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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Ice nucleating particles in Canadian Arctic sea–surface microlayer and bulk seawater" *by* Victoria E. Irish et al.

Anonymous Referee #2

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General Comment Overall my comments are rather minor on this paper. It is a nice addition to the literature on the sources of ice nucleating particles from ocean seawater, and on expectations for enrichment or not in the sea surface microlayer. As detailed in my specific comments, I wonder if there is a reason to rule out non-colligative effects on freezing for explaining salinity variations, I felt it unfortunate that total organic carbon measurements were not included in order to compare with the Wilson et al. (2015, Nature) study, and I feel it would be nice to see the full influence of the heating studies on the temperature spectra of INPs. There is overall perhaps too much emphasis on 10% freezing conditions. Nevertheless, this is an excellent example of the suite of data that one might like to have when simultaneously collecting atmospheric samples over oceans.

Minor revisions are recommended. Specific questions/comments for potentially addressing are listed below.

Specific Comments

Introduction

Page 1, lines 37-38: "Homogeneous ice nucleation becomes increasingly important below approximately -33°C. . ." This statement struck me as odd. Why -33°C specifically? Use of such a value seems to beg also listing a droplet size and a time scale. In fact, there are abundant observations in the literature of supercooled water present at this temperature and down to 4 or more degrees below this.

Page 2, lines 13-14: It seems likely that the transfer to the atmosphere also remains a highly uncertain process on the basis of recent studies, although it is not a topic in this paper.

Experimental

Page 3, line 7: I was curious that there was no apparent pre-sterilization for microbial contamination. Does isopropanol assuredly do that?

Page 3, line 20: Should blanks be in quotes? The reason is that this cannot be a true blank. There are literature reports of sub-20 nm particles acting as INPs. I think you will refer to these as "blanks".

Page 3, line 21: Can you state a conductivity level on the DI water?

Page 4, Line 5: Did you happen to test the filters after rinsing with ultrapure water?
Page 4, line 8: First, it would seem appropriate to state that the water activity correction is an average one based on fits, since uncertainties commonly occur. The authors may also wish to discuss how other elements in the seawater that induce non-colligative freezing effects might stymie this approach. So, for example, what if seawater contained AFPs?

Results and Discussion

Page 5, line 13: What is significant about this arbitrary T_{10} value chosen? Should not correlations be checked for a range of fractions or at single temperatures? Also, were any TOC measurements made? This seems a missed opportunity to correlate with the relation suggested in Wilson et al. (2015). Page 5, line 20: Note the extra space at the end of this sentence. Also, can you rule out non-colligative freezing effects that scale with salinity? I have no reason to understand why this would be so, since I know of no such studies for seawater. It could be useful to show a plot of the relation you are discussing, in the supplemental material, if not in the main manuscript. Then it might be clear if the correlation shows any bias that could be explained by a constant "delta" on the freezing temperature.

Page 6, lines 12-13: I suspect that additional studies could also indicate which method is closer to correct, or if a new and more elaborate method might be warranted.

Page 7, lines 3-4: The conclusion made here provides a reason to show full temperature spectra for sizing analysis. Would differences stand out at certain temperatures? Or at lower levels of freezing?

Page 7, lines 7-9: I am curious if there are known things that are non-microbial or non-proteinaceous that are denatured by the heat level used. Is there an expectation that the composition of exudates would be unstable at 100°C? I do not know the answer, just asking, as O'Sullivan et al. (2015, Scientific Reports) does not suggest anything other than microbial fragments and proteins as being particularly heat sensitive.

Summary and conclusions

Page 7, line 18: "Biological materials" seems too broad or non-specific of a category. They are heat labile biological materials, which might imply something more (i.e., comment just above)?

Figures

Figure 6: Question - if only 15 to 20 drops are used, how are frozen fractions below 5

Figure 7: It might be interesting to see separate plots for each filtering size as a function of temperature, as in other plots. This would highlight if any differences occur at low freezing fractions at the warmest temperatures and how things vary with processing temperature. In that manner, the full exclusion of a role of larger particles in the bacterial size range might be better supported.

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