Currently the aerosol effects are still not well understood, especially for the individual and combined effects of different underlying mechanisms (direct, indirect, and feedback).

This study presents a comparison of different aerosol effects including aerosol-radiation interaction (ARI), aerosol-cloud interaction (ACI), and aerosol-snow interaction (ASI) on the regional climate in California based on WRF-Chem simulations. The study also shows the different effects induced by local dust emissions, local anthropogenic emissions, and transportation. Overall, the manuscript is well written, and most of the content is well organized. The scientific findings are significant to our un-

derstanding of climatic effects of different aerosols. This study is useful for the relevant research community on unraveling the aerosol affects in climate and hydrologic cycles.

However, some statements are not clear and some of them may need further evidence. Part of the manuscript can be better organized for easy following. I have some suggestions and comments that I would like the authors to consider before the manuscript can be accepted for publication in ACP.

Major comments:

(1) Lines 254-256, Figure 3, Lines 36-40 (Abstract): The authors states that the model simulations represent reasonable magnitude of SWE, because SNOTEL data underestimates real SWE. They deduce the underestimate of SNOTEL SWE from "The main issue with weighing-type gauges for snowfall estimation is the undercatch of approximately 10%–15% due to wind (Serreze et al., 2001; Yang et al., 1998; Rasmussen et al., 2001). " (Lines 249-251). I should mention that snowfall is not SWE. They are measured differently: snowfall referring to a solid form of precipitation is measured by gauges, while SWE is measured using a snow pillow (https://www.wcc.nrcs.usda.gov/about/mon_automate.html). Therefore, underestimation of snowfall doesn't mean underestimation of SWE. If SNOTEL SWE is not underestimated compared to the reality, the model (with aerosol effects) may have large biases in SWE (up to $\sim$100 mm) (Figure 3b).

The authors state that inclusion of aerosol effects reduce the model biases (Abstract). Although it is generally true, it is not simply the case for a model simulation regarding the large uncertainties in current models. With aerosol effects, WRF-Chem reduces SWE biases by 0-60 mm (Figure 15), but still has the bias of $\sim$100 mm (mentioned above, if SNOTEL SWE is not biased low). Although the authors can still get the conclusion of reduction of SWE biases with aerosol effects, discussion on other reasons for the model biases (potentially larger than the biases that can be reduced by including aerosol effects) is desirable and helpful. In addition, model simulations are not always improved with the inclusion of aerosols effects. For example, CTRL simulation underestimates precipitation in April (Figure 3a). If the aerosol effects are removed, simulated precipitation is larger (Figure 14), which is more consistent with the observation.

For precipitation and temperature, there are multiple observations available for comparison with the model simulations. Without the investigations of the reliability of each observation, the selected observations may be arbitrary. Besides the CPC, DWR, and CIMIS observations used in this study, there are also other datasets (including a widely-used dataset, PRISM-Parameter-elevation Regressions on Independent Slopes Model) available but not included. The resolution of PRISM (4 km), much higher than CPC (0.25 degree) used, is also similar to the model resolution (4km). I am wondering how the simulation results are compared to the PRISM observation at similar resolution.

Overall, more investigation is needed to support the improvement of model performance when aerosol effects are included, by comparison of model results with more observation datasets and consideration of the reliability of these observations.

(2) Table 3, Lines 216-223: The authors decompose the effects of ARI, ACI, and ASI from these multiple experiment. Do they assume the linear combination of ARI, ACI, and ASI? It is possible that ARI, ACI, and ASI can be interacted to generate overall effects. CTRL-NARI (CTRL-NASI) may include the interaction of ARI/ACI and ARI/ASI (ARI/ASI and ACI/ASI), which may be different from NASI-NARS (NARI-NARS). If any difference between CTRL-NARI and NASI-NARS (CTRL-NASI and NARI-NARS) is found, it is also helpful if the authors can explicitly mention this nonlinear combination of ARI/ACI/ASI. Although it is difficult to identify the interaction of ARI, ACI, and ASI, at least some discussions are needed.

(3) Section 2: The authors describe the three pathways of aerosol effects in the order of ARI, ACI, and ASI in Introduction, but describe their representation in WRF-Chem in the

order of ASI, ARI, and ACI in Section 2. This tends to give the readers an impression that ASI is more important than ARI and ACI and the main focus of the paper. I think this is not exactly what the authors want to show. In addition, the model version and modifications lacks some clear outlines. For example, WRF-Chem is first designed to simulate aerosol cycle, such as by MOSAIC; ASI is further included by coupling SNICAR (in CLM4) with aerosol cycles. Therefore, it would be better if this section can be re-organized as follows: brief description of model framework (WRF-Chem and WRF), representation of aerosol cycles, and aerosol effects (in the order of ARI, ACI, and ASI as in Introduction). Following the model description, some configurations for the specified simulation (such as domain, resolution, initial and boundary conditions, emission files, etc) in this study can be presented. Lines 195-223 can be kept as it is.

(4) Table 2, Lines 199-215: I am wondering what kinds of chemical species are transported into the domain. Do these species include dust or anthropogenic aerosols? Please explicitly mention this. If they include dust, NoDust should be NoLocDust. If they include anthropogenic aerosols, NoAnth should be NoLocAnth. Since their domain only covers a small region of Southwest United States, is it possible that dust and anthropogenic aerosols are also transported from adjacent regions (California-Arizona borders, Arizona, New Mexico, and the country of Mexico)? The authors only mention the long-range transportation from Asia and Africa. Please also clarify this.

