Atmospheric
Chemistry
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Discussions

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Interactive comment on "Polyols and glucose particulate species as tracers of primary biogenic organic aerosols at 28 french sites" *by* Abdoulaye Samake et al.

Anonymous Referee #4

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Comments on "Polyols and glucose particulate species as tracers of primary biogenic organic aerosols at 28 french sites" by Samake et al. This is a rich manuscript pooling together datasets from 28 sampling sites across France, focusing o polyols and glucose in the atmosphere. I find the manuscript to fit the scope of the journal, and to be generally well-written. I do have some fairly significant concerns about the analysis and technical comments which should be addressed prior its publication.

General comments:

1. I find the title misleading, as there is not really use of polyols and glucose as tracers of PBOA. To achieve that, both had to be quantified and recommendations provided on

how the formers can be used to estimate the latter. Instead, the manuscript is rather descriptive on polyols and glucose, and results from a largely unclear PMF analysis is given, which leads to my next comment.

2. The PMF analysis is overwhelmingly under-reported and under-explained, given that only its main results are presented. The analysis is actually referred to a report on a url which is no longer valid, or refers to publications in preparation, which is not acceptable, given that one cannot first publish the results and later the analysis. If the authors decide to keep PMF analysis for the revised version of this study, further (and complete) details of the analysis must be provided.

3. Please present your results (e.g. Fig. 6) limited only to PM10 sampling, as it is bound to represent more closely the actual atmospheric concentration, instead of being limited by too low sampling cut-off for the species studied. I recommend maintaining though section 3.3 (PM25/PM10 comparison) to report fine vs coarse mode analysis.

4. I find the sampling site denominations used here unsuitable. Urban sites are typically strongly impacted by traffic emissions, so their distinction feels arbitrary. And why rural? Do you mean background? From those denominations it feels like it is lacking filter sampling at forested sites, for example. An improved description of the sampling sites is necessary to better understand its somewhat unexpected results.

5. There is certainly a lot to gain from combining several sampling sites, but I find that the analysis has become too shallow, unfortunately. Could you also focus on one sampling site and add more analysis (e.g. comparison with FBAP, total number, other species, wind direction/speed, backtrajectory, etc.) to try to better understand what is driving polyols and glucose atmospheric concentration? The manuscript seems to bring more questions than to answer at this point. Especially when it is kept fairly general (unclear PMF, unclear sampling periods, unclear site characterizations, etc.).

Specific/technical comments:

Abstract: Unclear why dust ressuspension would be linked to PBOA factor.

L.53: PM affects climate, not necessarily negatively.

L.57: please refer to a more recent reference for carbonaceous matter.

L.57-L.66 I suggest focusing on OM on the introduction, rather than OC, an artificial species from analytical limitations.

L.63: a significant fraction of OM can be associated with . . .

L.72: Please specify in which environments you are referring this figure, including atmospheric layer and aerosol sizes.

L.74-76: And fluorescent techniques?

L.79: Unclear how atmospheric transport complements sources and abundances.

L.101: Datasets

L.104: Please define atmospheric emission pathway. Do you mean the processes the plant underwent to emit polyols?

L.132: Please define "very rural".

L.152: Please state that this number typically ranges from 1.2 to 2, so the estimates here represent an upper value of OM, thus a lower estimate of the contribution of PBOA.

L.185: extra space before comma.

L.186. Define JRC

L.194: It is unclear why mix up filter-based BC with already quantified thermo-optical EC. Or there was no EC from DECOMBIO project? Please clarify.

L.200: See comment #2

L.211: Range values refer to min/max? In terms of readability I prefer you remove this info and present only avg±std.

L.212: Please define Primary Sugar Compound (SC).

L.228: Please increase axis font sizes.

L.233: The asterisk is hard to readily identify. Please show only PM10 cutoff filters on this figure.

L.233: The selected period feels somewhat arbitrary, thus lacking a clear definition of what is shown. Please be more direct on the chosen periods (dd-mm-yyyy) and criteria applied.

L.255: Please add the information of their estimated atmospheric lifetime.

L.256: It feels like a weak hypothesis to me, from the PBOA perspective, could it be emission ratios change with wind speed, temperature, RH? If focusing on comparable season/meteorology, could the correlation be improved, given distinct emission pathways? And how about interferences from other sources? Is it mixing PM2.5 samples?

L.267: To improve readability, please remove SD and describe only the four average values of both sampling sites, given the interest is the distribution of fine vs coarse mode.

L.290: Please remove "compartment".

L.282: Please indicate the number of samples used on this analysis.

L.301: Does it make sense that PBOA-related polyols are "only" 2-3 times higher in summer in comparison to winter time? The trend behind concentrations in "rural", "urban" or "traffic" feels inconsistent with PBOA interpretation.

L.404: In which time series?

L.440 Please correct sea-salt and not " sea minus salt".

L.445: Unclear sentence.

C5