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Atmospheric Chemistry and Physics Discussions

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 #2

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Stopelli et al. analyze a two year time series of ice nucleating particles collected from falling precipitation at the Jungfraujoch high altitude observatory. The data are analyzed using simple correlation and multi-linear regression analysis.

I read the paper with great interest. Understanding atmospheric variability of 'warm' ice nucleating particles is important and the factors that control their abundance are poorly understood. This paper presents some new insights on the subject. However, the multi-linear regression models do not help the community to glean widely generalizable conclusions. They are location specific. Their ability to explain the temporal variation in the data does not rise beyond an order of magnitude estimate.

The conclusions are overstated. This work shows that there is some statistical cor-

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rleation between INP @-8C in precipitation and temperature, fv, wind speed, source region, N500, and source region. Understanding the uncertainties in the data, the physical mechanisms driving these correlations, and the covariance matrix between the identified parameters is needed. Any claim about the role of INP in precipitation and impact on the water cycle is not supported by this work needs to be removed from the paper. The authors need to more clearly articulate the utility and limitations of the proposed statistical models.

Notwithstanding a report on the inferred correlations merits publication to guide the experimental design of future studies.

Comments: The dynamic range of the measured signal log(INP @ -8C) is ∼2.5 orders of magnitude, ranging from ∼-0.5 to ∼2 in Fig. 2. The majority of data fluctuate between -0.5 and 0.5, and so do the residuals of the model. The statistical model is successful in identifying the high INP episodes Jun/Aug 2013 and May 2014. Other fluctuations seem to be mostly noise in either the data, the model, or both. I have several question regarding Fig. 2 and the model. (1) What is the uncertainty in the concentration (temperature) data? Were frequent pure water tests run to determine the measurement noise? (2) INP spectra can be steep functions of temperature. How do small errors in temperature propagate into INP @-8C? (3) It seems that the calibration time-series for the model is at higher resolution than the data? (Trying to count green and black dots and get different numbers). (4) How does measurement noise, in principle, affect the multi linear regression analysis? Residuals can be interpreted as either measurement error or insufficient number or assumed physical relationship of the proposed explanatory variables. How can these be separated?

The highest INP were observed for very high wind speeds (60-80 km h-1). Were precautions taken that resuspended snow and wind-blown aerosol did not contaminate the measurements?

Even if the source is local, how could local vs. long-range sources be distinguished?

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It is difficult to foresee all the possible cross-correlations. One of the strongest predictors for high INP seems to be temperature (even though the Rˆ2 is low). Values of log(INP) > 1 only under the condition that temperature is at 0C. Thus the ability of the model to guess the high INP events is to guess when it is warm, which correlates with season, source region, RH, N>500. Assigning explanatory power to any of the variables in terms of a physical mechanism is likely erroneous.

It is still unclear to this referee what the potential application of the statistical would be. A parameterization for a model? Improved process level understanding? Neither approach would seem promising to me without much more analysis. Perhaps the authors have another use in mind?

Relating INP in precipitation to INP in air, and INP in air feeding the cloud, is problematic due to a wide range of known artifacts. These include capture of INP by falling rain snow and chemical or physical modification of INP by the precipitation. None of these issues are acknowledged in the manuscript. Even if the explanatory power of the model were perfect and the intrinsic measurement error of LINDA was zero, what relevance do INP @-8C measured in precipitation have to clouds over Jungfraujoch?

The discussion in Section 3.3 should be removed since they discuss very specific cloud types, updrafts and meteorological conditions. It is entirely unclear to what extent the INP @-8C at Jungfraujoch have any role in precipitation without considering the larger scale meteorological forcing, including liquid water content, cloud top height, updraft velocity, the INP spectrum at warmer and colder temperatures, the efficiency of the competing warm rain process, and the cumulative ice phase history, including possible seeding by wind-blown ice crystals along the storm trajectory, including all mountains upwind. No information can be gleaned from this work about any of these processes.

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