Response to Reviewer #1:

Review for:

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

by Lin et al.

This study examines the impacts of marine INPs on orographic clouds and precipitation associated with atmospheric rivers. For their investigations, they simulate an episode observed during the ACAPEX campaign with the WRF-Chem model, coupled with a spectral-bin microphysics scheme. They find that marine INPs have significant influence during the periods before and after the atmospheric river event, resulting in increased snow formation and precipitation.

This is a well-written paper and a very interesting study, as the role of marine INPs in the atmosphere remains poorly quantified. The study also includes some novel modeling aspects, such as the inclusion of a freezing parameterization for marine aerosols in WRF-Chem. For this reason, I recommend the manuscript for publication after some minor comments below have been addressed.

We thank the reviewer for your time and helpful comments. Our point-by-point response is enclosed.

Comments:

– The paper includes a rather long introduction which provides brief information about the ACAPEX campaign and the examined case study. I would suggest making the introduction shorter and add two separate sub-sections about: (a) the field campaign, the utilized instruments and their respective uncertainties, and (b) a description of the examined event (meteorological and aerosol conditions). I think it would be very helpful for the reader to have a clear view of the episode's characteristics before reading section 3. Also information on instrumentation is scattered in the manuscript, while it would be better if this was gathered in a separate section, again before section 3.

We have shortened the introduction, mainly on the INP description. We have added another section - section 3 for case description and measurements and their uncertainties (right before the Result section), since those details are not appropriate to put in the introduction section. This also leads to moving the original Fig. 6 up to Fig. 2 in the revised manuscript.

– lines 144-145: *The SBM scheme is a fast version in which ice crystal and snow (aggregates) in the full version*. I don't understand the meaning of this sentence could you explain in more detail?

The sentence has been clarified as "The SBM scheme is a fast version in which ice crystal and snow (aggregates) are represented with a single size distribution (low-density ice) with a separation at 150 μm in radius, and graupel or hail is for high-density ice represented with an additional size distribution" (Line 137-138)

– line 263: I assume that cumulus parameterization is neglected in both domains. However it would be good to state that also in the paper.

We have added the sentence "Cumulus parameterization is not considered for the simulations over both domains" to the last sentence of the 2nd paragraph on Page 12.

– line 296: *shown in a later figure*. Please state the number of the figure

It is Figure 10. Since a few figures before this figure have not been cited yet, we are not allowed to cite that figure. Here the main purpose is to let readers now it will be further discussed with figures later.

– line 296-297: *This is because dust is mainly from aerosol bins at larger sizes.* I guess this is something indicated by the measurements? If so, specify, and if possible provide information on the prevalent dust particle size range that was observed.

Here the discussion is about simulated dust mass and number along the aircraft path. We meant to say the dust number concentration is dominated by small particles but it was said oppositely. Thanks for capturing the mistake. It is now corrected, and numbers are provided, i.e., "Although the simulated dust mass fraction is ~14%, the derived number concentration for sizes larger than 0.5 μm is very low (less than 0.02 cm^{-3}, shown in a later figure). This is because the dust number concentration is dominated by small particles (14.71 cm^{-3} for the sizes smaller than 0.5 μm)". (Lines 344-346)

– Line 313-316: This is not very obvious to me as the three simulations look very similar. Maybe it would be better if you could provide a mean precipitation value in the text for the region you are examining in these lines (and also be more specific about the exact location where these differences are observed)

We have added white boxes in Fig. 4a to mark up the region we are examining. We also discussed mean precipitation over the white box area. The differences in precipitation rates between the simulations and observations were also plotted and shown in Fig. 4c (note we did not plot the differences with percentage because some very low values in denominator make huge values in percentage). All quantitative discussion has been added as "All three simulations predict a narrower but higher peak precipitation compared with the observed wider peak with lower values (Fig. 4b). However, the overestimation of the peak value by DM15+MC18 is lower than the other two (30% vs. 45% for DM15 and 58% for Bigg; Fig. 4b-c). The accumulated precipitation in the southern mountain range (the southern part of white boxes in Fig. 4a) is generally less than 100 mm in observations and less than 120 mm in DM15+MC18 but more than 140 mm in other two simulations. The mean precipitation over the white box accumulated over the AR period are 89, 128, 130, and 116 mm for observations, Bigg, DM15, and DM15+MC18, respectively. Again, although all three simulations overestimate the precipitation, DM15+MC18 simulates the lowest value and closer to observations" (Lines 358-367).

