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Interactive comment on “Hygroscopic properties of the Paris urban aerosol in relation to its chemical composition” by K. A. Kamilli et al.

Anonymous Referee #1

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Review of “Hygroscopic properties of the Paris urban aerosol in relation to its chemical composition”, by Kamilla et al.

Overview:

In the following manuscript the authors investigated the hygroscopic properties of sub-micron particles in Paris during summer 2009 and winter 2010. A humidified differential mobility particle sizer (HDMPs) and a twin differential mobility particle sizer (TDMPs) were used to measure the hygroscopic properties and a high resolution aerosol mass spectrometer (AMS) was used for chemical measurements of the non-refractory material. There have been a few similar studies recently. Additional studies like these are needed to give a more global picture of the relation between hygroscopic properties of

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particles and chemical composition, and therefore, I think the topic is well suited for the journal of Atmospheric Chemistry and Physics. However, when predicting the descriptive hygroscopic growth factors (DGF), the authors used several simplifications that were not justified and not used in recent studies. The paper would be much improved if the authors didn't use these simplifications, and hence I encourage the authors to redo their analysis without these simplifications. In other words, a more in depth analysis is warranted.

Major comments:

1)Based on Figure 3, all computations were made with ammonium sulfate for summer and ammonium bisulfate for winter. However, later they point out flaws with this assumption/simplification. For example on Page 14314, line 11-12 they state "However, for the end of the campaign the AIM model calculation reached ion balance with ammonium bisulfate in absence of ammonium sulfate. This is in contrast to our original assignment." As another example, in the conclusions they state that possible reasons for differences between predicted and measured growth factors include ambiguous ion balance calculations. After reading the document I wonder why they have used such a simplification for ion balance? Some other recent papers on a similar topic do not appear to make the same simplification [Hersey et al. ACP, 2009, 9, 2543-2554 and Gysel et al. ACP, 2007, 7, 6131-6144]. Why not use an ion balance that is calculated daily or hourly?

2)In the conclusions the authors state that a possible reason for differences between predicted and measured growth factors include "evaluation of the descriptive growth factor measurement for a particle diameter of 285 nm in comparison with growth factor calculations using the chemical composition of PM1." Why didn't the authors use the chemical composition of 285 nm for the calculations, since I assume this information is available from the AMS. Some other recent papers on a similar topic have used sized resolved chemical composition from the AMS in their calculations [Hersey et al. ACP, 2009, 9, 2543-2554 and Gysel et al. ACP, 2007, 7, 6131-6144 and Aklilu et al. 2006,

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40, 2650-26621].

3)What size did the authors assume when calculating growth factors? I guess they used a size of 285 nm and used the total mass from the AMS, but this was not absolutely clear from the figures and tables. For example in Table 1 they indicated a growth factor for 285 nm particles, but in the figure caption for Figure 6 they indicated that the calculated growth factor is for 1 micrometer particles and in the legend for Figure 6 they indicated that the calculation is for PM1, implying < 1 micrometer particles. Please clarify.

4)On Page 14311, line 10 the authors discussed results for air masses brought from over the Atlantic Ocean to Paris. The modelling/interpretation do not consider NaCl particles. For the uninformed reader, it would be useful to add some discussion on why NaCl particles do not need to be considered.

5)Page 14313, line 3. “It is remarkable that the measured times series of the DGF shows a much higher variability in winter than in summer. This decreases the correlation coefficient of the wintertime data to $R=0.52$ (Fig.6c).” Is it obvious that a higher variability in the data will lead to a decrease in the correlation coefficient?

6)Page 14310, line 4, “The reason for this is a concentration maximum within the measurement range of the HDMPs directly below a diameter of 300 nm. This maximum becomes obvious in Figure 5.” I don’t understand this statement, since Figure 5 does not show concentration.

7)It would be very useful to show the size resolved mass spectrum from the AMS so that a reader can see the size distribution for each component.

8)Page 14312, line 17. “As already mentioned in Section 4.4, the first few days of the summer campaign are different from the rest of the campaign. This is also evident in the DGF measurements of 285 nm particles which show the strongest variation in this time interval (Fig 6a).” When looking at the first few days, I don’t see a stronger variation

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in the data then during other time periods. Please include quantitative statistics.

9)Page 14312, line 18. “ Furthermore the deviation of the measured and the calculated DGF is strongest during this period, whereas the rest of the time series fits well if one considers the use of a simplified growth model.” Again, when I look at the data the deviation of the measured and calculated DGF doesn’t appear to be any worse than the rest of the time series. Please include quantitative statistics.

10)Figure 2 shows data for the summer. Why not show the same analysis/data for winter?

11)The authors state on page 14305, line 26, “as long as no stepwise changes with particle size are observed in the average hygroscopic growth factors, the SM remains valid. In this study stepwise changes were not observed.” Please refer to the figure where this is shown (Figure 5?). Also, please give a reference to where it is shown/proven that the assumption is valid as long as no stepwise change with particle size is observed. If this has not been shown/proven in a previous publication (as suggested by the other reviewer) then some proof should be given here.

12)At the end of the document they give a few possible explanations for why they have over predicting the growth factor, one possibility being evaporation of ammonium nitrate. Have the authors also considered the possibility of the organic being in a glass state or the possibility of liquid-liquid phase separation?

Technical comments:

1.Abstract, line 15. Units are incorrect. Should be m^{-3} .

2.Page 14299, line 1. “direct and indirect light scattering properties of aerosols” is slightly confusing. Maybe rewrite to “direct and indirect effect of aerosols”?

3.Page 14301, line 21. “well below 30%” is not precise. It would be more useful to state the range. For example, < 20% RH or < 10% RH.

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4. Page 14302, line 23. The authors refer to crystalline organics; however, I thought in most cases crystalline organics are not expected. See work by Marcolli et al. JPC, Volume: 108, Issue: 12, Pages: 2216-2224.

5. Figure 6, Panels b and d. In these panels the data are divided into high NO_x and low NO_x, implying the authors have applied a filter to separate ALL high NO_x from ALL low NO_x. But on line 5, page 14313, the authors indicate that they use the dates Jan 23-29 and Feb 6-15 for high NO_x. I suggest changing the legend slightly to be more consistent with what is plotted. Instead of indicating high NO_x, change to something like Jan 23-29 (high NO_x conditions). Alternatively the authors could include in the figure caption exactly what they mean by high NO_x (i.e. Jan 23-29 and Feb 6-15).

6. Page 14314, line 29. Indicate the RH for the growth factors of the organics.

7. Page 14311, line 18. Typo. “saltsis”.

8. Several of the figures were hard to read due to the small size of the axis labels and annotations.

Interactive comment on Atmos. Chem. Phys. Discuss., 13, 14297, 2013.

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