Anonymous Referee #1

The paper Aerosol impacts on California winter clouds and precipitation during CalWater 2011: local pollution vs. long-range transported dust investigates the impact of dust and anthropogenic aerosol on precipitation in California. For this a model framework with sophisticated cloud microphysics and prescribed initial concentrations of IN and CCN was used. The study is based on the CalWater 2011 field campaign investigating the hypotheses derived from the observations with numerical simulations. For this a set of sensitivity simulations was performed and validated with the available observations.

The impact of aerosol particles on the distribution and amount of precipitation is still poorly understood. The paper is a good contribution to improve our understanding of how different aerosol types can impact the precipitation formation and therefore the water availability in regions like California.

I find the paper suitable for publication in Atmospheric Chemistry and Physics after the following major and minor comments have been taken into account.

- We appreciate the reviewer for the careful review of the paper. We have modified the paper accordingly to address all of the comments. Please see our point-by-point responses below.

Major comments

• simulation design

As discussed by the authors the chosen boundary conditions of CCN have a very strong impact on the discussed results, especially in the MAR02 case.

I appreciate the detailed discussion of this problem by the authors but strongly recommend to rerun at least the MAR02 case using the increased CCN also in the boundary conditions (e.g. only at the boundaries crossing the Central Valley and coastal plains). In my opinion, the currently used boundary setup does not allow for a representative analysis of the CCN sensitivity in the MAR02 case.

Because the entire CCN profile was increased, the difference in the updraft mass fluxes between case FEB16 and MAR02 is in my opinion of minor importance for the different resulting CCN sensitivities. The profiles in Fig. 12d can only be explained by the advection of low concentrations from the boundaries (or a very efficient sink of CCN in the upper layers).

Are the dust concentrations also effected by the boundary conditions? If yes, I also strongly recommend to include/exclude the dust layer in the boundary conditions.

Please specify in more detail how the boundary conditions are treated in the individual simulations. (be more specific at p.19935 line 7 – What are the sources? Constant boundary concentrations? ...; e.g. extend Table 1)

- As suggested by the reviewer we did sensitivity tests for MAR02 to evaluate our point regarding the small CCN effects due to dilution by clean air from the lateral boundaries in the polluted case. The results show that indeed if we set the air at the southern and western boundaries (wind blows from southwest) as polluted (i.e., high CCN), then the CCN concentrations above 1 km are not diluted except around the cloud base where nucleation scavenging occurs (see figure below). Then CCN effects on Nc and Nr are much larger (red lines in the figure below) compared with HiCCN&Dust in which CCN at the boundary condition are set to be the same as the low CCN run (LoCCN&Dust) to mimic the local pollution only. Wind from the southwest brings in clean maritime air that would dilute the pollution produced locally in Central Valley, leading to small impact of local pollution on the clouds and precipitation. We maintain that the simulation setup for HiCCN&Dust described in the paper represents a more realistic condition to evaluate the impacts of local pollution on clouds and precipitation. However, the sensitivity test and the results have been included in the revised paper to discuss the effects of CCN boundary conditions on simulating CCN effects in the region.

