

Interactive comment on “ A numerical framework for simulating episodic emissions of high-temperature marine INPs” by Isabelle Steinke et al.

This manuscript presents a novel framework for estimating concentrations of episodically elevated high-temperature marine ice-nucleating particles (INPs) in the ocean surface and their subsequent emission into the ambient. Although there are larger uncertainties of this method and several scientific gaps (such as (1) size-resolved measurement of INPs, marine bacteria and MBPAs, (2) enrichment factors of marine bacteria and MBPAs, and (3) ice nucleation surface density site of bacteria and MBPAs in different temperatures) remain unclear, this work still gives us a new way to link the ocean activities and INPs contribution. The following major comments must be satisfactorily addressed before consideration for publication.

Major comments:

1. In this study, the production of bacteria and MABPs is assumed by jet drops. However, considering the size of MBPAs (100 nm), the production of MBPAs via film drops might be also important, as the small organics are enriched in the sea surface microlayer. Besides, can bacteria be produced via film drops? In terms of the number concentration, particles produced via film drops are much higher than jet drops.
2. Will the diameter of MBPAs and bacteria affect your model results? And How? I preassume that with a precious number size distribution of MBPAs and bacteria, the model results will be more close to reality.
3. I think it needs more discussion concerning the model results (Fig. 6-9). For example, (1) the EF and ns linearly correlated with model INP number concentration, but the high concentration (90th) of bacteria and MABPs are not. (2) Which factor is more important to control the model results? It seems the high bacteria and MBPAs concentration is less important than EF and ns numbers. (3) In the MARCUS site, field INP number concentration in DJF is higher than SON, the model results also follow the same trend. (4) Why model results in the MARCUS show larger variation, but in the MAGIC and MHD show small variation? But the field results in all sites show comparable variation.

4. A suggestion: It would be nice to show the simulated INP concentration from bacteria and MBPAs on a global map and compare it to previous studies, such as Burrow et al., 2013.
5. Why you only compare the episodic INPs, i.e., the highest 75th percentile INPs? If you compare the median of all INP number concentrations with your model, the results might be comparable. As the paper is well structured, I just need clarification in the response.

Minor comments:

1. Lines 99-102: I am not clear why you have this paragraph in the abstract. The logic flow seems been interrupted by this paragraph. Please clarify it.
2. Lines 116-120: It worth mentioning that the enrichment factor is also related to particle sizes. See Fig. 5 in Quinn et al., 2015.
3. In Fig. 6-9, Error bars of field measurements (blue dots) are needed. I also suggest combining Fig. 6-9 into one figure, with (a) to (d) panels.

References:

Burrows S M, Hoose C, Pöschl U, et al. Ice nuclei in marine air: Biogenic particles or dust?[J]. *Atmospheric Chemistry and Physics*, 2013, 13(1): 245-267.

Quinn P K, Collins D B, Grassian V H, et al. Chemistry and related properties of freshly emitted sea spray aerosol[J]. *Chemical reviews*, 2015, 115(10): 4383-4399.