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.

- We appreciate this reviewer's comments and his/her time in reviewing our paper. Please see our detailed responses to each comment 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.

- Yes, there is a possibility that dust could act as CCN. However, ice nuclei have to exist to form ice at temperature above -36 $^{\circ}$C. The clouds on MAR02 were warm as stated in the paper (T> -15 $^{\circ}$C generally). If all the dust particles were activated as CCN and there were no INP, there would not be any ice, yet ice was measured in this case. Therefore, we inferred that some of dust/bio particles have to be effective ice nuclei.
To be clearer, we have reworded that part of text as "Still, dust and biological aerosols were found in 60-70% of precipitation residues and about 10% of cloud residues (Creamean et al. 2013). Dust aerosols can be activated as CCN and thereby enter clouds and precipitation. However, since ice was measured in these clouds, it is possible that some dust or mixtures of dust and biological aerosols (referred to as "bio" hereafter) interacted with some clouds that were cold enough to form ice in this case

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.

- The reviewer has raised a longstanding issue for which the modeling community has not been able to develop a perfect solution. Since most models use the Eulerian formulation, air parcels cannot be tracked as the reviewer suggested, i.e., (no nucleation if the parcel temperature is not changed). The air in the next time step is not the same as the current timestep for a given grid box in the model. Although the cloud temperature may not change much between two time steps, nucleation still can happen when new INP move into the grid box with the prognostic INP. Besides using prognostic INP, we also applied an upper limit for nucleation, i.e., the total ice concentration at a grid point cannot exceed the initial INP concentration. With these constraints, over-nucleation did not appear to be a problem in our simulation. Furthermore, the modeled ice particle concentrations (Ni) were validated with in-situ aircraft data as shown in Section 4. The good agreement with the observed Ni suggests that over-nucleation may not be a problem.

We have added the following additional description about our implementation of DeMott et al. 2013, which should be much clearer now. Thanks for the comment.

"An upper limit of ice particle concentration was applied after nucleation to prevent excessive nucleation, i.e., the total ice particle concentration can not exceed the initial INP concentration set at a specific grid. With the prognostic INP and the upper limit constraint, we are able to reasonably simulate ice particle concentrations."

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?

- Yes. This was stated in the sentence "Different nesting approaches and large-scale data are used for the FEB16 and MAR02 cases to achieve more realistic results compared with observations". We were not able to get the observed cloud system for FEB16 when we used the NARR data (clouds stayed in the northern part of the domain only). After switching to NAM data, the cloud system was simulated much better. This indicates that the synoptic conditions are important for realistically simulating the cloud systems in the two cases and different forecast and reanalysis products do provide synoptic conditions different enough to affect the simulation of cloud systems.

4. page 19934, lines 5-20: What were the median radii of the aerosol distributions being used in the simulations?
- The smallest and largest CCN bin sizes (in radius) are 0.05 and 2 microns for FEB16, and 0.063 and 2 microns for MAR02, respectively. This has been added to the manuscript.

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.

- The reviewer's point is valid in general. However, for these cases, the dust layer is embedded in the stably-stratified region of clouds and vertical transport is not a concern here (unlike the case of deep convection with dust in the PBL). We admit there is a big jump of INP from the background 1 /L to dust layer of 2-4/cm3, which may amplify the dust effects. Hence we noted that the dust effects shown in this study could be treated as the upper bound in Discussion (Section 5). For California winter clouds over the hills, INP may be scarce when there is no long-range transported dust. The CFDC-measured ice nuclei were only 0.1-1 L^{-1} on Feb 15 when there was no dust, and INP are virtually un-measureable in marine layers.

6. page 19935, line 23: Are background INP particles removed upon nucleation of ice particles?
-Yes. The sentence is changed to reflect your point as "Note that the background INP of 1 L^{-1} (Table 1) is set to be uniform vertically in the NoDust runs at the initial timestep. It is used as input to the ice nucleation parameterization of DeMott et al. (2013) and the contact freezing parameterizations described in Section 3.1.2 for calculation of ice nucleation and is treated prognostically similar to dust 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.
- Removed as suggested.

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?
- We have added statements "The model results are averaged over the cloudy points identified by the detection limit of each instruments in the aircraft-measurement domain instead of the flight track due to the extreme heterogeneous nature of the clouds in this region" to the figure caption.

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.
- Deleted as suggested.

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.

   - As suggested by Reviewer #1, the unit kg kg^{-1} has been transformed to kg m^{-3} in

Figure 8. The results have bee described accordingly in the initial two paragraphs of Section 4.2 (most of the results are the same except that rain mass concentrations is decreased by CCN now, which is consistent with the changes of in-cloud raindrop mass).

- Since we want to identify how rain and snow are affected respectively (not just the total precipitation), the rain and snow near the surface are examined here. Our surface precipitation in mm from the model output is calculated based on the rain density (1.0 kg/m3) only. We realized that it is not appropriate to use $T < 0\ ^{\circ}C$ for snow and $T > 0\ ^{\circ}C$ for rain at the surface (we tried that anyway but got confusing results).

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?

- Sorry for the confusion. Now the statement in the previous paragraph reads as "Fig. 11d shows that the surface precipitation from the Central Valley to the lower part of windward slope of the mountains is reduced by 5-9%..." And the statement on page 19941 lines 6-7 reads as "The increase of precipitation by CCN occurs mainly on the upper slopes of the Sierra Nevada Mountains (Fig. 11)".

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 a/'way 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.

- Now we have added quantitative results about the WBF and riming growth. As the paper already has too many figures (15), we did not include an additional figure but stated the quantitative changes. The major text we have added to Section 4.2 is "The increase of snow is because of the stronger WBF and riming processes as the total riming growth is increased by 3 times and the total ice deposition growth is increased by 5 times in LoCCN&Dust compared with LoCCN&NoDust. We also see that ice deposition growth is dominant, which is about 20 times larger than the rimming growth in both dust and non-dust cases, consistent with the findings of Saleeby et al. (2013). Note that riming efficiency may be reduced due to the smaller ice particles size in the dust run, but the riming occurs more extensively due to increased ice particle number concentrations". Other associated text has been changed to be more specific too.
- In our opinion, it is difficult to distinguish vapor deposition areas from areas of

riming and WBF since it could be that the areas are the results of riming or WBF. For example, the WBF process could lead to the disappearance of droplets at certain grid points. Then clouds in those grids become pure ice-phase only, and it is not reasonable to count those grids as non-WBF or non-riming areas. Therefore, we examine the changes in total riming and total deposition growth over all mixed- and ice –phase clouds.

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.
- The figure size and font look very different from the manuscript that we provided in which the figures are larger and clearer. We have enlarged the font and figure sizes of those figures in the revised manuscript.

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

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 snow?
- The y-axis label has been changed to "Diff. in precipitation" to be consistent with the caption.