Response to Reviewer #1

Fan et al. report on an aerosol-cloud-precipitation process modeling study regarding two cases from CalWater 2011. The advantage of this work over FAN2014 is based on the comparison of variable cloud phase conditions (WMOC versus CMOC), providing an added level of detail. One of the more surprising findings is the increase in snow precipitation when CCN concentrations are high in the CMOC case through changes in local circulation, due to invigoration of mixed-phase clouds from latent heat release. Although the results from this study are interesting and worthy of placement in the literature, there are a few issues that need to be resolved prior to publication in ACP.

Although containing pertinent information, the introduction is somewhat difficult to follow. I suggest reordering and refocusing the introduction such that there are four paragraphs to guide the reader in a more efficient manner: 1. An abridged, broad background on aerosol-cloud-precipitation interactions, cloud phase, etc.

- Thanks for the helpful suggestions to improve the paper. Please see our point-by-point responses as below.

General comments:

Some of this information is already provided in the beginning of the introduction. Much of the information in the paragraph starting on p 5, 73 could be placed in the first paragraph. 2. Introduce the concept behind CalWater and briefly describe previous relevant results, including the main findings from Ault et al. (2011), Creamean et al. (2013, 2014, 2015), White et al. (2015), Rosenfeld et al. (2013, 2014), and of course FAN2014. 3. Discuss what is missing from those previous works, as motivation for the current study. For instance, has anything been previously done regarding WMOC versus CMOS simulations? This seems to be a new approach that could be emphasized. 4. Clearly list the objectives for the current study and what is novel about it. The information on p23, 492-494 would be suitable for the list of objectives. Further, the authors state this is a follow up on FAN2014, but should specifically discuss what is new and why this is an improvement versus serving only as an extension (i.e., the information on p 9, 168-172 and p 23, 489-492 is an improvement that should be mentioned in the introduction).

Creamean, J. M., Lee, C., Hill, T. C., Ault, A. P., DeMott, P. J., White, A. B., Ralph, F. M., and Prather, K. A.: Chemical properties of insoluble precipitation residue particles, J Aerosol Sci, 76, 13-27, 2014.

Creamean, J. M., Ault, A. P., White, A. B., Neiman, P. J., Ralph, F. M., Minnis, P., and Prather, K. A.: Impact of interannual variations in sources of insoluble aerosol species on orographic precipitation over California's central Sierra Nevada, Atmos Chem Phys, 15, 6535-6548, 2015.

Rosenfeld, D., Chemke, R., Prather, K., Suski, K., Comstock, J. M., Schmid, B., Tomlinson, J., and Jonsson, H.: Polluting of winter convective clouds upon transition *from ocean inland over central California: Contrasting case studies, Atmos Res, 135, 112-127, 2014.*

White, A. B., Neiman, P. J., Creamean, J. M., Coleman, T., Ralph, F. M., and Prather, K. A.: The Impacts of California's San Francisco Bay Area Gap on Precipitation Observed in the Sierra Nevada during HMT and CalWater, J Hydrometeorol, 16, 1048-1069, 2015.

- The Introduction generally follows the line that the reviewer suggested but starts with a general background about California precipitation and cloud properties that this study focuses on. Then the factors – AR and aerosols that impact cloud properties and precipitation are introduced in the second paragraph. The third and fourth paragraphs basically follow the second one to give a more detailed literature survey about aerosol impact on orographic clouds and supercooled water. After that, we discuss what is missing from those previous studies, and introduce FAN2014, and state the objectives of this study following FAN2014. All authors are in agreement that the Introduction of this paper is organized in a logical fashion to introduce the topic and goals of this study.
– We have included the references that the reviewer suggested except White et al. (2015), which we think is not much related (see L75-77 and L90). We have also slightly modified the text to more specifically discuss what is missing from the previous studies and what is new in this study (i.e., the text at L100-102, L122-123, and L129-130).
- The text on p23, L492-494 in the original manuscript was already stated in our objectives #2 and #3 (i.e., the current L147-150), and the information on p 9, L168-172 was also already included in the introduction (i.e., the last sentence of the Section 1).

Even though the conditions for each case are described in FAN2014, they could be reiterated here. Some characteristics are presented on p 10, 194-199, but what were the average cloud top and base heights? What was the frequency of occurrence for each cloud phase type and were the particular days chosen extremes? On p 10, 192-193, I am assuming these averages for the case days only, but it would be interesting to provide information on if these are conditions that were anomalous or typical of this region. Additionally, the description of the cases on p 23 494-497 would be better suited earlier on when describing the cases.