– **Line 317-318:** When I first read about the spillover effect here, I was surprised that this is simply mentioned as a hypothesis with no further detailed investigations. Then I figured out that this would be further examined in another subsection. It worths mentioning here that this will be discussed in more detail in section 3.2

As suggested, "This will be discussed in more detail in section 4.2" has been added (Line 370).

– **Line 319-320:** Again this difference is not very prominent at latitudes >40N. Either provide a mean estimate for the examined region or maybe show contourplots of the difference between the different runs

We calculated the accumulated precipitation during 06:00 UTC 5– 09:00 UTC 8 February averaged over the region with latitudes greater than 40° N and the values are 45, 42, and 48 mm for Bigg, DM15, and DM15+MC18, respectively. The text was modified as "In the northern part of the domain (> 40º N), DM15+MC18 predicts more precipitation (i.e., 48 mm for the mean accumulated precipitation) than the other two simulations (i.e., 45 mm in Bigg and 42 mm in DM15" (Lines 371-373).

– **Line 333-334:** To solve this problem, many WRF studies conduct the simulations in segments (e.g. in 48-hour segments including a 24-hour spin-up after each initialization). Then they concatenate the outputs from the different segments. Consider adapting this method in your study

Thanks for suggestion. Indeed, we sometimes employed this approach. For this case, we did not adopt it because we wanted to simulate the entire AR event continuously without resetting the simulation in the middle of the event.

– **Figure 5:** While indeed the inclusion of MC18 parameterization substantially improves cloud fraction, the representation of total condensate is in worst agreement with observations. This is not mentioned in the text at all, while it would be useful to have a more quantified discussion on these discrepancies.

This was discussed (now Lines 403-407). We have modified it by adding the quantitative values which are shown in Figure. That is, "LWC is overestimated in all three simulations with DM15+MC18 of the largest overestimation (6 times higher than observation), while IWC is underestimated in Bigg and DM15 (nearly an order of magnitude lower in DM15 than observation) (Fig. 6c). DM15+MC18 predicts much higher IWC than the other two simulations, with an overestimation of IWP by ~3 times".

– **Line 393:** It is not very obvious to me how such large differences (>100%) in precipitation are estimated from Figure 7a, while precipitation rates are so close for the two runs

We calculated the relative change in percentage as [(DM15+MC18) – (DM15)] /(DM15)*100%. Since the precipitate rates in DM15 at some point before AR landfall are very small, the large increase from DM15 to DM15+MC18 is difficult to see. For example, at 16:00 Feb. 5, the percentage increase is 327% from 0.008 mm/hr in DM15 to 0.033 mm/hr in DM15+MC18.

– **Figure 9:** I find very interesting that the vertical structure of liquid and ice is so different between the two simulations. Is it possible to evaluate which structure is closer to observations? Did the aircraft make some vertical profiling of cloud properties? In Figure 5a only a relative shallow LWC/IWC profile is presented

The aircraft did not sample much in the vertical direction. Fig. 6 showed the flight path, and vertically it only spans over 4.3 -5.0 km, which is only 1-2 vertical layers in model.

– **Line 463:** homogeneous freezing rates are mentioned in the caption of this figure but not in this text line.

This sentence talks about the heterogenous freezing rates from the DeMott et al. (2015) and MC2018 parameterizations. It is not for the description of Figure 10 or the simulations of DM15 and DM15+MC18.

– **Line 471:** while differences in nucleation rates at temperatures above the -15^{O}C isotherm are discussed, this is not the case for differences below -37^{O}C (which are also very prominent).

We have added the discussion about the differences in the homogenous freezing rates, i.e., "Homogenous freezing (< -37 ºC; Fig. 10d vs. 10c) occurs less in DM15+MC18 because of a larger consumption of liquid drops and supersaturation in the heterogenous freezing regime. This is commonly seen in convective clouds (e.g., Zhao et al. 2019)." (Lines 510-512)

– **Table 1:** why some values are discussed in %, others in 'times' and other parameters are presented in absolute values? It would make more sense to use the same approach for all parameters

We use % in general in this table. For IWP, because the increase is so large, for example, it will be 44000% for post-AR if percentage is used, we use "times" which is more straightforward to readers. For the glaciation ratio and snow precipitation ratio, it is more physically meaningful to show the ratio (actual value) instead of percentage change.