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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Impact of interannual variations in aerosol particle sources on orographic precipitation over California's Central Sierra Nevada" *by* J. M. Creamean et al.

Anonymous Referee #1

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Review of Creamean et al. "Impact of interannual variations in aerosol particle sources on orographic precipitation over California's Central Sierra Nevada"

In this manuscript Creamean et al. describe studies of particles 'released' from ice-phase precipitation in the Sierra Nevada snowpack. The particles are analyzed using single particle mass spectrometry and the authors try to draw correlations and conclusions over three winter seasons of data (2009, 10 and 11).

This is a reasonable important advancement in the field although this is minimized by the fact that it forms the third in a set of three papers. The references Ault et al. (2011)

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already described the 2009 data while Creamean et al. (2013) described the 2011 data. Thus the only new data in this paper is the 2010 data set and the fact the authors attempt to use these three sets to draw interannual statements on aerosol sources and effect on precipitation. These are generalized as 'dust and biological particles in the first year, biomass burning and pollution in year two, and long range transport in the 3rd (note as an aside that 'long range transport from distant sources', the phrase used in the manuscript, is redundant and the last three words can be eliminated in all cases).

My major concern here is the level of repeat of previous work (see this paragraph and next regarding the figures). As a whole, this paper seems to just meet the minimum requirements for publication in ACP. There is a bit of new data here - the addition of one season and some slightly expanded correlations that result, but really the authors appear to have published essentially all this data and conclusions before. I sincerely hope that there are not going to be yearly publications from this study; I, personally, think the minimal new data presented here just barely passes for justification of a new paper. Unless something new is really learned in a future year I hope the authors will step away from such minimal incremental additions to the literature.

Regarding figures, I went back and re-read the highly referenced Ault et al. (2011) and Creamean et al. (2013) – much material is cited instead of appearing here. What is rather surprising is that many figures here are essentially repeats of what was published previously. Figure 1 in Ault et al. is Figure 1 here (Ault et al. actually contains more data) – please eliminate and use reference as is done in the text ; the representative spectra shown in Figure 2 appear identical – but contain less information than Fig. S2 of Creamean et al. (2013) ; Figure 3 here appears to repeat the data from Figure 5 of Ault et al. ; Figure 5 here appears to repeat the data from Figure 1 of Creamean et al. I will ask the editor to decide how to handle this but the authors should remove these repeated data figures and instead reference the material.

That said, this manuscript does represent a chance for the authors to explain in depth some points that don't seem to have been previously addressed and I hope these will

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be done in revision:

1. The technique is interesting. In essence, ice-phase precipitation and collected. The trapped particles are then analyzed and the authors draw some correlations. What isn't discussed here – and only minimally in the references to the past work – is what fraction of the analyzed particles were actually responsible for ice nucleation? What fraction are scavenged from the particle phase? What fraction are scavenged as droplets or other ice was collected during precipitation? As it reads the authors seem to suggest that all particles were ice nucleating agents but this can't be true. A reasonable calculation for the different meteorological conditions is warranted and should be included in a revised manuscript. Without it I find it hard to believe the correlations are statistically relevant. I'm thinking here of past references from the DeMott group who indicate from ice chamber measurements that only something like 1 in 10^5 or 6 particles act as ice nuclei. If each precipitation element were to scavenge say 10 or 100 particles would the authors draw a reasonable correlation that had anything to do with the formation of precipitation or only the background aerosol? 2. Speaking of which, I'm somewhat surprised by the lack of references to the work of DeMott, especially field studies, in this paper. Indeed most of the non-self references to ice nucleation here are laboratory studies. It would be good to include a solid section on past studies of ice nucleating particles regarding the above comment on abundance versus scavenged particles. 3. I notice that in the text "dust/biological" and "dust and biological" are almost always grouped together and yet the figures separate these groups. Can the authors explain why these two are carried through as a group but then separated in the figures? Perhaps more specifically, can the authors offer a correlation of these two categories? Is the implication here that the two have the same source? That seems to be the implication but doesn't appear to have been actually proven. 4. Something not described in any detail here is the lack of soluble species in the figures. I think I understand that this material – perhaps sulfate or sea salt or water soluble organics – would be 'lost' when the ice-phase precipitation is melted. That seems reasonable if one is looking at ice nucleating particles but what about for the CCN? Looking at figures 3, 4 and 5 during

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‘rain’ events (that is, the CCN conclusions drawn in the paper) what is the expectation that the insoluble components represent any significant fraction of the droplet forming particles? Aren’t these essentially all expected to be hygroscopic – sulfates and the like? This makes me wonder if any of these conclusions are at all reasonable or only an artifact of the method.

Interactive comment on Atmos. Chem. Phys. Discuss., 15, 931, 2015.