Second Review of:
"Impacts of ice-nucleating particles from marine aerosols on mixed-phase orographic clouds during 2015 ACAPEX field campaign"

General comment:

While some aspects of the manuscript have been improved, the authors need to better address my past comments that have been restated below. There are several places where the impacts of marine INP make the simulation worse, yet the authors gloss over these differences. Further, there are places where the marine INP simulation is comparably indifferent with respect to the "control" simulations, and these effects are exaggerated. The most dominant marine INP impact appears in the spillover of precipitation and the glaciation ratio. The authors need to give a more fair assessment of the simulations and the impacts of marine INP. I feel that it is important not to overstate tiny changes and also not to understate situations in which the marine INP cause more disagreement with the observations. A fair and balanced objective assessment is needed at all times.

We thank the reviewer for the further comments. We fully agree with fair assessments of the simulations and the impacts of marine INP and we have attempted to achieve that by examining every conclusion based on the data. From your specific comments below, we would like to bring up a few remarks about the limitations of the observations and evaluation and how marine INP effects are examined in our study so that we are on the same page:

- The poor performance of the model occurs only at the post-AR stage when cloud cells are small and very challenging to simulate, but the post-AR stage is not our focus as we noted in the paper. Our conclusions about marine INP effects are not derived from this stage. This is also one of the reasons we examined marine INP effects by separating AR into different stages.
- The evaluation of the clouds at the post-AR stage with the aircraft data is only for a single cell, which might not be generalized to the clouds in other locations of the domain. Therefore, we limit the discussion of the post-AR stage to avoid distraction from the main focus of the study. To address your concerns, in the revised version we removed the conclusion that adding marine INPs improves cloud simulation of phase state from the abstract, and we have added discussion of the implication of the overestimated supercooled liquid to the modeled marine INP effects.
- Our marine INP effects are derived from the model sensitivity tests. In other words, the effects are for the modeled clouds only. We do not have any claim that it is the observed marine INP effects. We do not have the observations to evaluate if the modeled marine INP effects are similar to the observed effects or not. This has been emphasized in the last section in the revised manuscript and we have also changed the title of the paper to "Modeling impacts of ice-nucleating particles from marine aerosols on mixed-phase orographic clouds during 2015 ACAPEX field campaign".

Please see our embedded point-by-point responses below.

Specific comments:

1. Figure 3: I stated in my first review that your wide logscale for aerosol profile comparisons is visually minimizing the differences between the observed aerosol concentrations and the simulated concentrations. You have now stated in the text that there's a 4x higher value for simulated aerosol concentration at 2.8km. However, you do not discuss the implications involved in running simulations with much higher aerosol numbers than observed. This will inherently over emphasize the aerosol INP effects compared to the observations.

- We thank the reviewer for emphasizing the need to discuss the model limitations and implications for modeling INP effects. First, we would argue in aerosol simulations, the number concentrations are difficult to simulate since major processes influencing aerosol number are not well understood such as aerosol nucleation. Four times higher is not something surprising. Second, we want to note that the 4 times difference is for the total aerosol concentration which is not used in our model parameterization for the effects of dust and marine INPs. The number concentrations of dust and marine SSA used in model parameterization are not predicted so they are derived from mass concentrations but there are no mass observations for a single aerosol component. Therefore, we cannot infer that marine INP effects are overestimated based on the 4 times difference in total aerosol concentration. Furthermore, this evaluation is only from the aircraft measurements at the post-AR stage, which is not our focus of the study. We do not know how well the aerosols are simulated before AR and after AR landfall. The lack of relevant observations for robust evaluation of model simulation was discussed in the last section, and we further revised it in this version and now it reads as "More observational data particularly on the extended spatial and temporal coverage are needed in the western U.S. for (a) evaluating model simulations more robustly, (b) developing ice nucleation parameterizations for potentially variable marine organics and (c) understanding marine organics emission and chemical mechanisms and accurately simulating marine organics in the model" (Line 662-664).

2. Figure 4 discussion and Lines 367-368: In my previous review I requested the inclusion of difference plots of accumulated precipitation so that we can better visualize how the precipitation changed, particularly over the white box area. Without a difference plot it is nearly impossible to discern the differences as pictured. Please provide difference plots that can be added to figure 4. You could plot one as (DM15 - Bigg) and (DM15+MC18 – Bigg). These 2 new panels would help us see the differences more clearly.

- In the previous round, we hesitated to add more panels since the results were clear to us. Now we have added the difference plots between Bigg and DM15 and between DM15+MC18 and DM15 as the second row (using DM15 as a reference to be consistent with the marine INP effects that we focused on). The conclusions are the same. The corresponding description of the plots includes "The simulated precipitation between Bigg and DM15 is very similar except for more precipitation in Bigg in the northern part of the domain (Fig. 4a-b)" (Line 370-371) and "There is a clear spillover effect caused by marine INPs (Fig. 4a-b, right). That is, with marine INPs considered in DM15+MC18, there is a notable decrease in accumulated precipitation (~ 30-50 mm) on the windward side but a large increase (~ 50-70 cm) on the lee side (Figure 4b, right)" (Line 373-376).

3. Line 388: It seems here you are simply disregarding the differences in cell heights and just moving forward? The cell depth is quite important given the need for cold temperatures for ice nucleation and the necessary depth for ice growth. You need to discuss the limitations in interpreting results if your simulation cannot reproduce the depths of the cells.

