

Interactive
Comment

Interactive comment on “Direct quantification of total and biological ice nuclei in cloud water” by M. Joly et al.

Anonymous Referee #2

Received and published: 21 March 2014

General comments:

This paper adds very valuable new information to the current debate about bioprecipitation.

In particular, the authors found that

(1) Biogenic ice nuclei were active at ≤ -6 C and were the dominant active ice nuclei between -6 and -10 C. At -11 C and below the non-biological IN accounted for more than $\frac{1}{2}$ of the ice nucleation activity, at -13 C reaching more than 90 % of total activity. (Fig. 2).

(2) The number of total biogenic IN (Table 2) that are active at high T (-6 to -8 C) was

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larger than expected based on earlier findings, e.g. approx. 100 x of Christner et al. The authors also speculate about the total biogenic IN being made up largely or even totally by intact bacteria. While that part of the paper is not convincing, it is still an interesting discussion – nevertheless the paper could stand alone without that part.

Critical to the quality of the paper is in my view the severe absence of statistical analysis.

In addition, as nice and new as the data are, a principle limitation of this work is that the mountain peak can produce orographic clouds, and thus the clouds can be contaminated by bacteria derived from the side of the mountain. Without addressing this issue, the authors cannot remove my suspicion that not all the clouds investigated were true high altitude clouds. It would be better to discuss this matter (instead of not mentioning the problem): Maybe the authors could have a meteorologist make an educated estimate how large the contribution from the surface of mountain side possibly could be?

Specific comments:

Page 3709, line 26 - : It appears that the authors have not considered the papers by Santl-Temkiv on hailstones' bacterial content and bacteria's origin (FEMS Microbiol Ecol 81: 684–695; PLoS ONE 8(1): e53550).

Page 3716, line 23: The statement on the bacteria targeted by lysozyme is not correct. See also the interactive comment by C. Morris on this matter.

Page 3719, line 1-2: OVERREPRESENTATION does not seem to be what the authors mean here. Either the use of this term should be explained, or the expression deleted.

Pages 3718-3719: Otherwise I agree with the authors' conclusions. See my general comments, above.

Page 3722, lines 23-25: I should think that this conference presentation can be replaced by citable publications originating from the same authors.

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Table 1: I strongly suggest that the sample volumes should be added as a separate column. This will enable the readers to fully appreciate the work done, and to use the data for their own considerations and estimates in the future. Another information that I feel is missing is the type of cloud(s) encountered in each event, e.g. convective, stratus, orographic.

Table 2, and corresponding discussion in the text: I miss a calculation of the number of IN per cloud droplet. Based on their LWC, the authors should be able to provide such an estimate. For the readers, such numbers would aid in appreciating the significance of the authors' findings for cloud physics.

Table 3, legend: (1) DETECTION limit is the wrong term, as the authors are referring to an upper limit of quantification (accidentally) caused by how much the droplets were diluted. (2) Please change first sentence to “Inferred maximum possible fraction of INA bacteria among total bacteria . . .”

Fig. 2: (1) Should the y-axis label not read “. . . frozen droplets”, instead of “. . . samples”? (2) Please change first sentence to “Cumulative proportion of frozen samples (droplets?) at specific freezing temperatures . . .”. (3) Please include error bars.

Fig. 3: What is the reason for showing every single data point, i.e. individual samples? The whole paper lacks statistics. Here is one obvious place to offer to the reader a regression, with corresponding statistics, over all samples.

Fig. 4: Also here the use of statistics would improve the paper. Here the reader would appreciate seeing error bars, confidence intervals, or similar measures of statistical significance.

Fig. 5: (1) Again, what is the reason for showing every single data point, i.e. individual samples? Also here is one obvious place to offer to the reader a regression, with corresponding statistics, over all samples. (2) The data points are the same as in Table 3. There is no need to show the data twice – unless . . . (see remark no. 1). (3) The y-

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axis scale is correct, but confusing. Please eliminate the “%” and change the numbers on the scale accordingly (e.g. 10% becomes 10e1). (4) Please change the sentence to “Inferred maximum possible fraction of INA bacteria among total bacteria ...”

Technical corrections:

Please re-work the whole manuscript for using grammatically and syntactically correct English. Here is a non-exhaustive list of spelling errors and wrong use of words:

CloudY air, several places in the paper, including abstract and legends

Back trajectory PLOTS (these are called back trajectories, not plots)

Cloud’S microphysics

At –10 C there WERE ..., seen in the abstract (correct to say “were observed, were measured, but not just “there were”)

guarantY, page 3710

wrong use of “nor”, page 3710

MaterialS, Page 3712, line 1

CIN, page 3713, line 12, (please put IN in subscript)

HITTING the puy de ..., page 3714 (better: “reaching” or “arriving at”)

otherS, page 3716, line 23

Interactive comment on Atmos. Chem. Phys. Discuss., 14, 3707, 2014.

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