Atmos. Chem. Phys. Discuss., 14, C12174–C12178, 2015
www.atmos-chem-phys-discuss.net/14/C12174/2015/

Atmospheric Chemistry and Physics Discussions

Interactive comment on "Investigation into chemistry of new particle formation and growth in subtropical urban environment" *by* F. Salimi et al.

Anonymous Referee #3

Received and published: 13 February 2015

This manuscript presents measurements of particle size distributions and chemical composition in Brisbane, Australia, with a special focus on new particle formation (NPF) events. The authors conclude that the organic markers f43 and f44 behave differently when organics are produced from NPF and when it is traffic-produced.

The paper is mostly well-written, and does address an important scientific question. However, I do find several major shortcomings that need to be addressed before this manuscript can be considered for publication in ACP.

Major comments

Statistics:

Full Screen / Esc
Printer-friendly Version
Interactive Discussion
Discussion Paper

One of the main problems with this manuscript is the statistics. In part because the analysis is based on only 5 NPF days, but even more so because at no point do the authors acknowledge this limitation of the analysis. An effect that is found based on the analysis of three specific days is taken as a general truth without further discussion, and stated in the abstract that "this finding can be used as a tool for source apportionment". More discussion is needed on the applicability of the findings in this manuscript.

Related to the comment of the use for source apportionment based on f43 and f44, to arrive to this conclusion the authors have already used f57 as an accepted marker for traffic-produced aerosol. What is then the use, or additional value, of the f43 and f44 markers?

How many non-NPF days are included in the analysis from S12 and S25? What are the shaded areas in Figures 1 and 2? Certainly there has to have been more variability over these periods. This needs to be shown in the figures.

If Cheung et al (2011) found 65 NPF events per year in Brisbane, why did the authors now only detect 5 events over the course of 10-15 weeks (five 2-3 week deployments, as I understood from the text, although it was a bit unclear in the formulation)? Such variability needs to be discussed. What triggers NPF? Certainly there has to be more meteorological data available that would help in understanding chemistry of NPF than what is presented here. These might have been discussed in Cheung et al, but also closely relate to the results in this manuscript, and therefore need to be discussed here as well.

Condensation sink:

The way the condensation sink (CS) is discussed and used in this paper, raises the question whether the authors analyzed and interpreted their data correctly. P27949, L23-24: "The surface area of aerosol particles that is available for condensation can

ACPD
14, C12174–C12178, 2015

be measured using condensation sink (CS)." This is a very awkward statement, and certainly the surface area is not "measured using CS". CS can be calculated from the measured size distribution.

If the formula for the calculation is given explicitly, the variables also have to be defined in the text. In order to show that the equation has been used correctly, some discussion should be included e.g. on the value of the mass accommodation coefficient used.

Fig. 1: The absolute values of the CS in these plots are extremely high. While I am not aware of the specific aerosol sources at this location, I still expect that these numbers are erroneous as they are presumably calculated from the PSD of particles smaller than 400nm and get values orders of magnitude higher than I have ever seen. This also raises the question about the huge variability in the values, especially at S25. While I believe the numbers are erroneous, the interpretation based on the CS plots, that it is lower a few hours before NPF onset in accordance with previous work is also highly questionable. The CS is all over the place, and is on average higher during most of the NPF days, which certainly is not in agreement with typical findings. These data need to be thoroughly checked, and if the authors still seem to find such high CS and CS variability, the calculations (and reasons) need to presented in great detail to be convincing.

AMS data:

For anyone familiar with AMS data, it is obvious from Figures 3-5 that the data has not been analyzed correctly with regard to ammonium. Ammonium is typically present as ammonium sulfate or ammonium nitrate, and in both salts the ammonium makes up less than a third of the total mass. In Fig. 5 the authors propose that ammonium makes up 2-3 times more mass than nitrate+sulfate in the 50-100 nm range, which would be a shocking finding in itself. However, when looking at the size distribution (or in fact lack

ACPD
14, C12174–C12178, 2015

thereof) in Fig. 3-4, it becomes evident that this must be an error in data processing. The authors need to improve their knowledge on how to correct the ammonium signal in the AMS and replot and then reinterpret all these figures. In addition, if the ammonium was so badly off, it does raise the question how well for example the organics have been evaluated, as there exists many pitfalls there as well. Perhaps this only relates to the size dist. data of the AMS, as the NH4 values in Fig. 2 seem more reasonable.

The PSD should also be converted to volume in order to be able to compare properly to the AMS data size distributions. I recommend the authors compare to Fig. 3 in Zhang et al (2004), and use this as a goal for their own plots.

Other comments

P27955, L1-5: With such a short description, it is very hard to understand what was clustered how, and what the real outcome was. The authors need to expand on this, possibly including a figure.

P27955, L18-20: While the behavior of f57 convincingly shows that traffic related aerosol seemed to stay constant between NPF and non-NPF days, it certainly cannot "explain the pattern observed in f44 vs f43".

Specific comments

Look over use of parentheses in references. At least P27948 L23, P27949 L9 and L22.

P27950, L23: What does the term "component mass spectrum" mean? Why not just say total organic signal? Also, I would not say that f44 and f43 "can characterize" the degree of oxidation. Please reformulate.

P27951, L6-11: I would like to see a statement on why the authors used this model. Both in general, why fitting was needed, and why specifically this model.

ACPD
14, C12174–C12178, 2015

P27953, L13-14. What does “similar” refer to here?

P27953, L19-20. “percent”

Interactive comment on Atmos. Chem. Phys. Discuss., 14, 27945, 2014.

ACPD

14, C12174–C12178, 2015