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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Temperature-dependent accumulation mode particle and cloud nuclei concentrations from biogenic sources during WACS 2010" *by* L. Ahlm et al.

Anonymous Referee #1

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This manuscript details a very comprehensive measurement campaign WACS2 in Western Canada, with main scientific goals related to extensive characterization of organic aerosol. I find the methodology, writing, hypotheses and overall level of scientific quality to be generally very good. There are some techical questions.

The main methdological problem in all campaign datasets is the representability of the datasets. The length of the measurement campaign was slightly over 1 month, a reasonable period for some short time analyses, but making the presented correlations slightly less reliable, a feature which could require some additional analyses.

The instrumentation is very impressive, and the interactions between different datasets

are clearly defined.

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My only methodological problem is the use of correlations. First of all, they assume linear behaviour. This is neatly shown in Figure 10 for NCCN and T, but do such behaviour exist for other properties as well? There is no need for extra figures, but a comment for using linear correlation could be useful. Related to this, fig 10a-b could be also (noisy) exponential, but I agree that there is too little data to make such conclusion. Also, use of coefficient of determination ("r2") could be more consistent with other literature.

The main problem with correlations are, that only R value gives quite low degree of understanding on the processes included. The number of data points is not clearly defined (although could be deducted from the campaign length), and there is no indication of the level of autocorrelation (autocorrelation: corr(x(t),x(t+dt))) between the measurement points. The autocorrelation is significant, as can strongly affect how important different correlations are. The text uses now very qualitative comments on the correlation significance, which is not necessarily a bad thing, just leaves the reader to think how these are derived.

For quantitative understanding of how "significant" * the results are could be determined by calculating some kind of confidence interval for the correlations given. This is needed in this article, as some of the conclusions are based on lower and higher correleations between different properties. Without any analysis, a R=0.3 and R=0.5 might actually be both completely insignificant (or both highly significant) and thus their difference might be either insiginificant or significant.

A word of warning: it is quite important to understand the role of autocorrelation to the correlation, as two strongly autocorrelating timeseries can easily give larger correlations between each other than non-autocorrelating ones. In (perhaps) more clear terms: If one calculates a classical t-test p-value for a correlation, the p value will most

likely be strongly underestimated if the two timeseries are autocorrelating.

For a methodology to do this, I would refer to some time series analysis book. I have used M. Mudelsee, Climate Time Series Analysis, Springer 2010 (ISBN 978-90-481-9481-0) and there algorithm 7.1. However, I leave the details (or even inclusion of this analysis) to the authors to include as they will. The key point, which I think will improve the level of discussion in this paper is to get some sort of quantitative level of understanding CAN one say anything about the differences in correlations. Please note that traditional p-values given most correlation coefficient routines (e.g. Matlab corrcoef) do not directly give reasonable values due high autocorrelation of datapoints.

* (the quotes are just to point out that all significance is up to the scientist to determine - not a comment on the significance of the results here)

Minor comments

pg 27994 - I assume LT has daylight savings time included? - When did the sun rise in the site? Approximate time is enough. This has some implications on understanding fig 8 and associated discussion.

Pg. 27997 - The authors refer to Mcdonald et al ACPD paper, which is at least in the references still in prepararation. I would like to ask them then to include slightly more information on the periods chosen. Eg. cooler, warmer, etc should be more detailed on what was the actual temperature range. One way would be to somehow include temperatures and precipitation to fig2, altough it seems to be rather full as it is.

Pg 27999 Please detail MKV/MACR

Pg 28005 Sect 3.4, line 23 on. The authors discuss some of the peaks in concentration and discuss their similarities and differences.This corresponds to period 3, and I would like to know more in detail how much of the speculated anthropogenic influences could be seen from the trajectory analysis as well. Additional comment is that the authors very carefully analyze each minor peak of the time series. Considering the usual vari-

ablity of any atmospheric property, especially over a mountain site, I would perhaps not overanalyze each detail.

Pg 28008 When you say that combustion and N values correlate, which values do you use for N? Averages of the same periods?

Pg 28009 I think the correlation between T and CCN is very interesting indeed. However, could this be happening from e.g. similar advection patterns? Could we get similar behaviour just from observing updrafts from valley below, which would typically have higher T as well as higher NCCN? Also, higher temperature could indicate higher updraft, and thus more influece from below. Please discuss.

Figures:

- I find it very useful that the authors have included the periods on all figures: this is a very nice addition and makes reading the figures easier

- Fig 4: It might be good to include also your chosen labels for the factors in this figure (detritus, etc)

- Fig 9.a), sometimes it might be better to plot the aerosol concentrations in log-scale, to avoid the need of quite non-linear colour scale. However, the figure is perfectly readable as is. Authors mention earlier that some indication of biomass burning was evident in last days of the measurements. Are they seen in this figure somehow?

Interactive comment on Atmos. Chem. Phys. Discuss., 12, 27989, 2012.