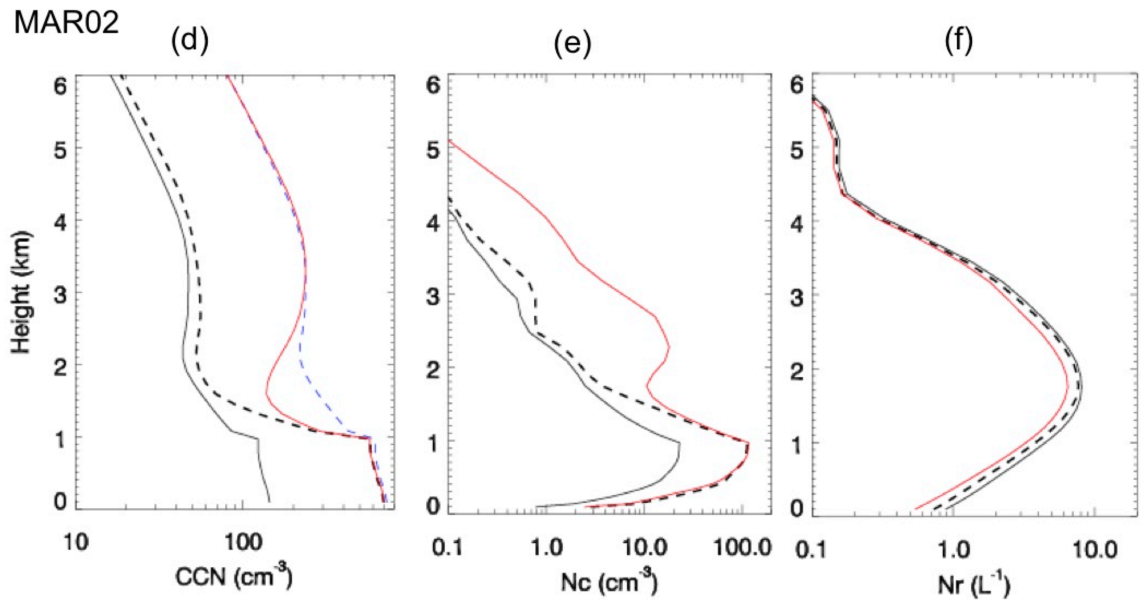

Figure title: Vertical profiles of CCN, N_c, and N_r from LoCCN&Dust (black solid line) and HiCCN&Dust (black dashed line) for MAR02 (d, e, f). The red line denotes a sensitivity run which is based on HiCCN&Dust but has the polluted CCN concentrations at the south and west boundaries. The data are averaged over the regions where the elevated CCN are applied in HiCCN&Dust (i.e., land with sea levels < 200 m) during 0-23:00 UTC. The dashed blue line in (d) denotes the increase of CCN from Base (black line) by 5 times.

- As stated in the title and discussed in more details in the paper, we are examining the impact from long-rang transport dust (not local dust). Therefore, for the runs with dust, dust concentration at the boundary conditions is set to be the same as the initial dust concentration in the dust layer. Performing simulations without dust included in the lateral boundary conditions (i.e.. without dust advected into the domain) would not be useful to answer our specific question of the impacts of long range dust transport.

- We have added more details in the descriptions of boundary conditions of CCN and INP (last paragraph of Section 3.2), and also extended Table 1 to include the setup of the lateral boundary conditions.

• **model description**

Please add more details to the model description, since it is important to understand the results. How are the prognostic CCN treated? There is no Fan 2009a in the references only Fan 2009, but I do not find the description of the CCN treatment in this paper. (Is Khain et al. 2004 the exact reference for the CCN treatment?) Please include information about the assumed composition and size distribution of the aerosol/CCN or the shape of the CCN spectra and how the activation is treated within the model.

- Fan et al 2009a has been changed to Fan et al. 2009. Khain et al. 2004 is the correct reference for CCN treatment. It provided the prognostic equation and detailed description in Section 2a and 2b.
- The description about how CCN are prognostically treated is in Section 3.1.1, i.e., "The CCN size distribution is calculated prognostically with sinks and sources, which include advection, nucleation, and CCN regeneration from droplet evaporation (Fan et al., 2009). Scavenging of CCN by precipitation is not considered". The CCN setup for each case such as size spectrum is provided in the case design section (Section 3.2; 2nd paragraph of Page 19934). To address the reviewer's concern about how CCN are activated, we have added "CCN activation is calculated according to the Kohler theory, i.e., CCN with radii exceeding the critical value that is calculated based on supersaturation at a grid point are activated to become droplets, and the corresponding bins of the CCN spectra are emptied". About the composition, we also have added a sentence to Section 3.2, i.e., "Sea salt composition is assumed for CCN activation since the air was under relatively pristine marine aerosol conditions (Rosenfeld et al., 2013). This also makes our setup of total CCN concentrations based on the observed droplet concentrations valid since sea salt is easily activated under supersaturation.".

What is your definition of CCN? All sizes and types of aerosol particles? (the number concentrations of 32cm□3 and 145 cm□3 are really low in this case) Or cloud condensation nuclei at a specific supersaturation or particles above a certain size?

-Section 3.2 provides the following details about CCN definition. The initial CCN size distribution for the base run is set using the PCASP measurements under cloud-free conditions and the total CCN number is adjusted based on the CDP-measured cloud droplet concentrations. Since the observed cloud droplet concentrations are about 30 cm^{-3} on FEB16 and about 120 cm^{-3} on MAR02, the total CCN concentrations used in the base run are ~ 32 cm^{-3} for FEB16 and 145 cm^{3} for MAR02. 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.