- The averaged cloud top height for each cloud case is described on P5-6 when the cases are introduced for the first time. Now we have reiterated here (p10 L209-211). The cloud base information has been added as well (L212-213). Those two cases correspond to anomalous conditions as they are influenced by both AR and long-range transported dust/bio (L214). The description of the cases later on p 23 494-497 (the original version) is just a short version of the description here but with a little different wordings. Now we have added the same wordings (L217-218).

While a wide range of information is yielded from this more elaborate study, it is somewhat difficult to follow due to the nature in which the results are presented. As an example, the results quickly transition to comparing the CMOC to the WMOC case even before the basic results from the WMOC case are presented (p 17, l 356-366). I recommend reordering section 3 such that the CMOC results are presented first (section 3.1, without the subsections), WMOC second (section 3.2), followed by comparison of the microphysical changes from each case (i.e., section 3.1.3), and lastly a comparison on the disparate effects on precipitation from each case (i.e., section 3.1.2). Another option would be to condense and fold the comparison of the cases in terms of microphysical and precipitation effect differences in the discussion and conclusions. The authors could still focus on the CMOC case since it affords surprising results, but should be bolstered in the discussion. As a result, the figures would need to be restructured such that they are easier on the eye and align with the recommended reordering of section 3. For instance, Fig. 2 could instead be a combination of the current Fig. 2 and Fig. 3 panels, and Fig. 3 could be a combination of the current Fig. 4 and Fig. 5 panels for CMOC. The subsequent new figures (4 and 5) would then be the same structure, but for the WMOC case. The current Fig. 11 should be introduced with the WMOC case section (3.2). The current Figs. 6, 7, 8, 9, 10, and 12 would be pushed back to when the microphysical and precipitation accumulation differences are discussed. If restructured such that the results are reordered to enable better flow, the novelty of the work will be more apparent to the reader.

- Section 3 is already organized in the way that the reviewer suggests: CMOC is discussed first (Section 3.1) and then WMOC (Section 3.2). Within each case, we discuss the basic results first and then look into the mechanisms. We do not quite understand the reviewer's comment "the results quickly transition to comparing the CMOC to the WMOC case even before the basic results from the WMOC case are presented". The place that the reviewer pointed out (p 17, L356-366 in the previous version) only contains one sentence that mentions a sensitivity test done based on WMOC to confirm a factor in the mechanism we presented, but that is after all the basic results and the mechanisms for the CMOC have been presented and discussed. We discussed that sensitivity test here in order to give a whole picture of the mechanism at the same place. The WMOC is chosen for this test because of less mixed-phase regime compared with CMOC, so the factor would have a more significant role in the CMOC if it plays a role in the WMOC. This has been clarified further on P17 L377-381 in the current manuscript. The subsection titles are useful for the readers to follow the result section clearly.
- The sequence of the figures is also already presently constructed around the logical discussion of research findings: starting from the significant results/concerns in precipitation (Fig 2), then looking into how they are related to cloud microphysical properties (Fig 3), which are determined by major microphysical process rates (i.e., budgets; Fig 4, and 5.). After that, then we present the physical mechanisms leading to the significant changes (Fig. 6-Fig. 10). So, they are separately presented in their natural ways, e.g., Fig.2 is about precipitation and Fig. 3 is about cloud microphysical properties. Besides, each of the figures has multiple panels already. We prefer not to combine the figures as the reviewer suggested.
- Moving Figures 6, 7, 8, 9, 10, and 12 to the end does not align with the flow of the presentation in the paper, as these follow the discussion logically. In addition, it is quite common to introduce figures with comparative results prior to completing their discussion (i.e., they are referred back to), and this goes along naturally with the manner of discussing CMOC first and WMOC second.

Publishing the new findings is key. To emphasize that this study entails new findings and is not a just a slight modification of FAN2014, the authors should consider providing specific statements as to how and why the results here vary from FAN2014 throughout the results section.

- All the results discussed in the result section are new from FAN2014. We do not think that it is necessary to say it throughout the results section. The only thing that we can compare is that the significant CCN impacts on precipitation was not seen in FAN2014, which was simply because we only increased CCN by 3 times based on the baseline cases in FAN2014, making CCN concentrations of ~ 160 and 720 cm^{-3} in the high CCN cases for CMOC and WMOC, respectively. They are smaller than 1000 cm^{-3} where the significant effect is seen in this study. In addition, CCN and IN are set to be uniform and increased uniformly over the domain, while in FAN2014, only CCN over the central valley and coastal urban area were increased. This discussion has been added to the last section (P25,L551-556).