(5) Lines 261-273: The evaluation of model simulations are only on the atmospheric aerosol. This study lacks the evaluation of aerosol-in-snow concentrations. Reasonable simulations of airborne aerosols don't necessarily imply reasonable simulation of aerosol-in-snow distribution, as there are lots of processes going after aerosol deposition on snow. Although the observations may be limited, some basic examination of aerosol-in-snow concentrations and their evaluation (if possible) is desirable to increase the reliability of ASI in this study. The results can be put in the supplement.

Specific comments:

Title: There is a word “convection-permitting” in title, but it is not mentioned in the main text. To increase the significance, I would suggest adding some brief discussions on the benefit of convection-permitting WRF-Chem simulations in Introduction.

Lines 35-36: Please make the order of ARI, ASI, and ACI consistently throughout the paper.

Lines 46-47: Transported anthropogenic aerosols or transported aerosols?

Line 50: Please mention the year for the period (since there is only a year for comparison).

Lines 70-71: The most adiabatic structure of the atmosphere is not clear.

Lines 87-88: The short atmospheric residence time can’t cause geographical distributions. Compared to natural aerosols (dust), anthropogenic aerosols with smaller particles can be transported for a longer distance and a longer residence time. Please clarify.

Lines 191-192: Is the impact of aerosol on ice cloud formation included in the model?

Line 194: How long is the timestep?

Lines 197-199: If the results are similar, why are they still provided? Please clarify.

Line 222: Is NARS similar to the CTRL, except that ARI and ASI are not included?

Lines 237-238: Is it possible to find a reference for CPC? In addition, I cannot open the link for CPC data (Line 496).

Line 244: I am wondering how to get DWR data? What is the resolution? Is it gridded dataset or station measurement? It is not found in Data availability.

Line 245: Is it possible to find a reference for CIMIS? If so, please delete “http://www.cimis.water.ca.gov/”, since Data availability is the place to mention it.

Lines 249-251: Does this affect both CPC and DRW datasets? Please clarify it.

Lines 291-293: what period is used for the calculation of difference and for daily data?

Lines 317-319: Probably mention that increase in temperature by reduced snow amount also overwhelms the decrease of temperature which may be caused by more clouds.

Line 322: I cannot find the runoff results..

Line 327: what’s the aerosol-snow albedo feedback? Are you meaning snow-albedo feedback?

Lines 328-329: Please mention that reduced SWE can also initialize the snow albedo feedback.

Lines 347-348: The increased SWE can be canceled out to some extent by reduced snowfall (Lines 344-345). Please don’t just mention the increased SWE and reduced snowfall separately, but consider them together (northern part of Sierra: ARI>ACI; southern part of Sierra: ACI>ARI).

Lines 358-359: Please be aware that this only applies to the total runoff change, but not to the monthly change which the snowmelt change also contributes to.

Lines 372-374: The authors are talking about the relative change here. Why is the relative change of runoff smaller when the relative change of SWE is larger? This can be partly explained by the slightly smaller change of precipitation (both liquid and solid form of precipitation are converted to runoff, soil water, and evapotranspiration eventually). Is it possible that the change of evapotranspiration also contributes?

Line 397: what’s the orographic forcing?

Lines 423-424: The definition of surface runoff can be put earlier in Line 352 (when it appears at the first time).

Lines 425-426: If the authors are talking about total runoff (in an annual scale), surface runoff is mainly associated with precipitation. But in a monthly scale, surface runoff is mainly associated with rainfall and snowmelt, and a portion of snowfall will become surface snow accumulation (epically for the winter season). In the melting season, precipitation is mainly in the terms of rainfall, which will mostly become runoff. Please clarify this.

Lines 428-430: Please indicate this is consistent with change of SWE.

Line 431: Please add “less snowpack available for melting caused by” before “earlier snowmelt”.

Lines 462-463: Again, this is for longer time scale (e.g., annual). In a shorter time scale, runoff can be generated from snowmelt. This is actually one point in this study: seasonal cycle of runoff is modified by aerosols through the impacts of aerosol on snowpack.

Line 467: Probably add “less snowpack available for melting caused by” before “earlier snowmelt”. In the earlier period of snowmelt, the author can say there is more runoff due to earlier snowmelt. But in the late period of snowmelt, it is more correct to say that less runoff is due to less snowpack available for melting to generate runoff.

Lines 481-486: Does underestimation of AOD imply that the aerosol effects are also biased low here? If so, please explicitly mention it.

Lines 489-492: The authors have mentioned that aerosol effect on ice cloud formation is not explicitly treated in the model (Line 314). They also mentioned the potential significance of aerosol effect on snow formation (Lines 122-124). May the limitation of the model (i.e., inexplicit treatment of aerosol effect on ice cloud formation) affect the results presented here? It will be helpful to add a brief discussion.

Figures: Surface runoff is one of key variables the authors focus on. However, the authors don’t present any spatial distribution and temporal evolution as other variables (precipitation, SWE, T2). I would suggest adding the spatial distribution and temporal evolution of runoff as well as spatial distribution of runoff change by aerosols. They

can be put in supplement.

Figure 1: If possible, please provide some indicators for the main mountains (including Sierra Nevada and Klamath Mountains) and valleys, which can be easily referred to in the main text. This will help the general readers of the journal.

Figure 3 captions, Lines 791-794: I would say “from CTRL simulations and xxx observations” instead of “simulated from CTRL and the observations from xxx”. In addition, do (a) and (c) refer to a regional mean? Please clarify.

Figure 3: X-axis in (c) is overlaid by white shaded box.

Figure 5: I am wondering how the authors do the significant test, as there is only one year simulation for each experiment.

Figures 6-12: Can the result of significant test be shown as in Figure 5? This is normally required as the authors mention multiple times of “significant” in the text (Lines 304, 313, 317, 326, 339, 369, 479).

Figures 14-17: Please add the “zero” line in the figures for easy viewing.

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