Atmos. Chem. Phys. Discuss., 9, C6109–C6119, 2009
www.atmos-chem-phys-discuss.net/9/C6109/2009/

Atmospheric Chemistry and Physics Discussions

Interactive comment on "Measurements of coarse mode and primary biological aerosol transmission through a tropical forest canopy using a dual-channel fluorescence aerosol spectrometer" *by* A. M. Gabey et al.

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Received and published: 21 October 2009

1) Overview: Overall I think the use of the WIBS for real-time field measurement of primary biological aerosol (PBA) is highly valuable, and this paper shows some of the exciting things that can be done with this instrument. The authors discuss measurements of fluorescent PBA particles taken above and below the forest canopy at a tropical forest site in Borneo, Malaysia for several weeks. Very few measurements of PBA have been made in real-time or with similarly high time or size resolution, and so this experiment is certainly of great scientific merit. I think the results comparing differ-

ences from above and below the forest canopy and the plots showing diurnal variability were particularly interesting. I think that the reported measurement results certainly should be published, and I hope that the paper will trigger and support further work in this direction. I have several comments, however, which I hope will constructively help the authors to strengthen and clarify their messages.

2) Establishment of protocol: I think that the analysis and discussion of the measurement results is somewhat incomplete, and that the writing could be improved to help make the text more easily readable. Unless I am mistaken, I understand this manuscript to be the first to be sent for publication using ambient field data from the WIBS instrument. As a result of that, therefore, I suggest that this manuscript establish exactly how they have operated their experiment and data analyses, including discussion of protocol, reliability and uncertainties. The authors cite Kaye et al. (2005) with respect to instrumental details and cite Agranovski et al. (2005) as having employed "a similar protocol . . . with other UV bioaerosol spectrometers" and so do not discuss many details of their own experimental or analysis procedure. Both papers, however, provide only laboratory measurements, Kaye et al. utilized a "prototype" instrument, and Agranovski et al. uses a very different type of instrument (TSI UV-APS) that uses one UV wavelength and determines particle size by a very different method (via time-of-flight between two lasers, thus reporting aerodynamic diameter rather than optical diameter). In view of these points, the works cited by Gabey et al. do not adequately establish all the details that should be specified. Because this is the first time that the WIBS has been operated in a field setting for measurement of biological aerosols, I would like to see more detailed discussion of both measurement and analysis procedure.

3) Diameter measurement uncertainty: The authors report the "optical equivalent [particle] diameter" in this manuscript (Page 18969, Line 11). Along the lines of the previous paragraph, I think it is important for the authors to detail exactly how they achieved this measurement, since the Kaye et al. paper that they cite only peripherally deal with

quantifying particle size. Kaye et al. states the following in Section 2.4: “Particle sizing by this method is approximate only since the magnitude of scattered light received by the FL2 detector will be a function of particle shape and refractive index as well as size.” Kaye et al. further go on say: “For the reasons mentioned in 2.4 above, no attempt has been made in this preliminary data to convert the recorded particle scatter signals to a spherical equivalent particle size though the scale extends approximately from $\sim$0.5 to 10 um.” How are particle shape and refractive index dealt with in terms of particle sizing within the WIBS data analysis? How does this optical diameter compare with other particle diameter metrics (e.g. physical, aerodynamic, etc.)?

Moreover, the Kaye et al. paper discusses the WIBS-2 instrument while the Gabey et al. paper discusses the WIBS-3 instrument, but does not mention what (if any) significant differences might exist between the instruments. In general, I think it is important to clearly discuss how particle size is quantitatively achieved with these optical parameters, including a short discussion of what the uncertainties are (sources and magnitudes). I don’t doubt that the problems of quantifying particle size by this technique are surmountable, and the Kaye et al. manuscript further reports that they were at that point working on these issues. No mention of this or any further work is cited by Gabey et al., however.

Again, since this paper establishes the WIBS technique for use in ambient field study, I suggest that it would be helpful to specify and discuss details related to how the authors calculate particle size from the measurements, including what uncertainties exist and their magnitudes.

