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.

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.

– 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?

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

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

– 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.

– 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)

– **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

– **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

– **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

– **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.

– **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

– **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

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

– **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).

– **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