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Interactive Comment

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

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The work described in this paper is based on a simple and powerful idea: a direct way to determine the potential for ice formation in a cloud is to collect cloud water and determine the content of ice-forming nuclei in it. Furthermore, whether those ice nucleating particles (INPs) are of biological origin can be determined via some direct and some indirect tests. The authors' practical approach to this idea was to collect cloud water from a mountain peak when a cloud envelops it.

Not surprisingly, it is difficult to realize the idea in its pure form. Complications arise from a number of directions. The main ones can be put in question form: 1. How complete is the transfer of all potential INPs from the air in the which the cloud forms?

2. How many ice particles have already formed in the cloud and have fallen out before sampling? 3. Is the exclusion of other modes of ice nucleation, other than immersion freezing, justified? 4. Is there any evidence for aging of the sample after collection?

In spite of the fact that answers to the questions raised are missing in the paper, or are minimal, it is a valuable contribution. The paper demonstrates that detection of INPs in cloud water is a promising approach to shedding light on long-standing questions.

The main shortcoming of the paper is that little information is provided about the clouds that were sampled. Was there precipitation occurring at the same time? Were the clouds forming in the uplift forced by the mountain slope or were they part of extensive cloud layers? How deep were the clouds? What can be said about the age and history of the cloud parcels? Clearly, it would take a project of much greater complexity to gain information on these aspects, the lack of even some broad descriptions and possible sorting of the data according to these variables weaken the results obtained.

At what temperatures were the collections made? It is mentioned that some samples froze onto the plate, but it is not clear if that made any difference. How long were the sampling periods?

Some information on the sampling intake and the general setup of the apparatus would be helpful.

The absence of data on cloud liquid water content is handled in the paper by using historical data with three different values assigned according to the collection rate of the sample. One wonders why the sample collections rates were not considered reliable enough to be used as a measure of cloud liquid water content. Changing droplet size distributions and variable collection efficiencies due to different wind conditions clearly weaken the reliability of such an evaluation. To what degree? The authors' reasoning for not using that approach should perhaps be in the paper.

The presentation of the results of the measurements is not always clear. Do expres-

ACPD

14, C500–C503, 2014

sions such as “ . . . samples froze at -8°C . . .. “ (3715/6), “. . . none remained supercooled ..” refer to one drop (sample tube) from the sample or some other measure? Comparisons based on “maximum freezing temperature” and “highest temperature” are subject to large errors and should be viewed as rough indications. More extensive use of the concentration functions and comparisons of concentrations at fixed temperatures, as in Table 2, would improve the paper.

What is the reason for stating -11°C as the lowest observed freezing temperature (3715/7) when Table 2 and the figures show data to to lower temperatures? How can the data in Fig. 4 extend to -14°C when the last points on Fig. 3 are at -13°C? The impression is that the low number of samples that provided data at -13°C and -14°C lead the authors to some hesitation about the data presentation. It would improve the paper if the results were presented in a more consistent way.

In Fig. 4, the substitution of lower bound values for those not detected introduces an upward bias in the data. How would the analysis look if only samples with measured values were included at all temperatures?

The data in Table 2 gives the impression that the most heat-labile samples had relatively low total concentrations of IN. Could the authors comment on this?

The higher values of INPs detected in cloud water compared to precipitation (3715/17) is a significant result and, if confirmed by more data, calls for an intensive search for explanations. Even as an early indication, it is a strong motivation for more work with cloud samples even if they are considerably more difficult to obtain.

It would be good to know whether the correlation stated on 3717/10 would also hold between bacterial concentration and total INP concentration.

Minor points (page/line):

3711/8 “all” and “throughout” are redundant 3711/9 “high-temperature IN” is difficult to replace with better wording, yet is awkward to call sub-zero temperatures ‘high’ 3713/12

ACPD

14, C500–C503, 2014

CIN instead of CIN 3713/18-19 Fewer significant figures would be sufficient (1.6 rather than 1.59 etc.) 3716/2 As shown in Table 2 “at least 77%” should be “as low as 77%” 3722/27 Initials for first author missing

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

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14, C500–C503, 2014

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