Along these lines, the fact that snow increases with increasing CCN is surprising. The authors present some comparison with previous work (i.e., Saleeby et al. (2011)) and what key differences may have led to the disparities between the studies. First, this should be done throughout the discussion: are the results (besides this one) surprising or expected in the context of previous work? Second, what other studies contradict this finding and why? The authors state that this result, "...is different from previous modeling studies in the literature..." but which studies specifically and for what reasons?

- Such comparisons with literature studies were already presented in the Discussion section in two places (starting from L572 on P25 and the last paragraph of the paper). For the sentences that the reviewer pointed out, we have provided the possible reasons and modified that text as "different from previous modeling studies in the literature such as Lowenthal et al. (2011). Many possible reasons could lead to the differences including different cloud cases and different model parameterizations especially for riming processes" (P26 L575-577). In addition, we have provided more detailed discussion by comparing with other previous studies brought up by another reviewer as shown in L583-596.

The authors do show the spatial heterogeneity in several resulting parameters in a couple figures, but are the main conclusions based upon the results time-dependent as well? For instance, CCN increasing snowfall, is that after (X) hours of simulation? Does this occur immediately? Or is this an average over the entire simulation time period, which could be highly variable over time? The authors could consider showing a figure of key parameters over time, which would be interesting.

- Yes, time evolution is important to look at the mechanism responsible for the changes. But we already considered this information and discussed when the precipitation (or snow) enhancement starts, and then looked at the related variables at the start time and how they evolve in the subsequent 1-2 hours as shown in Fig. 6-10. The corresponding text to discuss this starts from "Since the precipitation enhancement begins at 1400 UTC, which is a couple of hours into the simulations, we focus on the time period of 14-1600 UTC" (L342-344 in the current manuscript).

It is not initially clear that the simulation parameters, namely CCN and INP concentrations, chosen are of realistic values to what is observed in the Sierra Nevada or if these are idealized situations. It is not until much later in the conclusions and discussion section that the authors mention CCN of > 1000 cm^{-3} is considered an extreme for this region (p 26 l 554-555). This should be clearly delineated much earlier, in the methods. Also, what is "normal" versus extreme for the INP concentrations at the temperatures observed for each case?

- We agree that it is useful to further frame the INP concentration range used and have added Table 2 and text to the present discussion of how dust/bio particle concentrations relate to INP concentrations as a function of temperature. This discussion has been added on P9 L196-205. The extreme conditions for CCN and INP were mentioned when the simulation setup was introduced (now L190-191).

There are several typos and grammatical mistakes throughout the manuscript, which the authors should take care in correcting for the revision. Some examples include: (1) "INP" is used in several instances where the plural form should be used (INPs), (2) CCN are plural but are commonly referred to as a singular, and (3) "Mountains" is typically capitalized mid-sentence. Also, please write in past tense when describing the results from the simulations.

- We have carefully checked these places and corrected the typos and grammatical mistakes throughout the paper.

Abstract: It is not apparent that the comparison of the WMOC and CMOC case are conducted under the same INP and CCN concentrations. Please clarify.

- We have modified a sentence in Abstract to clearly say it, i.e., to "We quantify the CCN and INP impacts on supercooled water content, cloud phases and precipitation for a WMOC and a CMOC case with sensitivity tests using the same CCN and INP concentrations between the WMOC and CMOC"

Specific comments:

P 2, l 28: Please clarify the type of deposition (i.e., in-cloud nucleation, in-cloud scavenging, etc.).

- By deposition we are referring to the depositional ice growth process.

P 2, l 30: "...WMOC *with* low INP *concentrations*." Also provide the INP concentration used here for reference.

-As we have clarified in the current version, INPs are dependent of temperature besides dust/bio concentrations. So, it is not just one value that can be put there. .

P 2, l 30-31: Remove the sentence starting with "However" as this is redundant to the following sentence, which is better because it provides more detail. Once removed, the following sentence can be started with "*However,* we find a new mechanism..."

- Since the new mechanism is to explain the sentence “this reverses strongly for CCN > 1000 cm^{-3}”, we think putting the word “however” before this sentence better conveys what we want to say here.

