Atmospheric
Chemistry
and Physics
Discussions

Open Access

ACPD

**Interactive
comment**

Interactive comment on "Biogenic cloud nuclei in the Amazon" *by* J. D. Whitehead et al.

Anonymous Referee #2

Received and published: 16 February 2016

The paper 'Biogenic cloud nuclei in the Amazon' presented by Whitehead et al. contains a detailed compilation of different measurements during a 3-weeks intensive in the transition period between wet and dry season at a remote research station in the Amazon. The authors focused on different measurements of micro-physical, chemical and hygroscopic properties of the sub-micron aerosol particle population as well as the fluorescence of super-micron particles - a thoroughly interesting, comprehensive and significant data set. The collected data and shown results are relevant to the scientific community and contribute to a deeper understanding of the significance of (biogenic) aerosol particles for cloud properties and the formation of (mixed-phase) precipitation and hence the hydrological cycle in the Amazon.

The subject matter is clearly in the area of ACP. Nevertheless, I think several aspects concerning the data analysis and further technical issues need to be revisited carefully before the manuscript can be accepted for publication in ACP. Please find my major

Full screen / Esc

Printer-friendly version

Discussion paper

comments below.

General Comments:

The manuscript shows an interesting but brief compilation of individual data sets, which are finally compared to previous studies. Since the whole data set comprises (as stated by the authors) a large variability e.g., for the total particle number concentration (100 - 800 cm-3, cf. Fig. 2), shape of the particle number size distribution (cf. Fig. 1), organic mass contribution measured by the ACSM (0.5 - 4 μg m-3, cf. Fig. 5), one would expect to find similar variability in GF or kappa. Nevertheless, GF and kappa are mainly discussed in terms of campaign averages and the applied color scale in Fig. 4 makes it hard to identify variability. Interestingly, the time series of GF does show clear episodes of stable conditions (cf. July 22th) versus episodes with higher variability (cf. July 23rd). Furthermore, during a short event on July 15th GF shows extraordinary high values (> 1.6), which is not discussed in the manuscript.

I suggest to carefully revisit the results section towards a more systematic and comprehensive analysis and discussion combining information from different measurements (particle number size distribution, total particle number concentration, hygroscopicity and chemical information).

The authors apply a hierarchical cluster analysis to the WIBS data, which is certainly a powerful technique to identify PBAB meta-classes. However, there is significant information missing about the input to the analysis and the corresponding discussion. This paragraph is not clearly outlined making it hard to follow the argumentation.

Finally, the title is very unspecific and does not clearly reflect the content of the paper.

I summarize more specific comments below.

Specific comments:

Section 2.1:

- first paragraph: The authors compare rainfall, temperature and humidity during their measurement period with AMAZE-08. Please specify the statement 'cooler and more humid'.

- second paragraph: This paragraph deals with detailed information on the location of the measurement site. Please consider to add a map. This would also be helpful for the discussion concerning the removal of pollution episodes.

Section 2.5:

- The authors describe how they flag and remove pollution episodes from the entire data set. Last sentence: 'Approximately 28% of the HTDMA and CCNc data were removed in this way, with 5% of the data being flagged as possibly impacted by biomass burning and most of the rest due to the Manaus urban plume.'

- Why are only HTDMA and CCNc data removed? Additionally, data gaps in the shown figures have to be specified.

- I further suggest to consider to show a figure containing all geographical information including the mentioned Manaus bounding box.

Section 3.2:

- In section 2.5 the authors already introduce a 'cleaning procedure' to exclude pollution episodes. Does f_{60} show any correlation with the detected pollution events?

- p. 7, ll. 21: 'The mean f_{60} at TT34 in July 2013 was 0.19% ± 0.07%. This is well below 0.3%, which is considered to be the upper limit for background air masses not affected by biomass burning' Have the ACSM data been filtered? Is the mean value calculated after removing pollution events?

Section 3.4:

- p. 8, l. 18: 'mean total particle number concentration of FBAP ..' Do you mean the mean FBAP or the mean total particle number concentration?

- p. 8, l. 31: 'The observed night-time peak in FBAP number concentrations in fig. 7 is consistent with nocturnal sporulation driven by increasing RH' Where did you measure T and RH? Are the measurements collocated (below or above canopy) or part of the regular measurements at the research tower (if so, at which height)?

