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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Ice nucleating particles measured in Swiss alpine snow samples are spatially, temporarily and chemically heterogeneous" *by* K. P. Brennan et al.

Anonymous Referee #2

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In their study Brennan et al. investigate INP concentrations from snow samples taken in the Swiss Alps. In total 88 samples were collected during the winter of 2018. Samples were obtained from 17 locations covering a vast area of the Swiss Alps. Attention was paid to terrain characteristics, elevation, snow age, snow depth and distance to Jungfraujoch. INP concentrations were determined in the lab together with other physicochemical parameters of the bulk meltwater. Based on the INP concentration per ml meltwater the authors also create a parameterisation to calculate cloud glaciation temperature.

The study is well conducted and scientifically sound. Sampling procedures are de-

scribed in detail in the supplement material and are appropriate. To measure INP concentrations the authors use a newly developed method that has been tested and is extensively discussed in a recent publication referred to in the manuscript. While the analysis of the data is rather descriptive, a large benefit of the study is that samples were taken at 17 different locations in Switzerland spanning a large area. Most other studies focus on single locations. Sampling was also done from snow depth profiles and the local variability was investigated. Such data are very useful because they are rare.

Overall the manuscript provides a large dataset that is very beneficial for the ice nucleation community. The study fits the scope of ACP and I recommend publication after the points below have been addressed.

General comments

Title – The title should be changed. The current title suggests that the nature of INPs was studied. However, I don't see that much about the nature of INPs can be said from the study. E.g. chemical analyses were done on bulk samples and as the authors point out I agree that "the chemical signature of an INP is probably lost among total aerosol chemistry" (P834-35). The INP size was addressed but actually seems to be within the same range for all samples, so not heterogeneous. Consider addressing the variability of INP concentrations in the title rather than the INPs directly.

Why did the authors not calculate differential freezing nucleus spectra? They present a very useful picture of the entire INP population (Vali 2019) and INPs could be qualitatively classified (warm mode and cold mode INP, see also Creamean et al. 2019). Looking only at T_50 values might disguise the presence of a few INP at high temperatures. Comparing T_50 values is a rather limited approach when investigating heterogeneous environmental samples. The authors should consider adding freezing curves to the supplement material. Their current data representation in form of box plots looks nice but omits potentially relevant information.

Specific Comments

P1L19-23: The abstract seems rather long. I suggest that the authors cut out L19-23. This describes just another way of plotting data (box plots). I don't see why this is really novel. See also my general comment.

P1L19: As far as I can see meteorological parameters were not used. Delete "meteorological".

P1L28: The equation stated refers to c_air and not the INP concentration per ml meltwater. Please correct.

P3L19: The elevation of both sites is about 2800m. Why is this well above the altitude of artificial snow production?

P4 Figure1: Please color code the sampling locations by altitude. At the moment it is not possible to attribute a certain altitude to a specific location, which would be useful and can be easily added.

P4L32: First, I am not familiar with biology reagent water. What is the purpose of biology reagent water? Is there a difference to ultrapure (MilliQ) water? Please explain. Second, what was done with the so determined background values? Were they subtracted from the respective snow sample freezing curves?

P6L4-10: This is a nice idea but I think this approach is not ideal for field samples with most likely heterogeneous INP populations. See also my general comment.

P6L7-8: Here it is stated that the data was not trimmed and all 96 data points are used, while the previous paragraph explains that the data was trimmed (omitting 2 wells). This is confusing.

P6L22: add "horizontally" . . .and scattered "horizontally" to avoid overlapping.

P7L35-37: I wonder whether blank measurements were done with all filters? Figure 5 suggests so. Add this information here.

P10L1-5: "SA background T_50" What is SA?

P11L14-15: I don't understand the conclusion that INP were abundant but inhomogeneously spread. Is there evidence for less snow drift at the St. Anna Firn site? Did the authors compare wind speeds during the last snow fall at the sites?

P11 Paragraph "Altitude Dependence": In order to evaluate the influence of the boundary layer airmass trajectories should be analyzed. A site at e.g. 2000m can be in or out of the boundary layer depending on meteorological conditions.

P14L32-34: I don't see in what way source regions or microphysical pathways upstream of the sampling locations were analyzed, but the statement suggests so. Neither meteorological data nor airmass trajectories were included.

P17L10: This section reads more like "Conclusions" and should not be a subsection to "4. Atmospheric implications".

References

Vali, G.: Revisiting the differential freezing nucleus spectra derived from drop-freezing experiments: methods of calculation, applications, and confidence limits, Atmos. Meas. Tech., 12, 1219–1231, https://doi.org/10.5194/amt-12-1219-2019, 2019

Creamean, J. M., Mignani, C., Bukowiecki, N., and Conen, F.: Using freezing spectra characteristics to identify ice-nucleating particle populations during the winter in the Alps, Atmos. Chem. Phys., 19, 8123–8140, https://doi.org/10.5194/acp-19-8123-2019, 2019

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