P 2, l 33: “...concentrations are > 1000 cm^{-3}.”

- We have revised it as “for CCN of 1000 cm^{-3} and larger”.

P 2, l 34: Please clarify that this is the Central Valley and foothills west of the range.

- Done.

P 2, l 33-37: There is quite a bit of information presented in this one sentence, making it appear as a run-on. The authors should consider breaking up into two sentences.

- We have re-constructed the sentences by breaking into short pieces. And now it reads as “In this situation, more widespread shallow clouds with greater amount of cloud water form in the Central Valley and foothills west of the mountain range. The increased latent heat release associated with the formation of these clouds strengthens the local transport of moisture to the windward slope, invigorating mixed-phase clouds over the mountains, and thereby producing higher amounts of snow precipitation.” (P2 L34-38).

P 2, l 37: The beginning of this sentence is vague. What concentration of INPs? With what concentration of CCN? Some more context is needed.

- This is a generalized summary of details that can only be understood through reading the paper. We have added “under all CCN conditions” to the sentence.

P 2, l 39: “However, *an increase in precipitation occurs* in both cases...”

- Changed.

P 4, l 51: The Ralph et al. article on CalWater would be a great citation for this statement.

Ralph, F. M., Prather, K. A., Cayan, D., Spackman, J. R., DeMott, P., Dettinger, M., Fairall, C., Leung, R., Rosenfeld, D., Rutledge, S., Waliser, D., White, A. B., Cordeira, J., Martin, A., Helly, J., and Intrieri, J.: Calwater Field Studies Designed to Quantify the Roles of Atmospheric Rivers and Aerosols in Modulating Us West Coast Precipitation in a Changing Climate, B Am Meteorol Soc, 97, 1209-1228, 2016.

- Added.

P 4, l 51-52: This sentence is redundant to that below, could simply remove.

- Removed.

P 4, l 54: Please clarify that this is over the Sierra Nevada mountains.

- Done.

P 4, l 57: Cloud *phase* (should be singular). Please correct here and throughout.

- Since cloud has different phases (liquid, mixed, and ice phases) and it has been used as plural commonly as well.

P 4, l 65: Remove "in the atmosphere".

- Done.

P 5, l 73: Be more specific by clarifying that these are aerosol *climate* impacts that depend on aerosol properties *such as number, size, and composition*.

- Added "such as number, size, and composition".

Table 1 does not seem necessary. The information on the concentrations used are already provided in the text.

- Table 1 more clearly and intuitively shows what kind of simulations we have conducted for this study than text.

All figures: Why are two markers (circles) listed in the legend for INPs?

- We listed two markers to show the legend more clearly.

Fig. 2: Please place the panels in the order in which they are discussed in the text. Also, provide what the arrows are in the caption for clarity.

- Fig. 2b and 2c follow the conventional presentation on warm and ice precipitation respectively. To be consistent with Fig.14 that is the same type of figure, we would like to keep it this way. This is allowed in scientific papers (i.e., Fig. 2c is referred before Fig. 2b). We have added the description of the two arrows to the Figure caption now.

Fig. 6: Why are there no ice nucleation rates for levels where nucleated ice particles were found?

- Fig. 6c is in logarithm scale and the nucleation rate is too low below 2.5 km altitude, so it is not shown.

Figs. 8 and 9: Why is this only shown for CMOC and not WMOC? I get that the CMOC case presents interesting results, so at the very least, the authors could provide the WMOC spatial figures in a supporting document and allude to them in the text.

- Fig. 8-9 are parts of the figures illustrating the mechanism leading to the drastic CCN impacts on precipitation in the CMOC. Since very similar mechanisms are seen in the WMOC, there is no need to present similar figures but in the text we clearly discuss the similarity starting from L490 on P20, "We have done the same investigation as in Section 3.1.1, and found the mechanism causing the increased cloud water and the snow production is similar as that in CMOC, that is, …". Furthermore, the key part of the mechanism for the WMOC is shown in Fig. 10b, which is the change of local circulation that increases the zonal transport of moisture to the windward slope of the mountains.

Fig. 9: It would be easier on the eye if a color scale much different than the previous figure were used, since these are differences and not absolute values. Perhaps red to white to blue?

- Right now it is red to green to blue, not much different from what the reviewer suggested (i.e., red to white to blue), and we feel that it shows the positive and negative data clearly.