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Atmospheric Chemistry and Physics
Discussions

Interactive comment on "Ice nucleating particles at a coastal marine boundary layer site: correlations with aerosol type and meteorological conditions" *by* R. H. Mason et al.

Anonymous Referee #1

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Mason et al. present results on ice nucleating particles (INPs) from a coastal site in western Canada during the summer. The INP concentrations strongly correlated with fluorescent terrestrial bioparticles at high temperatures, while particles that were likely mineral dust nucleated ice at lower temperatures. However, predicted INP concentrations using different empirical parameterizations did not corroborate the observations, demonstrating the need for improved modeling of INPs. The paper is overall well written and the methods and interpretation of the results are clear. There are a few needed improvements described below, however, once these are addressed, this paper is suitable for publication in ACP.

General comments:

The abstract could be strengthened by adding a sentence of two of broader implications at the end. What do these results signify and how do they advance our understanding of INPs? Perhaps here, and in general throughout the manuscript, one large motivation for work such as this is that the parameterizations did not corroborate the observations, demonstrating the need for more observations to improve simulated INP concentrations and their subsequent climatic impacts.

The introduction would benefit from more background, such as on primary bioparticles versus marine bioparticles. What are some of the sources of these types? What types of bioparticles are marine? Also, the authors conclude that dust was likely observed at the lower temperatures, so some background on mineral and soil dust as IN is warranted. It would be helpful to also include previously documented temperature ranges in which each of the different types of INPs nucleate ice at (use references such as Murray et al. (2012), Conen et al. (2011), DeMott et al. (2003, 2009, 2013), O'Sullivan et al. (2014), etc.).

The dates of the sample collection should be provided first thing in the methods. Otherwise, there is only one figure that includes an Aug time period but the exact dates and year should be provided.

In the methods, the DFT measurements were conducted at, "-10 C per minute to -40 C." However, many of the results are presented in -5 C steps. Why are measurements not presented as -10, -20, -30, -40 C? Perhaps the measurements started at -15 C, but this should be explicitly stated. Were measurements acquired at -10 C? That would be an interesting comparison since the focus is on biological particles and these can nucleate ice up to -2 C.

Can the authors comment on the possible contribution from soil dust? Wouldn't this fluoresce as well with WIBS (as in Gabey, A.M., Stanley, W. R., Gallagher, M. W., Kaye, P.H.: The fluorescence properties of aerosol larger than 0.8 um in urban and tropical rainforest locations, Atmos. Chem. Phys., 11, 5491-5504, doi:10.5194/acp-11-5491-2011, 2011.)?

Considering the particle sizes observed and shown in Fig 6. I find it odd that these large sizes are more abundant in number than smaller particles (i.e., 0.5 to 1 um). Wouldn't the authors expect to observe smaller bioparticles, such as bacteria? Perhaps this is due to the transmission efficiency of the WIBS, which could be discussed since this is a relatively new technique.

The method for using correlation of wind speed at the site and INPs emitted from the ocean surface may not be the most direct, since the wind speed may be different over the water versus land surface. Have the authors considered estimating the wind speed from the HYSPLIT trajectories? That may lead to a better estimate of wind speeds over the ocean along the transport paths, since most of the trajectories remained fairly low in the marine boundary layer.

There should be more broad discussion on the parameterizations in section 3.7. The fact that the parameterizations did not fit the observational data demonstrate the need to improve these parameterizations by conducting more observations in different locations, times of year, and land cover regimes (i.e., arid, vegetation, near BC sources such as fires, etc.).

Specific comments:

Page 16275, line 17: Clarify that these are chemical tracers, and if space permits, provide the tracers (i.e., MSA and Na).

Page 16279, line 4: Briefly define Cfb.

Page 16280, line 4: Change “measured” to “collected”.

Page 16280, line 18: Spell out DFT on first occurrence.

Page 16288, line 23: Instrument and sampling details for CO, NOx, and SO2 should

be briefly provided in the methods.

Section 3.4: Was there any correlation of INPs with wind direction?

Section 3.6: In regards to the possible free tropospheric transport of dust, the authors could examine 10-day air mass back trajectories for this particular time period to evaluate the potential sources of the aerosol. For instance, if the trajectories all pass over one of the major arid regions in Asia or Africa, this would support their assumption that mineral dust contributed to the INP concentrations at -30 C.

Page 16292, line 15: What are some of the potential sources of INP along the coastal NW that would be larger than 1 um? The vegetation coverage is discussed for the entire region in the first section of the methods, but it could be specified here what is NW of the site.

Page 16293, line 3: Up until this point, the maximum size for the WIBS used is 10 um, why the change here?

Page 16294, line 1: But in the introduction on page 16278, lines 2-3, sea salt is presented as having the ability to serve as INP. Perhaps the authors should clarify that these referenced studies investigated NaCl or sea spray to form ice at very low temperatures (roughly -35 to -58 C), thus sea salt has the potential to form ice, yet is inefficient at temperatures relevant to heterogeneous ice nucleation.

Fig 2: It would be useful to, in some way, also show the trajectories colored by source group (similar colors as in Fig 3). Perhaps an additional panel with the same trajectories colored by group would suffice?

Fig 5: In the manuscript, the authors state that correlations which are insignificant ($p > 0.05$) are not discussed, yet they are shown here and are actually discussed in the manuscript. Perhaps this statement should be removed or revised if the authors choose to keep these data.

Interactive comment on Atmos. Chem. Phys. Discuss., 15, 16273, 2015.