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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Characterization of aerosol particles at Cape Verde close to sea and cloud level heights – Part 2: ice nucleating particles in air, cloud and seawater" *by* Xianda Gong et al.

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General Comments

This is a very well executed, and fairly comprehensive study of ice nucleating particles in the Cape Verde region, especially novel in including measurements in all water and air compartments, and attempting to relate these meaningfully.

Overall I had only few comments of significance, and the rest are mostly editorial notes. Specific questions/comments for addressing before publication are listed below.

Specific Comments

1) Abstract, last sentence: I struggled to understand this sentence, although I think it is saying that unless there is an unusual SSA INP emission mechanism in the study area, the INPs cannot be from SSA. But the use of the phrase "unless there would be" seems to beg a question that I thought the papered endeavored to answer. As I read, it seems inconceivable. Or are you referring to situations that you did not measure? I suggest thinking about rewording this sentence.

2) Intro, page 2, line 8: A minor note, since it is not relevant for the main topic of this paper. It is difficult to encapsulate this discussion that seems to be required for every INP paper, but this statement does not reflect any role for INP at temperatures lower than -38 °C, which is not the case.

3) Intro, page 2, line 28: Higher than ambient INP concentrations at ground level?

4) Intro, page 2, line 29: I do not understand the meaning of, nor see the need for, the ending phrase of this sentence (i.e., . . .from the biosphere). It is clear that most INPs come from the biosphere, and the ocean source comes in the form of sea spray aerosol emissions. I favor being explicit.

5) Intro, page 2, line 33: Bigg suggested that INPs were contributed to at least some extent from marine emissions, in the data collected in that region at that time. His abstract statement reads that it is not feasible that they are "only of continental origin."

6) Intro, page 3, lines 10-11: This is an oft-misinterpreted point. These papers parameterize INPs following this segment of the aerosol population, especially in the free troposphere, but the intention is to reflect INPs at all sizes. It is simply a hook to these concentrations, not intended to represent an actual "fraction" of them.

7) Intro, page 3, lines 16: Perhaps add qualifier that these observations were in the Arctic "boundary layer". I bring this up a few times because many things could differ in the free troposphere and at the level of some colder clouds.

8) Experiment and Methods, page 4, line 13: The filter sampler was truly sitting at ground level at MV? Or what was the elevation of the sample head? E.g., 1 m above ground?

9) Experiment and Methods, page 5, line 19: Notes on Table S2. The volumes listed do not seem to work out with other information provided, and the header units seem wrong. First, is duration actually in minutes instead of hours? Also, is std volume (must be L, not L^{-1}) for the 2 cm^2 surface stated as taken? It does not quite make sense, since if this represented 1/100 of the volume flow of 500 lpm, then about 10 times this volume should have been represented. Please fix header units at least. And state in table description if volume is for the "punch" or the total filter. I will revisit this in the next comment.

10) Experiment and Methods, page 6, lines 24-25: So if I have it correct, this amounts to about 0.75 cm^2 of the filter surface area (96, 1 mm punches). Yet this area is stated as 2 cm^2 in the previous section. And these new numbers still do not seem to work out to give the sample volumes stated in the tables. I ask these things only if someone wanted to reproduce such work.

11) Experiment and Methods, Section 2.3.2, page 7: Was background testing also done for the other collections, for example by rinsing clean plates use for microlayer sampling and by rinsing the bottles used for seawater collection? Just looking for a few words.

12)) Experiment and Methods, Section 2.3.3, page 8: I am curious if this calculation of freezing point depression was checked for validation, by for example diluting a seawater sample?

13) Experiment and Methods, Section 2.4, page 8: When introducing surface active site density (the terminology I am used to it being referred to as), it could be good to mention already the fact that when applying it to the total aerosol distribution, this artificially assumes that all particles are the same INP type in the contained surface

area. This is distinct from a laboratory scenario of generating a specific aerosol, and so will fold in all of the influences present in ambient air.

14) Results, page 13, lines 5-6: If bio-INPs, the size range is a bit unexpected for marine bacteria, which tend toward micron or less sizes I think. This might support that the bioaerosols are coming from long distance. It also strikes me that a mention for future work might be to include a collection at >2.5 microns, as one wonders about details of the INP size distribution.

15) Results, page 13, line 12 and Fig. 6: It is not optimal that clouds impacted most of the filters without control, for example by shutting off the pumps, though one understands that clouds are likely pervasive there. This is interpreted as INPs being captured into cloud droplets, and this is supported, but not fully clear. I was struck in Figure 6 by the fact that the CVAO INP concentrations in 6b all appear higher during cloudy periods in comparison to the couple of periods in Fig. 6a with fewer clouds. Any ideas on why this is so? The elevation of INP concentrations is over an order of magnitude in a few cases with the largest humps. This is unusual, but also makes me ask if the conclusions about capture of INP into cloud droplets is the full reason for differences below and within cloud. One factor could be drizzle and precipitation. It should be mentioned are solid if clouds were clearly not drizzling, as this could remove or redistribute INPs.

16) Results, page 14, line 1: Do you discuss cloud diameters in that companion paper? If not, is it consistent with some inference to cloud droplet distributions? This seems to require statement at this point, not later only on page 16.

17) Results, page 16, lines 21-24: It is not stated explicitly, but it seems clear that for this study, the clouds tended toward relatively high water content for marine Cu, with the lowest values equivalent to the assumptions of Petters and Wright, and the highest values exceeding Rangno and Hobbs. This is just a comment. Drop sizing or LWC measurement would be quite useful in any future studies of this type.

18) No reply needed, just to note that the agreement shown in Fig. 8 is striking, even

stunning. Also, the discussion at the end of the Results section regarding Fig. 9 is excellent.

19) Discussion, page 20, lines 1-2: The referenced results for the lack of an influence of SSA on INPs is for a completely different location. I am not sure of the relevance of comparing the two studies other than to note that they concluded the same thing. Are you trying to say the SSA INPs never dominate? I would reject that notion. The only commonality in the two regions is that land sources are present at distances within a day or two trajectory distance. This is not the case everywhere.

20) Discussion, page 20, lines 3-4: How does one know what maximum T desert dust is active? Doesn't this study suggests that -10C is not unreasonable?

21) Discussion, page 21, lines 5-6: Is this too prominent a question? Perhaps, but perhaps not. In regions where marine and dust populations strongly intersect, and both populations contribute to the surface area, it seems that it will ultimately be necessary to parse out the contributions. This was not done in DeMott et al. (2016), and that probably makes inclusion of those data as purely SSA somewhat suspect for the data collected in the Caribbean, especially. It makes it difficult to discern anywhere, if a few percent of dust by number is sometimes present (a few papers on this are in press). Do you have any compositional inferences to use here? Consider figures 4 and 5 for of Gong et al. (2019a) for varied compositions during different times. Do the numbers roughly work out if you assume something to use as pure dust? You do not really dig into this at all. It may be a major question, but you appear to have some additional information that would allow you to state if your data are consistent with the proportions of mineral and marine particles.

22) Summary and Conclusions, page 21, lines 19-20: The sentence is not complete, and it is unclear if it is referring to other studies or this one. If referring to this study, I suggest to say that "biological particles appeared to contribute. . ." I note though that no confirmatory tests were performed to ascertain biological INP influence.

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