

Interactive comment on “Systematic Characterization and Fluorescence Threshold Strategies for the Wideband Integrated Bioaerosol Sensor (WIBS) Using Size-Resolved Biological and Interfering Particles” by Nicole Savage et al.

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This manuscript presents a very large set of laboratory observations of different kinds of fluorescent aerosol (both biological and non-biological) using a WIBS 4A, presented in the context of a recent analysis framework. The authors use this dataset to evaluate the ability of the WIBS to detect a variety of biological aerosol, to characterize the observed response in a particular instrument and to make recommendations for excluding

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common interferents. They have also extended the utility of the analysis framework by systematically investigating the effect of size on the fluorescence response for a given bioaerosol population and have additionally evaluated the performance of the asymmetry factor parameter, an output which is often used but which is of unknown value in distinguishing different types of particles.

The paper is well written and the community is sorely in need of this kind of characterization and critical analysis of performance if we are to make robust measurements of atmospheric abundances of bioaerosol. Questions of potential interferences are one of the largest hurdles in the use of UV-LIF technologies and this paper is a valuable piece of that puzzle. I have a few comments and suggestions as outlined below for the authors to consider but I certainly recommend publication in ACP with only minor modifications.

Specific comments:

1. On p5, I'm not totally sure how you guys are doing the calibration but I think you should probably include a bit more detail. Did you just run a few sizes of PSL and then fit with a 2nd order polynomial? Was there any consideration of the expected instrumental response given Mie theory? I have run some calculations of expected response and compared that to PSLs and usually get reasonable correspondence but I'm not sure than a 2nd degree polynomial is sufficient to capture the expected shape of the response. Admittedly any differences are likely at the larger sizes and probably don't impact the results much but size is one of the parameters that is used heavily and there seems to be wide variability in how it is treated. Most critically the size you are reporting is not simply the size the WIBS reports based on its internal calibration but is instead based on the observed peak heights and calibrated by you using multiple PSL sizes. I think this point could be made clearer as many WIBS users seem to still use the WIBS internal calibration, simply checked periodically with one size of PSLs. 2nd order polynomial extrapolation to larger sizes than are represented by PSLs are an additional uncertainty.

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2. Can you include a statement and/or reference for how representative these chemically-produced “brown carbon” compounds are of atmospheric brown carbon? This may be addressed in the Powelson reference and you do discuss it a bit later in the paper, however it would be useful to have some discussion of this in the methods section when brown carbon is introduced. I.e., we know it’s a surrogate but it’s the best option we have. We expect the absorption spectrum is similar but the cross section is different by. . .

3. Initially it took me a while to figure out what you meant in the text and figures by “miscellaneous particles”. Although the samples are delineated in the table, it might be better to relabel “miscellaneous particles” as “common household fibers” or something more descriptive for ease of reading.

4. I think it is worth explicitly noting somewhere in this manuscript that all of the populations sampled are fresh samples and we do not know how atmospheric aging would impact our ability to detect ambient bioaerosols. It is a necessary benchmark to understand what the fresh emissions would look like however we do not know how the fraction of particles detected would change over time so this may not perfectly reflect (would be a best case scenario of?) our ability to detect ambient particles.

5. I think the nuances of what you are seeing with the dust is critically useful and I would like to see a bit more context for these numbers and more detailed discussion of the different samples rather than lumping them all into a “dust” category. The expectation is that dust, by number, is much more abundant than bioaerosols such that, even if only 1% of a certain population of dust is misidentified, it could be a huge number relative to the abundance of bioaerosol. I suggest expanding the discussion of the dust to include where these dusts are from and whether you have any idea about how abundant these different kinds of dust are in the atmosphere. Is it possible at this stage to put bounds on how much dust may impact WBS measurements in different environments?

6. The suite of particles investigated is impressive and I can appreciate that it is not

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reasonable to discuss each individual particle type in detail. However, similar to the above comment, I think the current discussion is a little bit too case-study oriented and would benefit from a bit more distillation/bigger picture. I found myself wondering how representative Hulis 5 and the 15% fluorescent dust particles are of those populations. This is already addressed somewhat but I recommend expanding the discussion or possibly adding a section specifically about implications of known interferences on ambient measurements.

7. It seems that these results are fairly consistent with the Hernandez et al findings except for a couple of things. First, there are a lot of non-fluorescent particles in several of the pollen samples if I'm reading the supplemental graphs correctly. This is surprising as we have always found nearly all pollen particles in a sample to be fluorescent in previous analyses (i.e. the Hernandez paper). It's a little hard to see it in the Hernandez paper but, if you add up each row in Table A1 (which shows the percentage of a given sample that showed up as a particular type), they don't quite sum to 100% and, for at least those pollen samples, we had >95% of all particles detected as fluorescent. So I am surprised to see so many pollens with a large non-fluorescent contribution here. Second, in Hernandez, the type B presentation was at most a minor (<10%) fraction of particles for a given population and even that only appeared in a handful of biological samples (for two different instruments). Here it seems that many of the pollen samples have a substantial fraction of particles manifesting as type B. This is unfortunate as it seems that type B is often also found in possible non-biological interferences. Have the authors thought about what might drive this kind of variability? I suppose it could be specific to certain pollen species, it could be instrument variability or it could be something to do with the samples or nebulization but this probably deserves a little discussion.

8. The discussion of the size dependence of fluorescence is nice. I think it would be worth double checking that there is not a size-dependence in the FL2 detector for non-fluorescent particles. I think there was a batch of bad notch filters at some point in

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WIBS production that led to some bleed through of flash lamp light to that detector. This may be somewhat hard to assess given that some PSLs have a fluorescent surfactant (and thus “normal” non-fluorescent-doped PSLs will sometimes fluoresce) but it can be done with dioctyl sebacate or AmSO₄ or any other non-fluorescent material (which need not be mono disperse).

9. I appreciate your discussion of the asymmetry factor and the potential problems with it. On lines 726-727 I believe you meant to say that the forward-scattering detector may not be able to reliably estimate either size or AF? I also think you could give at least a hint at your ultimate conclusion about the AF measurement in your initial discussion of this measurement and, possibly, in the abstract. On my first read-through, after seeing the AF calculation in the text and the AF values included in the table, I thought you might not examine that parameter critically. Just something along the lines of “The performance of the asymmetry factor is assessed across populations as a function of particle size.”

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