How are the prognostic INP treated? (size bins, size distribution or a single tracer?) What are the sinks and sources? (p.19932 line 3). What is the assumed size distribution (p. 19932 line 20) How is the ice nucleation rate calculated? (in Khain et al. 2004 the derivative of the functional form of the IN spectra was explicitly used to calculate dNice=dt)

- The following text has been added to p. 19932 to be more clear, "Therefore, besides horizontal and vertical advection, the INP sink terms are the immersion and contact freezing as described above and the source terms are the initial and boundary conditions that are described in Section 3.2 for the case setup. Since INP number concentrations predicted by the DeMott et al. (2013) parameterization reference the number concentrations of aerosol particles larger than 0.5 μm, we represent dust with a single prognostic parameter in this study". To address your question on ice nucleation rate, the DeMott et al. 2013 parameterization gives the nucleated ice crystal number concentration (not a rate) by taking an aerosol concentration as described in the same paragraph.

It is hard to follow the explanation of why you treat no deposition nucleation. (line 1 - 10 page 19933) Please explain in more detail why the different ice particles sizes are an indicator that deposition nucleation can be neglected (to much nucleating particles?, nucleation in "wrong" growth regimes?,...). In my opinion Fig. 2 is unnecessary, because it includes no crucial information for the study.

- We have added descriptions about why deposition nucleation produces many small ice crystals. Figure 2 is necessary for supporting our point about large ice particles. The part of text about why we did not employ deposition nucleation has been modified as "the upper limit constraint, we are able to reasonably simulate ice particle concentrations. We do not include a parameterization for deposition nucleation in the simulations performed. Note that deposition nucleation of mineral dust is not supported, except for the largest supermicron particles, by laboratory studies in mixed-phase cloud conditions (DeMott et al. 2011; Hoose and Moehler, 2012; Sullivan et al. 2010; Tobo et al., 2012), nor in field observations (e.g., Stith et al. 2009; Field et al. 2012). In a test using the deposition parameterization of Meyers et al. (1992) to predict INP concentrations, but connected with dust concentrations on the basis of van den Heever et al. (2006), it was found that more than half of ice particles produced were less than 100 μm, while the observed ice particles in both cases are very large with sizes generally larger than 200 μm based on both the 2DS and CIP images (Fig. 2). Since new ice crystals form on tiny ice nuclei with at the dust sizes when activating in the deposition mode, so they are put into the first bin of the ice crystal spectrum which is about 4-5 μm in diameter, and thus a small ice crystal mode persists in the simulated clouds. For this reason, and the fact that the Meyers parameterization for deposition nucleation does not well represent the activation properties of mineral dust particles, simulations did not give results in good agreement with observed cloud properties. With our implementation of the immersion freezing (i.e., DeMott et al. 2013) as described above (i.e., the largest drops freeze first and then the smaller ones), we are able to obtain the majority of large ice particles in our simulations, matching better with observations"

• **result analysis**

Figure 8 and the related analysis needs improvement. If I am correct, the figure shows the mass mixing ratio (according to the axes label) of rain and snow in the lowest model layer summed over domain 2 and the day. Since a terrain following coordinate is used and the domain includes altitudes from sea surface to mountain peaks (and therefore a varying air density), the sum over the mass mixing ratio (kg rain per kg air) in the lowest model layer is not a good measure. At least the sum of the number densities (kgm□3) should be used in this case.

BUT, I do not understand why you do not use the total precipitation of rain and snow at the ground (kgm□2) for this analysis.

- The mixing ratio in kg kg^{-1} has been transformed to mass concentration in kg m^{-3} in Figure 8 as suggested. The results have been described accordingly in the initial two paragraphs of Section 4.2 (most of the results are the same except that rain mass concentrations are 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 ^oC for snow and T > 0 ^oC for rain at the surface (we tried that anyway but got confusing results).

Minor comments:

• 19926 l. 27

where mineral dust/biological particles were Please avoid the use of "/" in the text. This is used multiple times in different ways: the mineral dust/biological layers, dust/bio, droplets/drops, Central Valley/foothills, ...

Please refer to INP or only mineral dust instead of using dust/bio for the discussion of the simulation results. Because only dust INP are used in the simulations.

- Creamean et al. (2013) analyzed measurement data from the same case study of FEB16 used in our study and found both dust and biological particles. Since the CFDC measurements do not include composition information, the parameterization of DeMott et al. 2013 assumes all aerosol particles larger than 0.5 microns have mineral dust-like nucleating properties. Therefore, we added the following statements in Section 5, "In this study, we do not distinguish the actual composition of the aerosols that are effective INP to form ice, but evidence from the cloud and precipitation residues suggest the presence of mineral dust and biological particles on Feb 16 transported aloft from Asia/Sahara (Creamean et al. 2013) and dust/biological aerosols on Mar 02, which we assumed to have similar ice nucleating properties as mineral dust". In the beginning of discussion aerosol effects (Section 4.2), we also stated that "By removing the aerosol layer which has ice nucleating properties of mineral dust from the base run (LoCCN&NoDust)…". It is important that we mention both dust and biological particles because we cannot attribute the INP to purely dust or biological particles.
- We have removed "/" in connecting two words in the revised manuscript.

