

Interactive comment on “Predicting abundance and variability of ice nucleating particles in precipitation at the high-altitude observatory Jungfraujoch” by E. Stopelli et al.

Anonymous Referee #1

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Stopelli and co-authors present a series of ice nucleating particle concentrations at -8°C in precipitation (INPs-8) from over one hundred samples taken at the Jungfraujoch research station in Switzerland. The results, which show concentrations ranging from as high as 434 INPs-8 ml^{-1} to below the limit of detection, are then compared with the range of ‘standard’ meteorological, aerosol and chemical observations at this site. The results from the first $\sim 80\%$ of the observations are then used to create models to predict the INP concentration, with the last 20 % of observations used as a model validation. The models provided suggest that the INP concentration in precipitation is potentially driven by wind speed, temperature, aerosol concentration, the season and source region, or the precipitation history of the air mass.

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The manuscript is in my opinion ideal for publication in Atmospheric Chemistry and Physics, and is also in a good condition. I have a number of minor comments and some technical corrections (listed below), however it must be noted that based on my personal background an in-depth critique of the statistical model part of the paper cannot be given.

Minor Comments:

Introduction: The introduction explains the importance of INP's for cloud processes and precipitation formation and then talks about measurements of INP's and why they are relevant at high temperatures. However, I missed any discussion of how the observations made, specifically of INP's in collected precipitation water, relates to the cloud processes themselves. There are various processes that would increase or reduce the concentration of INP's in collected water; these and their possible effects on the measurements should be discussed.

Section 2.1.: How were the settling of particles and losses/experimental artefacts prevented/accounted for? What is the instrumental background?

Section 2.1.: Is there any other details of the precipitation water? For example, particle concentration and size distributions could some indication (rather approximate) of the number of primary ice particles per ml of water. Some correlation with the concentration of particles could be expected.

Section 2.2.: This section launches straight in to a listing of the considered parameters. Some short introduction might be helpful to give some context.

Section 2.2.: For the "source region score" and "season score", the text (specifically page 4 line 20) implies that numbers other than 1, 2, 3 and 4 were also considered. If so, it might be worth explicitly stating so and also stating the range of numbers considered.

Section 2.3.: Does the exclusion of 'zero' samples not cause a bias towards higher

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values? Did you consider trying the same fitting process with, for example, random concentrations 0 <> 0.21 INPs-8 ml-1?

Section 2.3.: For the multiple linear regressions, were many other models considered? What about other possibly intuitive combinations? Maybe some of the less favourable models could be included in Table 2.

Section 2.3., page 5, lines 10-12: You should consider providing more detail for the concepts in both sentences for the less statistically-minded reader.

Section 3.1., page 6, line 12: it would make it clearer for the reader if you explicitly state why the weak correlation suggests the washout of INP is more important.

Section 3.2.: Could the absence of one of the seasons in the validation data have any effect on the performance of second model?

Section 3.3.: I felt this section ended a little prematurely. Maybe the implications of this could be discussed. Would this imply that the 10 L-1 concentration requirement is too high, or is there some other factor that means the observations are below this level?

Section 3.4.: I am not sure what the extensive listing of various INP sources and literature adds to the argument. Consider removal or replacement with an appropriate review paper.

Section 3.4., page 8, line 2: Please explain what the several multi-day sampling campaigns were. Is this the validation data?

Section 4.: The conclusions is, I feel, too brief. I came out of this feeling that I know why you did the work, but not why I should care about the result. Please add a summary at the beginning, and provide some conclusions with regards to the models. The second half of this section (lines 15-24) reads much more like it should be at the end of the introduction – either move it there or make it more relevant to the work and results presented. The only real conclusion presented is not very impressive (line 14) – the main outcomes of the work need to be better stated and some implications.

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Table 2: Consider adding the actual fit parameters to the table. I'm not sure that you should include the linear-scale values, as the proportionality of these depends on where you are in the log scale - taking the log-residual of something at 10^3 and then re-linearising relative to 10^0 it does not give the actual linear value of the residual.

All figures: please add error bars.

Figure 1: For the classified data it might be helpful to add the number of data points per class somewhere. Could the number of datapoints in any class be high enough to cause any potential bias to the fits? Is the data from year 2 included? If so show it with a different symbol (add it if not shown). This would help with the discussion in section 3.2.

Figure 2/3: suggest adjusting the symbols such that the paper can be printed and understood in black and white.

Figure 3: This figure isn't really discussed in the text. I would consider either expanding its presence in the text or removing it.

Technical Comments:

Page 3, line 27. Suggest insert "of" before CO, and changing "values" to concentrations, and deleting "the concentrations of" from the middle of the sentence: "...hourly concentrations of CO and total reactive...".

Page 4, line 14: insert a comma after "season score".

Page 5, line 41: Citation for Wright et al is misspelt.

Page 6, line 33: Delete "the dots for".

Page 7, line 15: Please re-write the sentence so that the paragraph does not start with the word "Thus".

Page 7, line 18: Delete ", also for validation".

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Page 7, line 37: “DeMott”, not “De Mott” (please check elsewhere as well).

Interactive comment on Atmos. Chem. Phys. Discuss., doi:10.5194/acp-2016-108, 2016.

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