

Atmos. Meas. Tech. Discuss., referee comment RC1
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Comment on amt-2021-401

Anonymous Referee #1

Referee comment on "Observation of bioaerosol transport using wideband integrated bioaerosol sensor and coherent Doppler lidar" by Dawei Tang et al., Atmos. Meas. Tech. Discuss., <https://doi.org/10.5194/amt-2021-401-RC1>, 2022

General comment

The paper uses lidar and ground level observations with a bioaerosol monitoring system to investigate the transport of bioaerosol in a measurement site in China. The analysis is performed on three case studies. The topic is interesting and suitable for the Journal, however, there are some aspects not very clear including what is the real novelty on the paper that need to be addressed in a revision step.

Specific comments

It is not very clear what is the novelty in the approach or in the results. This aspect should be discussed and the choice to investigate a small number of cases (three events) should be justified.

Lines 48-51. These aspects should be backed up with a references. In addition, it could strongly depend on the type of bioaerosol with bacteria and fungi behaving differently from small viruses in terms of probability of attachment on pre-existing particles. I believe that this should also be mentioned.

Another aspect that should be mentioned is that the WIBS is not able to measure small particles (< 500 nm) so that the methodology used is focused on relatively coarse bioaerosol with limited potentiality for viruses for example.

Lines 294-301. Percentages of what of total particles or of fluorescent particles?

Lines 299-301. Actually the peaks in PM_{2.5} and PM₁₀ seems to be more related to the increase of the fraction of non fluorescent particles rather than of fluorescent at least according to Figures 2a, 2b, and 2c. This sentence should be explained better or corrected.

Looking at Figure 8, it seems that most of the increase seen in PM_{2.5} is actually due to non fluorescent particles. How it is explained this aspect or how this is in agreement with a transport of bioaerosol?

Lines 342-345. It should be explained better why it is believed that in this case there was bioaerosol attached to pre-existing dust and not in the other cases. A similar question arise for the lines 360-367. The difference with the previous mentioned work what that ratio is compared for aged and local aerosol could be due to differences in the local sources rather than on bioaerosol attached to dust. Please explain and comment better these points.

Line 345. Ok for diffusion, including vertical diffusion due to the increasing boundary-layer depth, however, I do not see any evidence of dry deposition in this dataset.

Line 402. See the previous comment on attached dust, if this is not the only explanation please be more cautious in interpretation.

Line 28. Probably it is better to say long-range bioaerosol transport.

Line 330. Use an apex for m s^{-1} .