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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Rainfall drives atmospheric ice nucleating particles in the maritime climate of Southern Norway" *by* Franz Conen et al.

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Conen et al. submitted a manuscript for review titled "Rainfall drives atmospheric ice nucleating particles in the maritime climate of Southern Norway." The manuscript compares 15 months of measurements of ice nucleating particles (at -8C), two molecular tracers (arabitol and mannitol), and rainfall data to present observations about INP behavior. The authors suggest that INP were likely to have local sources and are linked to rainfall, because of the evidence that INP concentrations correlated with rain. Further, they state that correlations with molecular tracers suggest INP "may consist largely of fungal spores." The manuscript presents interesting environmental data linking warm

temperature INP with rainfall and two commonly used molecular tracers. The region sampled is also not over-represented in literature and so provides some atmospheric perspective on this region, including possible parallels with other similar regions of the world, as mentioned. In general, I support the publication of this manuscript, but there is some work that I suggest be done before it is accepted. The analysis and evidence that the observed INP are fungal in nature are both relatively thin and should be improved. I list some suggestions for specific additions below, including some possibilities for added discussion and some suggestions to add to quantitative evidence. These statements are meant to suggest possible areas of improvement, but are not necessarily meant to be comprehensive.

General comments: Abstract: "INP(-8) correlated positive with the fungal markers arabitol and mannitol, suggesting that INP may consist largely of fungal spores." I think the confidence implied by this conclusions is somewhat over-stated. The evidence shown suggests to me that the INP have a source that is at least correlated with arabitol and mannitol, but this does not necessarily mean that the INP are spores themselves. The observations could also be explained if INP and fungal spores are co-emitted by a similar process or are somehow physically related to one another. A lot of evidence has suggested many fungal spores are not IN-active (e.g. Iannone et al., 2011) while other (i.e. rust spores from the cited Morris et al. paper) are IN-active at high temperatures. There is enough complexity in this conversation, that I think some discussion of these differences should be mentioned and the overall confidence in the implications that fungal spores are the source of INP should be scaled down a bit.

The discussion about molecular tracers should also be extended somewhat. For example, arabitol and mannitol are commonly used as tracers for fungal spores, but not without complications. One important thing to mention here is that these specific tracers are typically used as tracers for wet-discharge spores, but only poorly relate to other types of spores (i.e. dry discharge spores like Cladosporium that can be ubiquitous and a large fraction of spore mass). How might this understanding impact the conclusions that are being drawn here? I am aware that the general knowledge linking these tracers with ice activity is low, and so it is unreasonable to require any kind of a quantitative link between known ice fungal ice nucleators and the amount of arabitol or mannitol they release, but I suggest at least mentioning some of the uncertainties that come along with the analysis and assumptions as presented.

In general, I think Section 3.2 and Figure 3 need more detailed discussion and explanation to help a reader not experienced with this type of analysis. Can you explain what the z-scale implies from this figure and how it relates to the brief observations you make? It's hard to know how much to make of the summary observations reported. How much of this is a function of different averaging times that may lead to random differences? If this is an important piece of evidence, is there some statistical treatment that can be applied here? Flipping back and forth between the comments and the figure I can follow the reasoning of the trends mentioned, but it is hard to know whether the "striking" comment (P5 L14) is stronger than I would have stated – at least having briefly looked at the differences. If the authors are confident of the strong difference, I suggest improving the evidence for that distinction. In contrast to this, however, the last sentence in Section 3.2 essentially says that the authors think the effects are local, which implies to me that Figure 3 should provide evidence *against* long-distance sources, right? This goes back to how Figure 3 should be interpreted as striking differences between high and low INP concentration.

What happens if you do correlations of the traces in Figures 2 and 5? A lot of the observations come down to qualitative comparisons of these traces, but it is hard to know what this means quantitatively. I think this is one obvious area that could easily improve the manuscript. Without evidence beyond the visual trends presented, it is hard to know how much to make of the possible co-variance. As a simple addition, I would also add the R2 value (or something similar) to Figure 4.

Looking at Figure 5, it seems that there is a one-point lag in INP behind arabitol and mannitol during approximately October and November. Do you think this is real? If so,

what might be causing this? Or is it just a figure illusion and statistical aberration.

P7 L10: The statement here is that "Since INP were no longer directly related to fungal spore markers during this period, it might be that bacteria contributed more to the total number ...". Another possibility is that the type of spores being released are of a different variety and are just less efficient at producing IN-active.

Bigg, Soubeyrand, and Morris recently published a paper reporting long-term statistical correlations between ice nuclei and rainfallin Australia (Bigg et al., 2015). I think reference to this work would be appropriate here, probably in the conclusions.

P6 L1: The authors cite Schumacher et al. as having observed a 2.5 – 3.0 um mode of fluorescent particle during 18-months of study in Finland. That paper also mentions a prominent decrease in fluorescent particles during snow-covered periods, which qualitatively matches some of the observations shown here.

Is snowfall poor at launching INP because of snow-covered vegetation and soil or also because the kinetic velocity at which the drops fall does not kick up material? Some recent papers on rainfall velocity and particle ejection could be cited and discussed here (e.g. P7 L18).

P7 L23: Are the heat treatment properties of fungal proteins the same as bacterial proteins? I think of spores as relatively robust, and so I wonder if it is possible for some fraction of spore material to remain active, whereas the fraction for bacteria goes to zero? In any case, I think the evidence for these arguments should be stronger.

P 6 L12: Another paper by Maninnen et al. (2014) shows seasonal trends in pollen and fungal spores at the boreal Hyttiala site in Finland and they also break the analysis down into PM mass <2.5 um and >10 um. While not at the same land-use type, these measurements may (or may not) be useful for broad comparison here.

Minor technical comments: P1 L10: Move placement of "probably" to "INP were probably aerosolized ..." P1 L12-14: I though this sentence was confusing and could use a revision to make the point clearer. P1 L22: snowflake is one word P4 L7 , L8, L10: “Landuse” should be two words P5 L19: Specifically mention that Tenerife is off the W coast of northern Africa

References: Bigg, E. K., Soubeyrand, S., and Morris, C. E.: Persistent after-effects of heavy rain on concentrations of ice nuclei and rainfall suggest a biological cause, Atmos. Chem. Phys., 15, 2313-2326, 2015. Iannone, R., Chernoff, D. I., Pringle, A., Martin, S. T., and Bertram, A. K.: The ice nucleation ability of one of the most abundant types of fungal spores found in the atmosphere, Atmospheric Chemistry and Physics, 11, 1191-1201, 2011. Manninen, H. E., Bäck, J., Sihto-Nissilä, S.-L., Hufffman, J. A., Pessi, A.-M., Hiltunen, V., Aalto, P., Hidalgo, P. J., Hari, P., Saarto, A., Kulmala, M., and Petäjä, T.: Patterns in airborne pollen and other primary biological aerosol particles (PBAP), and their contribution to aerosol mass and number in a boreal forest, Boreal Environmental Research, 19, 383-405, 2014.

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