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.

General comments:

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. Some of this information is already provided in the beginning of the introduction. Much of the information in the paragraph starting on p 5, l 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 p 23, l 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, l 168-172 and p 23, l 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.

Even though the conditions for each case are described in FAN2014, they could be reiterated here. Some characteristics are presented on p 10, l 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, l 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 l 494-497 would be better suited earlier on when describing the cases.

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.

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.

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?

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.

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?

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.

Specific comments:

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.

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

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

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…”

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

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

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.

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.

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

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.

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

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

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

P 4, l 65: Remove “in the atmosphere”.

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*.

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

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

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

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. 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?