

## ***Interactive comment on “Sources of organic ice nucleating particles in soils” by T. C. J. Hill et al.***

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Received and published: 6 February 2016

In addition to the excellent review by P. Amoto I have the following comments.

**General Comments:** This paper presents the results of a wide range of chemical and heat stress tests on a small selection of soils from Wyoming and Colorado to determine the number and nature of the organic ice nucleating (IN) particles in the soils. The paper has above average detail in referencing earlier research in the field, the methods are sufficiently documented and the paper is well written.

Overall the paper presents a unique set of data that expands knowledge on organic IN in soils. It is an important paper and will be a well referenced publication.

**Specific Comments:**

Page 3: lines 17 and 18. Since the 0.1 M HCL contained a lot of water, large numbers of organic IN may have been in the wash that was discarded. Were the washes tested

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for IN content? How is this potential large removal of IN taken into account? What is the purpose in sieving the soil? Were the smaller or larger particles used in the follow-on analyses?

Page 4: lines 10 and 11: Why were the samples filtered?

Page 4: lines 20-23: Again the water that was discarded probably had more organic IN than those left in the soil residue. Were the filtrates tested? Do I not understand the process here?

Page 7: lines 29-31: Same issue as above. Most organic IN will be in the filtrate, not within the remaining soil.

Page 8: lines 10-13: Schnell and Vali focused on litters from deciduous trees as they were noted to have more and better IN than evergreen vegetation. Possibly the earlier sample of evergreen litter contained deciduous plant litter as in later collections done more carefully, the evergreen litters were found to be poorer sources of IN than deciduous plant litters.

Page 10: line 6: When this test was repeated some years later it was found that the original tests probably allowed fine particles of dried leaf litter to contaminate the collecting apparatus. As such, vapor transfer of IN to the atmosphere should be discounted.

Page 11: line 12-13: It is not required that bacteria be alive to have IN activity as referenced in a number of publications cited in this paper. The IN on dead bacteria are general not the most active (no activity in the -1.5 to -30 C range) but can be active at colder temperatures.

Technical Corrections:

I read the paper mainly for the science. Dr. Amato has done an excellent job in the technical details.

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