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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Fluorescent Biological Aerosol Particle Measurements at a Tropical High Altitude Site in Southern India during Southwest Monsoon Season" *by* A. E. Valsan et al.

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This manuscript presents measurements from a 5 month study of fluorescing biological aerosol particles (FBAP) at an elevated, moderately remote site in southern India. The data quality appears to be very good and the scientific significance is potentially high. The problem is that the paper is probaby twice as long as it needs to be and the conclusions drawn are much too speculative with not enough concrete statistics to support the hypotheses put forth regarding either the source of the FBAPs or the physical mechanisms underlying the observed trends.

When discussing the results, the objective is to highlight the main points without trying to describe each and every peak and wiggle. Let the Tables list all the statistical details

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and focus on the most important variations that will then be used to drive the discussion. At the moment there are probably twice as many pages of results and twice as many figures as needed.

What is missing:

1) A topographical map of the research site and surrounding area, preferably something like a Google Earth rendition that would show the surrounding vegetation as well as areas with no vegetation such as is referred to in the text. Given the very low wind speeds, it is likely that most of the trends that are seen can be linked to more local sources.

2) A more in depth analysis of the periods with rain, taking a much closer look at the properties of the FBAP just before, during and after each event.

3) A cross correlational analysis between the FPAB properties and the meteorological conditions. One would not necessarily expect a one to one correlation between the meteorology and the FPAB concentrations, particularly if the spore production is local. Only for advected aerosols would they be linked one to one.

4) It is impossible to compare the shapes of size distributions when they are on different figures. Since the concentrations are quite different, normalize them to a unit area then put only the means or medians on the same figure so that any differences in the shapes can be clearly seen. These differences can then be evaluated quantitatively and linked to the hypothesis.

5) The backtrajectory analysis is useless as currently given. There needs to be an analysis of not only where the air came from horizontally, but vertically as well. Questions to be asked: a) How long had the air been close to the surface before reaching the site? b) What were the meteorological conditions along the trajectory, i.e. precipitation and humidity and c) How long had the air been close to the site? These all will impact the history of the particle as well as the air and whether particles had been removed

after leaving their source.

6) An error analysis of the measurements. What is the expected error in size and concentration based on error propogation that no doubt has already be detailed in earlier publications. Nothing is said about FBAPs that are not complete particles but are fragments. There needs to be an estimate of the size dependent losses in the sampling system. Even with no bends, there will be diffusion and turbulence losses that can easily be calculated with Aerocalc (Baron and Willeke).

I have attached an annotaed version of the paper that includes many more questions and comments,

The paper is relatively readable, given that the first author is not a native English; however, I am annoyed whenever I read a paper written by a non-English speaker but who has co-authors that are but who obviously have not read the paper, otherwise the numerous grammatical errors would have been corrected.

Please also note the supplement to this comment:
http://www.atmos-chem-phys-discuss.net/acp-2016-265/acp-2016-265-RC1-supplement.pdf

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