4) Determination of PBAP: The Gabey et al. manuscript also left me wondering exactly how the determination of PBAP is made with this instrument, and what kind of uncertainties exist with that measurement. The text states on Page 18970, Lines 3-5 only: “It was assumed that PBA are the only fluorescent aerosol in the coarse mode and that to be recorded as such, their fluorescence must exceed an instrument-defined minimum threshold in either channel.” I understand that UV wavelengths were chosen

for this instrument specifically to select biological molecules within the particles (e.g. tryptophan and NADH, etc.), and that this assumption is impossible to rigorously prove in-situ. Is there any chance, however, that there are PBAP that are not being detected by this technique or that any non-biological particles might be considered PBAP here? I think it would be helpful for the authors to mention possible sources of interference, if nothing else so the reader is able to consider additional possibilities and so that the reader can make an informed decision of how to mentally approach the reported data.

Further, the manuscript makes no mention beyond that “instrument-defined minimum thresholds” exist (Page 18970, Lines 4-5). Are these set at a conservative level to avoid introducing positive interference from non-biological material, or do they have other possible uncertainties that might be included? This is a very important point, and I think that discussing this procedure in more detail would make the understanding of the reported data much stronger. Have previous works dealt with this issue in detail?

The manuscript says on Page 18970, Lines 9-10 that instrumental errors contribute to “an overestimate of PBA number that is 2-3% of the total measured number,” but the authors to not discuss in detail the level at which these numbers will directly contribute to PBAP uncertainties. How much relative error in PBA number does this introduce? Are there other significant sources of error or uncertainty in the determination of PBAP? Would it be possible to show a box/whisker plot of the number fraction (diurnal, or average for this period, or both), and then discuss how the 2-3% spurious fluorescence contributes to error (or is insignificant). This is an example of further analysis that should be easy to perform and would show the data in a very clear way. Including this would significantly add to the strength of the manuscript, in my opinion. Once this is done, it would also be easy to compare the fractional number concentration, PBAP number or PBAP mass to what other studies have published.

I also think that the confident determination of “PBAP” from this measurement could be considered somewhat bold. I have no doubt that the measured particles are very much related to the overall PBAP concentrations. However, I would also think that there could

be some biological material that may not be detected as efficiently or that the WIBS might detect some fluorescent material (even in small concentrations) that is not of biological origin. I would suggest being very careful (i.e. conservative) with the scope of the authors claims about PBAP, even if it is likely to be mostly true. Minor uncertainty in the determination of PBAP without forthright discussion could make the use of "PBAP" in this case potentially confusing. Partially as a response to this issue, Huffman et al. (2009) recently reports particles that cross a fluorescent threshold in the UV-APS as being "fluorescent biological aerosol particles (FBAP), which can be regarded as viable bioaerosol particles representing a lower limit for the actual abundance of PBAPs." While this terminology still may not adequately convey the appropriate message and was written with respect to the UV-APS that uses only one excitation laser (and thus is likely only able to detect viable PBAP), I think the concept of carefully framing the discussion of measured PBAP is still worth considering in this case.

5) Morphological Parameter: The asymmetry factor (AF) discussed here, which relates information about morphology, is very interesting and is in my opinion is one of the advantages that the WIBS may have over some other fluorescence spectrometers used for bioaerosol measurement. The concept, through which it is applied however, is not thoroughly introduced. The text says on Page 18969, Lines 14-18: "... the fractional standard deviation between the quadrants parameterizes particle morphology; a low standard deviation indicates a more symmetrical scattering pattern, normally produced by a spherical particle. This morphological parameter is known as the Asymmetry Factor, AF, whose distribution is intrinsically log-normal." It is then not discussed again until briefly in Section 5.3. How does this AF relate to the dynamic shape factors commonly used within the atmospheric aerosol community? Are there other aspects beyond particle shape that are related to AF? What AF value would a spherical particle have, for example, or what range of AF values might one expect within ambient PBAP? This is a key point in the overall message of the paper, but the authors do not discuss morphology in terms of what it means for the particles or how it relates to existing research.

6) Text: In general I think that the descriptions within the text are sometimes short and either vague or unclear. I suggest lengthening the descriptions of many areas of the manuscript so that the reader has a more detailed understanding of how the experiment was performed, and also what scientific conclusions the authors are drawing. My impression is that the authors may see the manuscript as a short communication letter, but I would prefer to see more detailed analyses of the experiment published in one place. Some specific examples are suggested below as optional ideas.

