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Comment on acp-2021-537

Anonymous Referee #2

Referee comment on "The effect of marine ice-nucleating particles on mixed-phase clouds" by Tomi Raatikainen et al., Atmos. Chem. Phys. Discuss., https://doi.org/10.5194/acp-2021-537-RC2, 2021

Review Raatikainen et al. "The effect of marine ice nucleating particles on mixed-phase clouds"

In the paper "The effect of marine ice nucleating particles on mixed-phase clouds" by Raatikainen et al. 2021 the authors present a case study with the large eddy simulation model UCLALES-SALSA on the importance of local marine INP emissions on shallow marine mixed-phase clouds. They show that marine aerosol emissions can contribute to the boundary layer INP budget and influence the cloud (differently strong depending on background conditions). The study is very interesting and the analysis done very carefully. It fits well to ACP. Some of the background, basic assumptions and limitations of the study are unclear and have to be phrased out, therefore I suggest to accept the paper after major revisions.

General comments:

- The (regional) focus of the paper is unclear. In the introduction a lot of the literature etc. is focused on the Southern ocean, but the case study presented in an Arctic case study. Both regions are regions where marine aerosols as INP could play a role, but the introduction could be clearer and/or less diverse and more focused. The choice of ISDAC as a case study could be set in a better context.

- Another aspect related to the choice of ISDAC and the setup of the study is the question how valid it is to assume that sea spray emission in the ISDAC region acts like on "open ocean". The sea surface of ISDAC was not covered by an area-wide ice cover, but sea ice was present which certainly has an influence on sea spray emission. This should be discussed more in the manuscript. It should be stated how large the domain was (maybe with some map as well), how/where it was ice covered and if that was taken into account when calculating the sea spray flux. For other Arctic case studies there might not be a

marine source of aerosols from the surface just because of the presence of an ice cover.

- The description of the ice nucleation scheme used is very scattered throughout the text and difficult to follow. It seems that a CNT approach is used and in the text it is mentioned that the contact angle (for which species?) was increased because of the warm temperatures of the case study. It is unclear if that only yields for the dust aerosols and what the change of contact angle does represent (since that necessarily reflect on the composition/population of aerosols). It is not explained if the same parametrisation is used for the sea spray aerosol or not. If in the presented model setup sea spray freezes with the same contact angle as the dust particles (or even the adjusted contact angle) this would be quite a limitation on the study when it comes to the interpretation of the results. Sea spray does not trigger freezing as effectively as dust and the whole conclusion (that sea spray emission can have a significant influence on clouds) depends quite a lot on the assumptions made for the freezing of both aerosol types.

Specific comments:

- There is at least two studies missing when it comes to the simulation of marine aerosols as INP: Huang et al., Global relevance of marine organic aerosol as ice nucleating particles, ACP 2018 (https://doi.org/10.5194/acp-18-11423-2018) and McCluskey et al., Numerical Representations of Marine Ice-Nucleating Particles in Remote Marine Environments Evaluated Against Observations, GRL 2019 (https://doi.org/10.1029/2018GL081861). Check again your background literature.
- p. 2, l. 69-71: I would suggest to remove the last two sentences on the case study here. It is not the approach used in the paper and thus a bit confusing.
- p. 6, l. 155-156: I don't understand what is meant by "cloud base and top heights represent the domain minimum/maximum value". Are the domain minimum/maximum values used for the cloud base and clou top heights here? What was the range of these values throughout the domain?
- p. 6, l. 164: That sounds misleading, the simulation INP on with lower BKGD is similar to BKGD medium without marine sources, but not the simulation with zero BKGD? At least not until 14 h (and there most of the simulations look the same). So it does not seam correct to say it is the marine emissions alone.
- p. 7, l. 180: "The INP mass includes the total dust mass in aerosol, cloud droplets and ice crystals." How exactly is that done/meant? Is it accounted for which particles were activated/froze and how is that done for the different categories (when looking at the total dust mass it is not known how the ratio is of particles that would lead to freezing; when looking at the dust aerosols within ice crystals, they did obviously nucleate ice)?
- p. 8, l. 185: What is meant by "significant fraction of ice in all our simulations"?
- p. 8, l. 188: Why is the surface INP a 0.005 fraction of total SSA flux which is a lot higher than fraction of initial aerosol concentration?
- p. 9, l. 205: Specify: the other simulations (context unclear?).
- Fig. 5: Why is the simulation set with high background concentration chosen for this figure? Would it not make more sense to choose a lower background concentration simulation set?
- Fig. 5 caption: Rephrase: "diffusion, subsidence and surface flux contribution" instead of "diffusion, subsidence and surface" (?).
- INP and ice budget: Here it could be beneficial to have the plots from both analysis next to each other and maybe combine analysis in one text section (only a suggestion, also fine to leave it as is).

- Fig. 8: It would be interesting to plot an equivalent figure as a function of temperature.
- p. 13, l. 273: "higher INP mass leads to higher nucleation rate at a constant rate of vertical velocity" - is it not the other way around (higher nucleation rate leads to a higher INP mass...)?
- Fig. 9: Here the labels etc. in the figure and/or caption could be a bit more detailed. It is difficult to understand the plot like this without the text.
- p. 15, l. 293: "aerosol freezing" is unclear to me. Which freezing pathways are meant? Deposition and contact freezing?
- p. 16, l. 338: You write here that your results are limited to your simulations, that is also limited by the chosen case study. This could be discussed a lot more in detail (here and maybe even in the introduction and methods section).

Technical corrections:

- Values and units sometimes break over two lines, make sure to have a protected space between values and units to prevent this.
- p. 13, l. 275: I would suggest "cloud regions" instead of "clouds".
- p. 13, l. 278: "Following..." is without a clear reference, the sentence like that does not seem complete.
- p. 15, l. 321: Change to "maintain the simulated mixed-phase cloud".