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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.

Anonymous Referee #3

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General

This paper reports results of a three month measurement campaign of fluorescent biological aerosol particles (FBAP) at a high altitude tropical site in southern India. There are some unique aspects on the data. First, the marine air masses can be compared to local FBAP sources. Secondly, the campaign included long periods of heavy and persistent rain. Consequently, the authors observed a lack of correlation of FBAP with precipitation, contrary to several recent studies from other areas. The data has been presented in diverse ways that are also comparable to earlier studies. The material seems to merit publication. However, there are several issues that should be treated.

Most importantly, the paper is unnecessarily long. The authors should concentrate on the findings that are unique to this study. Although it is good to treat many facets of the data, I think the paper would benefit of focusing also in this respect. The authors have divided the three month period first into months and later to three focus periods that they call dusty, clean, and high bio. I find the latter division much more useful. I recommend keeping it and getting rid of the monthly results. Because the data is treated from many angles, many of the explaining factors and arguments are presented several times. An example is the effect of the clean SW winds. It would be good to try to collect the findings first and then treat them at once.

Specific

In subsection 3.5 on SEM images the authors state that “these images are not being presented here for any quantitative purpose and to draw any specific scientific conclusions”. Indeed, there are only a few particles shown. However, the authors use the images to support their hypotheses on the particle species. I propose either analyzing a large number of samples and particles to corroborate the hypotheses or moving the subsection to the supplement and being cautious on using them as evidence.

The measurements have been done with the UV-APS. Regarding the data interpretation, it would be good to acknowledge and point out that the detection efficiency for fluorescent particles of the UV-APS is low especially below approximately 2 microns (e.g. Healy, at al., ACP2014, Saari et al., AST 2014). This mostly affects the reported fluorescent particle size distributions. Further, it would be good to state whether a zero check cycle for the instrument was used.

Line 250 to 273: The authors find that the fluorescent and total particle concentrations do not correlate much, independent of whether the particles are coarse or fine. They then argue that the fluorescent concentration is not affected by non-biological particles. They later hypothesize that particles from combustion or similar activities do not get transported to the measurement site. This might well be the case, but what are the high concentrations of fine non-fluorescent particles (figure S2) then? Maybe a scatter plot of NF(<1um) VS NF(>1 um) would be useful. I would expect the submicron biological particles to correlate with the supermicron biological ones somewhat. Maybe use this point for shortening the paper and just state that there seldom are major sources of fluorescent non-biological coarse particles and therefore the numbers reported are relevant. I hypothesize that the lack of correlation for fine particles is at least partly caused by the low fluorescent particle detection efficiency of the UV-APS unit.

Line 357-360: The high extreme values of NF/NT actually do result from high variations of NF, as evident from fig S2. The presented figures do not support the argued inverse correlation between NT and NF/NT. The argument should be backed with a figure or a calculation or removed.

Line 703-729: The NF axis in fig 13 is such that it is very difficult to spot small changes in concentration. However, the slightly higher concentration starts at 17.00, not at 20.00. Apparently this data does not support the nocturnal sporulation argument. The argument on humidity is later supported by the scatter plot, but it might be good to show the diurnal pattern of RH here also.

Technical

Line 75: The first paragraph of introduction is rather long. It would be good to separate the latter part into a new paragraph, starting on line 75 from " It is likely that the surface". This latter part should also reformulated, as it is very difficult to follow the line of thought now:

"surface structure, ice nucleating proteins, and other characteristics" – characteristics of what? Of PBAP, bacteria, bacterial spores. . .?

"Other bioaerosols like pollen" Other than what?

"..Other bioaerosols like pollen and fungal spores are often using air as the transport medium..". By definition, all atmospheric aerosol particles use air as the transport

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medium. Maybe: Plants and fungi use the air as a transport medium for their pollen and spores. . .

“Play an important role in public health..” It would be good to convey the idea that the role is negative.

Line 123: The authors should either explain the relevance of the present study to Indian agriculture (or vice versa) or remove the sentence.

Lines 192: The description of the drop of the detection efficiency is tautological with the text starting on line222.

Line 195 on: For particles in the size range of 15-20 microns, the aspiration and transport efficiency of the sampling system probably is a more important issue than the calibration of the APS.

Line 225: It would be good to state that the 1 micron as the fine particle size limit is ad hoc.

Line 316-318: This is plausible, but should be written so that it is clear that there is no direct evidence within the present study.

Line 476-478: This is not due to the calculation. The mode mass will peak at higher diameter than number for any atmospheric particles one could find.

Line 484-486: Although the size limit is the same, the authors should warn the readers that the mode largely absent in NT as submicron is present in MT as supermicron.

Lines 496, 659: Note that the downwards slope of the APS detection efficiency might cause a peak to appear at around 0.9 um even when the mode would actually peak at much lower particle diameter.

Line 536: Should this be figure 14?

Line 591 and elsewhere: The date format should be homogenized between the text and figures

Line 625: “As expected, the NF was highest during the high bio period..”! This should be reformulated as the period was specifically chosen to be high bio.

Line 669 on: The medians in distributions do not make much sense for channels that exhibit a high number of zero values.

Line 770 on: The pollution/concentration rose figures are hard to read and difficult to use in backing up quantitative arguments. The interpretation instruction text in the caption of fig 15 is not helpful and the numbers seem to be wrong. Overall, it would be helpful if someone came up with a better way of displaying the correlation of measured quantities and wind. Why not start with this MS?

Figures 10-12: If the median values are shown, it would be good to continue to show the mean/median legends on the figures.

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