

Interactive comment on “Ice nucleators have shorter persistence in the atmosphere than other airborne bacteria” by Emiliano Stopelli et al.

Anonymous Referee #1

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Summary:

This manuscript presents the analysis of 56 fresh fallen snow sampled at Jungfraujoch for which concentrations of Ice Nucleating Particles (INPs) and bacteria have been determined and the fraction of water vapour lost by the air mass prior the sampling has been assessed by oxygen isotopic ratio. Based on the difference of dynamic ranges between INPs and bacteria concentrations, authors propose that INPs are more rapidly deposited from the atmosphere than bacteria. They confirmed it by a faster decrease of INPs than bacterial cells when air masses are submitted to larger losses of water vapour. A second part of the manuscript emphasizes the wind as a determining factor in the concentration of INPs. Finally, the authors isolated *Pseudomonas syringae* from the snow and compared them to strains isolated from a large panel of environments.

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General comments:

The manuscript is clear, well-written, and in perfect adequacy with the scope of the journal. It brings arguments to elucidate what happens in clouds, and more generally in the atmosphere, where a lot of processes concerning microorganisms are supposed but not clearly demonstrated. Still, I have some concerns/questions about different parts of the manuscript:

1. I regret the structure of this paper in two parts: the first concerning INPs and bacteria persistence in the atmosphere and the second part concerning *P. syringae* and its belonging to specific phylogenetic groups. These two parts although comprehensible in atmospheric microbiology form a disconnected content.
2. In the first Results section, authors determine “dynamic ranges” to evaluate atmospheric residence times and support this citing “Fig 2 in Griffiths et al., 2014”. First, I do not believe this citation is relevant in this context. Second, I wonder how efficient this index would be for example in the case of a particle with a short residence time and important emission sources all around the sampling site?
3. Authors measured total bacteria concentration and INPs-8. Is there a statistical correlation between these two parameters?
4. Title should clearly exposed authors were interested in INPs active for temperatures warmer than -8°C (and as much as possible an insight of the work on *P. syringae*).

Other comments:

P1 Line 22: It sounds like it is the first time *P. syringae* is found in snow: it is the first time at such an altitude, please precise it.

P1 Line 27: Hemsfield (missing “i”)

P1 Line 30: Modify this sentence: Sands et al. (1982) did not demonstrate that *P. syringae* actually act as an INP in clouds

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P2 Line 23 : why did you adjust samples to physiological conditions? What was the range of temperatures tested?

P2 Line 25: Are you sure the filter active area is 6 mm diameter?

P3 Line 10: How long were the plates incubated?

P3 Line 37: I would appreciate a brief description of this method.

P4 Line 5: Is there a scientific reason you excluded the winter period for sampling?

P4 Line 20: This sentence is ambiguous, please reformulate.

P4 Line 10: I would remove “seasonal” as you sampled only 7 months of the year. Furthermore, bacterial concentration in May 2014 is higher or equivalent to those in July/August.

P5 Line 16: Replace “is” with “in”

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