7) Introduction / Citations: Also, the authors use relatively few citations (13) within the entire manuscript, and the introduction is quite short. Little context is, therefore, given for what has already been done in this area. Probably a dozen groups, if not more, have developed bioaerosol fluorescence spectrometers and utilized them in a variety of environments (from detailed lab characterization, to specific outdoor environments heavily polluted with bioaerosol, to ambient urban background, etc.), and research groups have been measuring PBAP concentrations and properties by a whole host of techniques for decades. I would suggest expanding the introduction and citation list with these things in mind. In general the introduction is informative, but does not flow smoothly and could be made to link more effectively with the rest of the manuscript. For example, the paragraph beginning on Page 18967, Line 3 talks about fungi and dynamics within the forest canopy, but the text never returns to directly discuss any of the ideas introduced here.

8) Figures: I also suggest that the figures could be optimized for the ACP/ACPD format. Most importantly they should be in color, since publishing costs are the same for color or black/white in this journal. This would make many of the graphs much more easily readable. Many of the figures are also very small, but hopefully this should be easy to make better in the revised version.

9) Conclusion: Most importantly, I would like to help to the authors produce the highest quality manuscript. I think the WIBS instrument represents a great opportunity to measure PBAP in the field and to rapidly gain detailed information about PBAP sizes

and properties. I think the experiment of operating the WIBS to measure PBAP above and below the canopy of a tropical forest is extremely interesting, but I think that the manuscript could be somewhat improved before it is finalized in ACP. In particular I think the manuscript could be improved in its discussion of: (1) how results were calculated, (2) citations to previous works, and (3) mention of uncertainties of this technique. These improvements would help the paper be even more useful to the community as an "early" WIBS reference. I do not intend to be combative or personally negative in any way, but would like to help the field publish the best science possible.

Additional Specific Comments:

P18966, L21: PBA has always existed (i.e. natural) and so this can't be the stated reason there has been renewed interest in PBA measurement within the last 15 years. This sentence makes little sense as written.

P18968, L4: The writing here is a bit too short on words and is therefore a bit vague. I'm assuming that the author is discussing the topography of the measurement site by simply saying "at a ridge".

P18968, L8: If the instrument was located on the ground in both scenarios, how is it above the canopy in one location? Won't the tree tops (and therefore the canopy) still be above the instrument at both sites, or was a tower used in the second location? I'm a little confused, and I think it would be helpful to clarify this in more detail.

P18969, L13-16: This sentence is confusing to me. I suggest discussing the summation and calculation of optical size in more detail (or at a minimum a reference needs to be given). In addition, "a low standard deviation" doesn't immediately make sense. First I would probably add "value" to that sentence. Second, I suggest adding more detail to clarify how this works. This sentence also could use a reference to prior work somewhere, and might even benefit by breaking it into multiple sentences or possibly even its own paragraph to make absolutely sure the reader understands these key points.

P18969, L17: This morphological parameter, is it the "standard deviation" or sigma? This is confusing.

P18969, L21: The co-location of an OPC is important information, especially for an instrument that hasn't been rigorously compared to other standard instruments, but it isn't discussed again in this manuscript. It's not crucial, but that the particle concentrations were "confirmed in-situ" is tantalizing. If possible it would be great to see a comparison plot with the OPC in the manuscript, or at least a discussion in more detail with reference to already published work.

P18970, L14-17: How much uncertainty is introduced by the fact that measurements were recorded at the different sampling locations at different times? I would expect this to increase the size of relative error bars, but this is not discussed.

P18971, L6: NTW has not been defined. If the 'W' in the subscript refers to WIBS, I suggest dropping the letter, unless other measurements are compared with where contrast is necessary. Making the acronyms and subscripts as 'parallel' and intuitive as possible might help the manuscript to be a bit more readable. For example, why is one NTW and the other NPBA? Would it be helpful for the first to be something similar, like NTA, etc.?

P18971, L12-19: Were any statistical analyses performed here? Would it be possible to show scatter plots and discuss how RH contributes to PBA release in a more statistically significant manner (or why you think they are valid, but statistics are not, etc.).

P18971, L21: "around one-quarter" – I would suggest being more specific here and including statistics if possible.

