

## ***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 #2**

Received and published: 31 August 2015

In this manuscript, Mason and co-authors present an experimental study on the abundance, the nature and the origin of ice nucleating particles (INPs) measured at a coastal site in British Columbia. It was a pleasure to read this article for various reasons: it is clearly structured and well written, the applied methods are well described or cited, the measurements well documented, the data evaluation and interpretation is solid and appropriate, and the conclusions and atmospheric implications are clear and carefully formulated which I prefer versus over interpretation. In summary, this is an excellent manuscript with new and relevant results which in principle can be accepted for publication in ACP as is.

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I just have a few minor comments the authors may consider for the final manuscript version:

My first comment refers to the mixing state of the different compounds (biological, mineral, soot, etc.) in the aerosol size distribution. This mixing state was not measured in this study, but may have an influence on some of the conclusions, like the contributions of biological particles to the INP abundance concluded from the size distributions as shown in Figure 6. What if the larger particles are just more likely to carry a fluorescent biological particle but the ice nucleation activity is related to some other particle component? The same can happen with soot or other smaller particles that have been collected by larger particles and thus still may contribute to the ice nucleation activity of the apparently larger particles. This possibility or limitation may be mentioned somewhere in the manuscript and also in the conclusion section. This comment also refers to the need of particle mixing state information in future atmospheric INP studies. Would it e.g. be possible in future studies to co-locate the INPs with fluorescent signatures (or other particle compound or property signatures) on the same substrate?

I agree to referee 1 that the abstract could be strengthened. I also recommend extending the conclusion section for the most important findings and atmospheric implications.

In the abstract line 12 the correlation between INPs at  $-30^{\circ}\text{C}$  and total particles larger than  $0.5\ \mu\text{m}$  is mentioned. I think Figure S1 shows INP at  $-30^{\circ}\text{C}$  being equally well correlated with fluorescent and total particles. How can then be concluded for an extra contribution of non-biological particles to INPs? I recommend moving Figure S1 to the main manuscript.

The first sentence of the introduction reads as if there is no contribution of heterogeneous ice nucleation in cirrus clouds, which certainly is not the case.

When discussion the various ice nucleation modes you may also cite Vali et al., Atmos. Chem. Phys. Discuss., 14, 22155–22162, 2014.

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