Review of:
"Aerosol impacts on California winter clouds and precipitation during CalWater 2011: local pollution vs. long-range transported dust"

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Accept with Major Revisions.

This manuscript offers a comprehensive examination of the role of dust aerosols as ice nuclei particles (INP) and their impact on winter orographic precipitation in California during CalWater 2011. I think the authors do a fine job of providing insight into how these INP modulate snow production via ice crystal nucleation, vapor growth, and riming processes. The dust particles appear to make a substantial difference in snow production as well as total surface precipitation. These sorts of modulations are of key importance from a hydrological perspective to water resources. I recommend acceptance following major revisions and answers to some key fundamental question below. Based on the information given in the paper, I do question the magnitude of the changes in snow and precipitation as they relate to new parameterizations. The authors do state in the paper that their results may represent an upper bound of INP effects, but this reviewer believes this may be in part to mis-application of the ice nucleation parameterization and/or in the applied treatment of dust particles only as INP when immersion-freezing is the main ice forming mechanism. The details of my concerns are given below.

1. page 19929, lines 16-17: You infer that the dust residues in the Mar02 case (less cold rain) come from their being consumed as INP, but it could be just as likely that they were initially activated as CCN. (You note earlier that dust can act as both CCN and INP). If it's inconclusive how the dust entered precipitation residuals it is best not to choose to speculate on one mechanism over another.

2. page 19932 last paragraph: The paper needs more explicit explanation of how you applied the DeMott formula. This formula returns the total number of ice crystals formed from the INP field for a given temperature (assumed water saturation). For example, if you have a stationary air parcel at a given temperature, then you will create X new ice crystals via the DeMott formula and then subtract the number of INP from the available field. However, the next timestep you should not be applying the DeMott formula again within this stationary parcel since you will have already activated the total number of INP possible for the given temperature. Further nucleation should only be occurring if that parcel becomes colder. Otherwise, over-nucleation would likely be occurring.

3. page 19933, lines 15-21: Why were the initialization and boundary conditions treated differently in the Mar02 case? Was this necessary to get a realistic simulation?

4. page 19934, lines 5-20: What were the median radii of the aerosol distributions being used in the simulations?

5. page 19935, lines 2-5: This is of concern. You state that dust may act as CCN, yet you are excluding these particles from activating and undergoing cloud droplet nucleation. As such you are preventing them from potentially being nucleation-scavenged. A more realistic treatment would be to allow them to behave as CCN and track them to see if they are lofted to colder temperatures in which they can then act as immersion-freezing nuclei. By separating the dust particles and saying they only act as INP you are very likely getting a potentially strong over-ice-nucleation bias, especially since an increase in dust from 1 /L to 2-4/cm3 is a 3+ fold increase in number concentration.

6. page 19935, line 23: Are background INP particles removed upon nucleation of ice particles?

7. page 19936, lines 17-19: The sentence beginning "The surface RH values" should be removed. There are so many potential causes of misprediction of precipitation, and I would suspect a difference in RH is not one of the potential primary causes, unless of course the RH differences are substantial. If you are going to keep this speculative statement, then you should report what the RH difference were between the model and obs.

8. page 19936, starting line 20: Where was the model sampled to obtain the profiles in figure 5? Are these averaged profiles or are they constructed to match the flight locations for the observation times?

9. page 19937, lines 18-19: The statement starting with "possible related to the lateral boundary conditions" should also be removed. This is another speculative statement without evidence to back this up.

10. page 19938, lines 7-8: Why do you show condensate totals at the lowest model level in kg/kg? Viewing totals in this manner could skew interpretations since a kg of air at the surface near sea level is quite different than that over the Sierras. Why not use total accumulated precipitation values for comparisons.

11. page 19940 lines 22-25 and page 19941 lines 6-7: Perhaps I'm missing something here, but in the former paragraph you state that precipitation from the central valley to the windward slope is reduced by 5-9% when local pollution is increased, and then in the latter paragraph you say there is an increase in precipitation by CCN mainly on the windward slope. Can you please clarify?

13. General comment: Several times in the paper you talk about ice growth by the WBF process and riming, but you don't show any plots to address the degree to which this is occurring. Much of the snow growth could be occurring outside of regions of WBF and riming growth. A recent paper by Saleeby et al. (2013, JAMC) shows that in multiple cases the primary snow growth is vapor deposition away from areas of riming and WBF growth. Riming and WBF contribute to the total snow water, but are not necessarily the primary growth mechanisms. Lastly, the WBF and riming processes would be acting to buffer one another. Increased WBF ice growth would reduce droplet size or number and

should reduce riming. These arguments needs a bit more evidence rather than speculation. Information on these growth mechanisms can certainly be output from the model.

Figures 1, 4, 5, 9, 10, 11, 13, and 14 are all too small. It was very difficult to see in the figures what you report in the text. These need to be made larger before publication. Also, in the majority of the figures the fonts need to be larger and darker.

Figure 8: The labels that indicate the simulations are different from those used in the test. Please keep these consistent to avoid confusion.

Figure 11: These panels are labeled as “Diff in Accumulated Rain”. Are these indeed for rain only? If so, then you should also show the differences in Accumulated Snow.

Technical comments:

1. page 19924, line 8: change “a series CalWater” to “a series of CalWater”.
2. page 19930, line 3: “gages” should be spelled “gauges”.