P18972, L8: Text says Fig 6(i) here, but this is doesn't seem to be true and is confusing. The sentence may need to say that this mode only occurs in PBAP (for example), but it is included in the Total in Fig 6(i) and Non in Fig 6(ii), so the reader has to process this

textual error for themselves. This is an example of a minor flaw, but one that makes the reader need to work a bit harder to get information from the text.

P18972, L12: Use of "species" here is confusing to me, within the context of the paper. I'm assuming that the author intends to say that these are other non-biological, chemical species that show up as some peak. Within the context of biological particles, however, 'species' initially makes me think of 'biological species', and I did not realize my misunderstanding until nearly the end of my first read-through. These particles are likely the accumulation mode of organics, sulfate, minerals, etc. Somehow I think the terms here need to be changed a bit to make this clearer. Also, did the authors compare with any additional co-located measurements of aerosol chemistry? This would help verify that this sub-micron mode was indeed standard non-biological molecules. It's not crucial, but if the data is available it would be great to include.

P18972, L14 (and before): I would suggest stating what peaks correspond to PBA and Non-PBA more clearly. For example, what about the peak at $\sim$6um in Non-Fluorescing, below canopy? This peak is interesting, but ignored in the text. Where does this come from?

P18972, L15 (and rest of paragraph): I think it would be helpful to describe this topic (AF) and section in more detail. It was very confusing to me, and I never fully understood how you were achieving this or what information you exactly determined from it. Can you discuss uncertainties in the optical measurement associated with the AF values stated here? Are there references that you can cite to establish who has used this technique for similar purposes?

P18973, L10-13: Can you discuss the canopy transfer efficiency in more detail? The idea of this 'E' value is only very briefly discussed, but not in enough detail for the reader to understand why the author is discussing it or what the atmospheric or biological implications of these measurements are. Please introduce and give examples of what the numbers mean in words. For example, is it significant that the canopy transfer

efficiency is $\sim$0.5 for total particles and $\sim$0.3 for PBAP? Are there any other references to canopy transfer efficiency for PBAP, or is this work truly unique in that sense. If there are references available for this I would suggest discussing them in the introduction. If not, I would then suggest highlighting the fact that these results are the first of their kind.

P18973, L24-26: Nowhere but the conclusions does the author discuss this PSL experiment. This is interesting and useful, but needs to be discussed in more detail earlier in the text.

Additional Minor Textual Suggestions:

P18966, Line 3: First sentence in abstract is a bit confusing. I would suggest adding a period after "material" and dividing to two sentences.

P18966, Line 23: Here is an example of a place where I think it could be helpful to add a citation if the existing sentence remains as is.

P18966, L23: "It is here ..." What is the author talking about here? I was a little confused at this sentence as written.

P18967, L1: "their wider" phrase is a bit awkward

P18967, L5: I would suggest adding more references here, or at least add "e.g." to this one.

P18968, L23: "July ..." Can you state specifically which dates?

P18971, L15-16: Typographical error – Incorrect paragraph start. Should be one sentence.

P18972, L7: The "modes" are a bit hard to see with only a log-plot. You might consider including a plot with linear-axis in the supplement.

P18972, L10-11: I would suggest re-writing this sentence to say specifically where

peaks are located (with values).

References:

Agranovski, V. and Ristovski, Z. D.: Real-time monitoring of viable bioaerosols: capability of the UVAPS to predict the amount of individual microorganisms in aerosol particles, Journal of Aerosol Science, 36, 665-676, 10.1016/j.jaerosci.2004.12.005, 2005.

Hufffman, J. A., Treutlein, B. and Pöschl, U.: Fluorescent biological aerosol particle concentrations and size distributions measured with an ultraviolet aerodynamic particle sizer (UV-APS) in central Europe, Atmospheric Chemistry and Physics Discussions, 9, 17705-17751, 2009.

Kaye, P. H., Stanley, W. R., Hirst, E., Foot, E. V., Baxter, K. L. and Barrington, S. J.: Single particle multichannel bio-aerosol fluorescence sensor, Optics Express, 13, 3583-3593, 2005.

Interactive comment on Atmos. Chem. Phys. Discuss., 9, 18965, 2009.