Atmospheric Chemistry and Physics Discussions

Atmos. Chem. Phys. Discuss., referee comment RC2
https://doi.org/10.5194/acp-2021-316-RC2, 2021

Comment on acp-2021-316

Anonymous Referee #1

Referee comment on "A numerical framework for simulating the atmospheric variability of supermicron marine biogenic ice nucleating particles" by Isabelle Steinke et al., Atmos. Chem. Phys. Discuss., https://doi.org/10.5194/acp-2021-316-RC2, 2021

Review Steinke et al. "A numerical framework for simulating episodic emissions of high-temperature marine INPs"

In the paper "A numerical framework for simulating episodic emissions of high-temperature marine INPs" by Steinke et al. 2020 the authors present a framework based on numerical simulations and measurements (field and laboratory) to estimate the role of marine biopolymer aggregates and marine bacteria for episodic emissions of INP at 253 K. The combination of different tools is well done and the results reveal interesting aspects when it comes to marine INP. Therefore the study is suitable for ACP after some minor revisions.

General comments:

- One aspect of the paper, which is a bit unclear to me as a reader is the focus on 253 K as the one and only temperature for the analysis. Why was the analysis not done for several temperatures? Why was the INP concentration calculated with this constant temperature? Are the measurements limited to 253 K? How different would the analysis look like in a colder/warmer temperature regime, e.g. Fig. 1?

- Related to that: 253 K is not really "high-temperature" in the context of immersion freezing in mixed-phase clouds (one would assume rather 258 or 263 K as "high-temperature"). I would suggest to adapt the title (see also specific comments) and the text according to that.

- There are many assumptions done in the course of the paper/framework. This is necessary, but unfortunately often mentioned without reflections on the uncertainty etc.. The paper would gain a lot more value if a reflection and if possible also a discussion for related uncertainties could be added. Sometimes also the legitimizations of assumptions are missing or not described in detail. For example: it would be good to discuss the limitation that comes with assuming that all episodic INP are coming from marine emissions (section 2). What is the legitimation in doing so and how much would your result change assuming other potential episodic sources? Another example is the amount of measurements when comparing the results (Fig. 3-5, 6-9) - is it sufficient, where are observations missing etc. (when analysing the variability of the numerical simulations...).

Specific comments:

- The title could be more concise. "High-temperature INP" is not clearly defined and many readers might associate that with higher temperatures than 253 K.
- Page 2, line 49: Are deserts really the largest source when also considering biological INP?
- Page 3, line 93: It would be helpful to add another 1-2 sentences of explanation for your main hypothesis why these studies point towards episodically emission.
- Page 3, line 107-108: Orellana et al. found gel particles in clouds to be smaller than 100 nm, but you assign 100 nm in study - isn't that a slight contradiction?
- Page 3, line 110: Are the marine bacteria in the mentioned studies similar ice-active as bacteria from terrestrial sources? Be more specific here.
- Page 4, line 132-133: I assume the INP are collected on filter and then investigated in droplet freezing experiments? Add this information as well to be unambiguous.
- Fig 1: How is the INP concentration derived from the measurements for Fig. 1? Is the frozen fraction from the droplet freezing experiments estimated at 253 K and then multiplied with the ambient aerosol concentration present at the field measurements (at which height/temperature was these measured/is that consistent)?
- Page 7, line 212: How crude is it to assume that SSA concentrations = concentration of large jet droplets? What is the uncertainty related to this assumption?
- Page 7, line 217: Is the lifetime so short because of the large size of the particles?
- Page 7, line 242: What is the size of the bacteria based on?
- Section 3.3: Is n_s calculated based on these two fixed sizes? What is the uncertainty in n_s and INP concentration later assigned to this assumption?
- Section 3.3: I am missing a formula here how exactly you transferred c_mar to an INP concentration, please name/explain the procedure in more detail (c_mar leads to lambda leads to ...). Also the units are missing for the specific variables, for example lambda (unit less?).
- Page 9, line 264: At 253 K (n_s)?
- Fig. 6-9: What does the shape of the calculated points represent (which uncertainties etc.)?
- Page 12, line 306: Would a factor of 200 be enough?
- Section 4 enumeration gaps: the first point does not necessarily follow the presented analysis, sea-air transport could be a separate point instead of being included in the second point, the third point could be more concrete: what to focus on etc..
- It would be great if an hypothesis could be added in the end for the gap seen in the analysis: which species are maybe/probably missing etc.. Which further measurements

are needed...

Technical corrections:

- Page 2, line 82: Split up in two sentences (from , which...).
- Page 3, line 104: "larger" needs a reference.
- Page 5, line 169: Space missing (RRS30 to).
- Page 7, line 201: Flip entrainment and jet drop concentration (the second follows the first). Maybe reformulate entrainment to transfer (entrainment reminds of aerosols being entrained in a cloud/air parcel)?
- Page 8, line 231: Add that EF is the entrainment factor? Is r_c defined earlier?
- Add . at the end of the figure captions (inconsistent in the current version of the manuscript).