- Since we have noted that the post-AR cell produced negligible precipitation and it is not the focus of the study (Line 440-442, and 500-502), we indeed do not want to use a lot of text on this to distract our focus. Also, this evaluation is just for one single post-AR cell after 3-4 days into the simulation. We do not know if it can be generalized to all postfrontal clouds. Therefore, besides emphasizing the modeled INP effects might not be the same as what occurred in reality, we do not have a direction for discussing what this means to marine INP effect but we have tried to add discussion about overestimated supercooled LWC. We have added the emphasis and discussion in two places: section 4.3 "The effects of marine INPs on the postfrontal clouds might differ from the reality since based on very limited measurement data, the model seems not be able to capture those clouds well. The overestimated supercooled LWC can allow for more riming growth which may lead to a larger sensitivity to marine INPs." (Line 549-552), and the conclusion section "Since our model may not simulate clouds well at the post AR stage based on very limited measurement data, we emphasize that the large responses to marine INPs simulated at this stage might not reflect the effect in reality" (Line 633-635)
- Note the we can not say if the cell height is reproduced or not. Based on the reflectivity shown in Fig. 5 which speaks about precipitating particles, the modeled cell has a shallow precipitating core, but we do not know if the cloud top height is lower than observations or not. Note sure where the reviewer got information about the cell height.

4. Lines 413-414: You state that inclusion of marine INP improves the simulations of cloud phase states. As I discussed in my past review, the glaciation ratio is better, but the simulated amount of LWC and IWC, which is critical for accumulated precipitation, is the worst of the simulations. So, I disagree that the DM15+MC18 simulation is better. While it's crucial for a model to simulate both the total condensate and the water/ice ratio, I would argue that getting the total condensate amount is most important when the bottom line is the improvement of predicting accumulated precipitation and its spatial distribution.

- We acknowledge the reviewer's point of providing a fair assessment of model skill. Therefore our statement only emphasizes the glaciation ratio, which is improved drastically from 0.2 in DM15 to 0.70 in DM15+MC18; the observed value of 0.74 so the sentence "The inclusion of the marine INP effect improves the simulation of cloud phase states via enhancing heterogeneous ice formation through immersion freezing" is accurate. About the overestimation of LWC and IWC, this was clearly described in the text right above the statement regarding glaciation ratio. In the current version, we have also added discussion about the implication of high LWC in marine INP effect in section 4.3 (see our response to #3).

5. Lines 435-436: Your sentence in response to my past comment on figure 7a states: "Note that precipitation is very small at some point before AR landfall, so the large increases might not mean that much." This response is not adequate and appears a bit out of place here. I

think the discussion of figure 7a needs to be fairly addressed. The changes in precipitation rate are quite small over all AR stages, but the greatest difference is in "Before landfall". However, the largest difference is about 0.1mm/hr. Again, the % change is largely irrelevant if the initial magnitude of precipitation rate is very small. The current discussion overemphasizes the impact of marine INP in this assessment.

- What the reviewer described for Figure 7a is accurate and we believe that is what we have in the text written based on the data shown in Figure 7a and Table 1 for the three different stages. About the small precipitation at the stages before AR landfall and post-AR, we clearly noted this, i.e., "Note that precipitation is very small at some point before AR landfall, so the large increases might not mean that much. The total precipitation at the post-AR stage is negligible and the change in domain-mean precipitation from DM15 and DM15+MC18 is also small". For after AR landfall, we clearly stated "There is only a 4% increase in the total precipitation after AR landfall ", and "After AR landfall, precipitation increases significantly. Although the total precipitation is not changed much by the marine INPs, the marine INPs produce a spillover effect featuring reduced precipitation on the windward slope of the mountains but increase precipitation over the lee side (Fig. 8b and Fig. 9e)." We believe those sentences clearly describe the impact of marine INP, which varies at different stages as we noted, and they are in the right place and narrative flow.
- In addition, we do not agree with the reviewer's opinion "the % change is largely irrelevant if the initial magnitude of precipitation rate is very small". We already clearly stated that the small magnitude of precipitation rate results in some large values in the % change (Line 439-440). But the % change does help us quantify how the light precipitation can be influenced by marine INP. Let's explain by an example, the rain rate is 0.05 mm/h averaged over all domain grids (about 400*500 grids) over one hour 00-01 Feb 6 and increases to 0.1 mm/h by marine INP. This means that the accumulated rain over the domain increases from 10 cm to 20 cm within that hour, which is a significant change for light rain amount and in fact it means more to agriculture than heavy rain. Providing the information of the rain rate in the absolute value and % change is useful for readers to judge the significance.

6. Figure 8: Again, please show a difference plot (perhaps as a 3rd column) so we can better visualize the spillover effect. This is very important to emphasize, particularly since the changes in total precip and precip rate are quite small.

- We have considered your suggestion carefully but we think the spillover effect is clearly represented in several places so decided that the additional panel is not needed. First, here in Figure 8, the spillover effect is highlighted with the red rectangle box and it is very clear (drastically different colors). Second, the spillover effect is shown again with Figure 9e which is crystal clear. Third, the newly added difference plot in Fig. 4b (right) also very clearly shows it. Adding another column to Figure 8 is not necessary given there are three figures showing the effect very clearly, and it would just make the spatial map panels too small to be seen clearly.

7. Line 564: Please refrain from using "invigorated" to refer to an increase in condensate. This term is typically referring to dynamics and strength of a system, particularly vertical velocity.

- We actually defined “invigorated cloud cell” as the increase in both LWC and IWC in the paper. To avoid confusion with dynamical changes, we have removed the term in the revised manuscript.