• 19927 l. 6

What do you mean by microphysics data (with Aerosol and cloud microphysics data already mentioned before)?

- We mean liquid water content, ice water content, hydrometeor number concentrations and size distributions as described in the following paragraph.

• 19927 l. 24-27

I do not understand how lower-level convective clouds can be decoupled from the boundary layer.

- Removed “lower-level” in that sentence.

• 19929 l. 6

Sea salt aerosol might be also an important source of large particles in this regions.

- We agree but sea salt generally resides in the boundary layer. The assumption is for ice nucleating particles, i.e., the dust layer which is above 3 km for both cases.

• 19931 l. 5

I assume you mean cloud droplet nucleation/ aerosol activation and not nucleation of new aerosol particles/CCN, please be more specific.

- Clarified as “droplet nucleation”.

• 19933 l. 5

set according to the base run of FEB16, which is described in the next section.

- Changed as suggested.

• 19933 l. 7-8

change microns to _m

- Changed as suggested.

• 19933 l. 16-25

What is used for the fine-domain in FEB16 as initial and boundary data? I do not get the difference you mentioned between the FEB16 and MAR02 setup.

- We have clarified the description of the initial and boundary data for domain 2 in FEB16. Now the text reads as “For FEB16, 3-hourly NCEP North American Model (NAM) Data at 32 km resolution is processed to provide initial conditions for both Domain 1 and 2, and also provide lateral boundary conditions for Domain 1. Domain 1 and 2 are run at the same time with a one-way nesting approach”.
- As 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 used NAM for FEB16 and NARR for Mar02. 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.

• 19934 l. 9

period of Flt0206 were

- Changed.

• 19934 l. 13-17

According to your introduction you have multiple airborne aerosol measurements available. Why didn’t you use them for the number concentrations and the profiles of the CCN?

- There were no measurements of CCN available for these cases. The aerosol measurements are UHSAS and PCASP, which were used for determine CCN size spectrum. But the total CCN number concentration was adjusted based on CDP-measured cloud droplet number since we do not have CCN measurements. Those are described in that paragraph.

• 19934 l. 23
As mentioned above sea salt aerosol might also contribute significantly to particles above 0.5_m in this area. (Nevertheless the derived dust concentrations fits to the measured INP when applied.)

- See our response above.

• 19938 l. 2
The frequency of large Vd and Zr are maybe also underestimated because of the limited model resolution in contrast to the radar measurements.

- The S-PROF is a vertically-pointing radar of a single point measurement. Although the measurement frequency is high (in every minute), we sampled every 10 min to match the model data for the comparison (described in that paragraph). Although the model results are averaged over the four adjacent grid points at the location of S-PROF, the results are not changed much using values at a single point. Yes, it is possible that higher model resolution could improve the simulation results. This has been added to the text.

• 19938 l. 8
presents the mass mixing ratio of rain and snow at the lowest model

- We have changed to "presents the total rain and snow mass concentrations at the lowest model level" since the mixing ratio (kg/kg) is changed to the mass concentration (kg/m-3) as suggested above.

• 19938 l. 8
Is 40 m the middle of the layer or its boundary? In the figure it says 50 m.
- It is the boundary. We have changed to 50 m consistently.

• 19938 l. 18
raindrop mass mixing ratio

- Again, we have changed to "raindrop mass concentration".

• 19939 l. 4
What is the width of the strip (one gridcell?)? What you describe is the average precipitation in kg m□2 along the strip, or? (The description in the figure caption is also confusing mean values .. integrated over a strip) The varying length of the strip might also affect the analysis.

- Yes. The width of the stripe is one grid cell. We have modified the figure caption to clarify all of your questions here, i.e., "Differences of the accumulated precipitation averaged over the grid points along a strip one grid cell wide and parallel to the blue line in the panel (a) along the cross section".

• **19939 l. 19**
as mentioned above, please refer only to dust particles in the discussion of the simulation results.

- Please see our response above.

• **19940 l. 9**
But the near-surface rain is increased by a few times I think the surface precipitation (rain+snow?!) averaged over the strip is not directly comparable with the domain average near surface rain. Please skip the But and be more specific what kind of averaged/summed values you are referring to.