- p. 9, l. 8: '... FBAP clearly dominates the particle number concentrations for $D_p > 1 \ \mu$m, however non-FBAP concentrations are higher for submicron particles': How robust is the characterization of the WIBS instrument? I wonder if this statement might be influenced by a decrease in sensitivity of the fluorescence signal. According to Crawford et al. (2015), the WIBS-4 has a 50% detection diameter at 0.8 μm. Please specify the 50% detection diameter of your instrument.

- p. 9, ll. 13: The authors apply a cluster analysis to the WIBS data without providing details on the data preparation and the precise input. According to the cited paper by Crawford et al. (2015), several steps are involved to filter the data before clustering. Did the authors apply exactly the same criteria? Even if so it is worth mentioning those criteria and the corresponding rejection rate in this manuscript.

- p. 9, ll. 15: It is hard to follow the argumentation concerning the cluster analysis: 'Cl1 has previously been attributed to fungal spores (Crawford et al., 2014) based

on comparison with other sampling techniques and the diurnal emission pattern (see fig. 7) with higher concentrations observed overnight' Was Cl1 attributed to fungal spores based on the observed diurnal cycle (in this publication) or on the mean values (of FL1-3, AF, size) of the corresponding cluster in Crawford et al., 2014?

- p. 9, ll. 20: 'The statistical parameters of each cluster are shown in table 3 for comparison. Together, these clusters contribute approximately 70% to the total fluorescent particle concentration, with no significant diurnal variation in this figure, suggesting that FBAP were dominated by fungal spores during this study.' Why does the hierarchical cluster analysis cluster only 70% of the data? Why is there no significant diurnal variation? And why does it in this case lead to the stated conclusion?

Section 3.5.1:

- p. 10, l. 28: 'The HTDMA derived κ from the Borneo experiment shows more hygroscopic aerosol than in Amazonia, as discussed above, however the CCNc derived values are more in line with those in Amazonia. This discrepancy has been noted previously and possible reasons for it discussed by Irwin et al. (2011) and Whitehead et al. (2014).' It would be interesting to discuss the findings of the mentioned papers in the context of the here observed discrepancy.

Section 3.5.2:

- p. 11, l. 7: 'The median number concentration of FPAB observed below the canopy in this study was 372 l−1'. Unprecise – which study do you mean, Gabey et al. (2010) or this study?

- Concerning the observed discrepancies with Huffmann et al. (2012), the authors discuss instrumental issues, mixing effects related to strong vertical gradients

and pbl development. I suggest to add a discussion about possible effects of wet deposition, since the measurements of Huffmann et al. (2012) were performed during the wet season.

- p. 11, l. 28: 'Diurnal variations between this study and that of Huffman et al. (2012) were similar, however Gabey et al. (2010) reported an additional increase in the afternoon in Borneo'. Unprecise – which measurement parameter increases?

Technical issues:

Please reference all your physical variables in the text and/or figure captions.

Please do not use abbreviations like 'don't' (e.g., p. 11, l. 32).

Figure captions miss significant information:

Fig. 1:

- information on the derived GF and kappa is missing

- HTDMA, CCNc data comprise different measurement periods. Please specify that in the figure. Are these data averaged over the same time period?

Fig. 2:

- N_{CN} - is this measured by the CPC or integrated from the size-resolved measurements?

- please specify the data gaps

Fig. 4:

- please specify the data gaps

- all other figures use GF(D/D_0) instead of 'Growth Factor D/D_0'

Fig. 5:

- please specify the data gaps

- The unit is probably μg/m3

- What is the collection efficiency for the ACSM data?

Fig. 6:

- N_{tot} refers to the size range of the WIBS, make sure that there is no confusion with the term 'total counts' in Fig. 2

Fig. 7:

- N_{tot} refers to the size range of the WIBS, make sure that there is no confusion with the term 'total counts' in Fig. 2

- please add information about the sensor height and position for T and RH

- $°C$

Fig. 8 a & b:

- you use Dp instead of D_p

- unit of dN/dlog dp is wrong

Fig. 9: 'Irwin et al., (2011)'

References:

- page 16, line 15: lower case initials: 'Wiedensohler, Arana'

- page 17, line 3: full name instead of initials: 'Anna Stefaniak'

_____

Interactive comment on Atmos. Chem. Phys. Discuss., doi:10.5194/acp-2015-1020, 2016.