- The sentence was deleted as suggested, and we are more specific now by stating "increases the accumulated surface precipitation averaged over the grid points along the strips shown in Figure 11a".

• **19941 l. 6**
by CCN, change to by an increase in CCN

- We have changed "by CCN' to "by the increased CCN" throughout the paper.

• **19942 l. 8**
raindrop and snow mass mixing ratios

- It is mass concentrations now (in kg/m^3).

• **19943 l. 20**
westerly winds or westerly

- Changed to "westerly winds"

• **19945 l. 8**
better: mainly resulted from an increased snow mass mixing ratio
- Changed as suggested.

• **19945 l. 2**
allows more droplets to feed the ice generation regime of the orographic clouds and available for riming to increase snowfall ... restructure the sentence. I am not a native speaker, but there are several passages in the text, which appear strange to me. I recommend that a native speaker proofreads the manuscript.

- We have reworded the sentence as "allows more droplets to feed the ice generation regime of the orographic clouds, increasing snow through more riming". A few of the coauthors are native speakers and have read the paper.

• **19945 l. 21-23**
restructure: Since the winter mixed-phase clouds simulated herein do net reach the homogeneous freezing level their is no mecha...... in the simulations without dust.

- Restructured as suggested.

• **19945 l. 25-26**
The INP influence mixed-phase clouds mainly through riming and the WBF process. Be more specific.

- We have been more specific by changing the sentence as "The INP influence mixed-phase clouds mainly through the WBF process as ice deposition growth is about 20 times larger than riming growth".

• **19946 l. 20-21**
is calculated after immersion freezing in our model which significantly reduces the INP available to the contact freezing Is this really the case? In page 19932 l. 15 you mention that the activated fraction due to immersion freezing is only a few percent or less.

- That was the case when we used other ice nucleation parameterizations in the early stage of this study. But with the parameterization of DeMott et al. 2013, it should not be the case as the frozen fraction is low (~0.03) even at -30 C. Thanks for catching it. The statement about "immersion freezing significantly reduces the INP available to the contact freezing" has been removed.

• **References**
Please check again the references. E.g. Fan et al. 2009a appears in the text but not in the references.

- Fan et al. 2009a has been changed to (Fan et al. 2009).

• **Table 1**
The CCN and INP setup for the simulations

- As suggested, "set up" has been changed to "setup".

• **Table 2**
Please specify how you calculated the average in the caption, because the concentrations are really low. Are all grid points included in the average? Or only grid points containing cloud hydrometeors above a specific threshold?
Try to avoid footnotes in the table. Use consistent abbreviations: LoCCN&Dust instead of Base.

- The concentrations are low because they are averaged over all grid points in Domain 2 below 7 km and over a whole day. We have further clarified this in the figure caption. Also the footnotes have been removed and added to the figure caption. Now the caption reads as "Cloud microphysical properties from the simulations of (a) FEB16 and (b) MAR02. Data shown are averaged over all of grid points in Domain 2 below 7 km during the day (0-23:00 UTC). The CCN effect is calculated by (HiCCN&Dust-LoCCN&Dust)/LoCCN&Dust *100, and the dust effect is calculated by (LoCCNNoDust-LoCCN&Dust)/LoCCN&Dust *100".

• **Figure 5**

Enlarge axes labels or entire figure.

- Done.

• **Figure 6**
Please add axes labels (degree North, ...). This holds also for Fig. 4,9,10,13, and 14 but is especially important here, since no coastlines etc. are included in the figure.

- Added.

• **Figure 7**
The line colors (brightness) in the legend do not fit to the data curves.

- Removed the simple figure legend and noted the colors in the figure caption.

• **Figure 8**
mass mixing ratios. See major comments.

- Changed to mass concentrations.

• **Figure 9 - 10**
Mixing ratios. Enlarge the color bars.

- Changed as suggested.

• **Figure 11** The figure must be strongly enlarged in the final version (I know the limitations of the discussion format). If I got it right it must be Differences of the accumulated precipitation averaged over a strip parallel to the blue line in the panel. Specify the width of the strip. In the axis label only rain is mentioned. Please change if it is actually all precipitation (rain+snow+...).

- Figure 11 is enlarged. Figure caption has been changed as suggested in a comment above.

• **Figure 7**
The line colors (brightness) in the legend do not fit to the data curves.

- This is a repeated comment as above.

• **Figure 13 -14**
Please improve the figures: reference wind vector is missing, arrow density is too high, arrows should not cross the boundaries,...

- Figures are improved as suggested. Arrows cross the boundaries because of high wind speeds of the grid points near the boundaries.