Review of “Low hygroscopic scattering enhancement of boreal aerosol and the implications for a columnar optical closure study” by Zieger and coauthors.

General comments:

This paper presents a detail study of aerosol hygroscopicity in a remote site using state of the art instrumentation. The authors combined many instruments which provide a valuable insight in the aerosol properties. The paper is of interest for the scientific community and it is clear and well written. The paper is suitable for publication in ACP after major revisions.

Specific comments:

P3330-Lines 13-14: I wouldn’t say that the aerosol hygroscopicity is “significantly lower” at the study site compared to other European sites. Of course, it depends on the sites that you are comparing with. For example, Carrico et al. (2000) reported a f(RH=82%) value of 1.46 for polluted air masses at Sagres (Portugal), Fierz-Schmidhauser et al. (2010) reported a value of 1.8 at Mace Head (Ireland) under polluted conditions and a value of f(RH=85%)=1.6 was reported by Titos et al. (2014) at Granada (Spain).

P3330-Line 14: This is in fact a general comment. Why the authors use the 450 nm wavelength? I encourage the authors to focus on the 525 nm wavelength. The comparison with other sites will be more straightforward this way.

P3333-Line14: State the duration of the humidification cycles (time scanning RH up and down).

P3333-Line26 to 3334-Line10: It is not clear if this comparison is performed in this study or it was previously done by Fierz-Schmidhauser et al., (2010c). Concerning the comparison with the commercial humidified nephelometer by ECOTECH, it would be interesting to see how good the comparison was (slope and regression). This is a novel instrument and it will be great to see its performance compared with a well-tested humidifier (PSI humidifier).

P3335-Line15: The correction factor for the aethalometer, was it determined for this campaign? This value might change depending on the predominant aerosol types…

P3336-Line13: Can the heated inlet affect the measured size distributions?

P3336-Line15: How are the DMPS and APS size distributions merged?

Section 3.4: No specification about the inlet at which the ACSM is connected is given here. State if it is the same inlet than for other instruments, if it is PM10 or PM1, etc.

Section 3.5: Why the ecotech reference nephelometer is not used to retrieve the complex refractive index? Introducing the TSI neph in this study, which is measuring in a different cabin and with a different inlet system seems unnecessary from my point of view and it adds confusion to the manuscript. I suggest the author to focus on the neph tandem measurements.

P3341-Line9: This statement is confusing, it is not clear if a humidogram cycle takes three hours or not. As mentioned before, state the duration of the RH cycles.

P3341-Line14: To contextualize the aerosol properties at the measurement site, I recommend the authors to include a table with, at least, mean, std, min and max values of the dry scattering coefficient, absorption coefficient, single scattering albedo and scattering Ångström exponent.

P3342-Line13: Why the f(RH) is given now at 450 nm? In the previous paragraph it is given for the 525 nm!!! Figure 1 also refers to the 525 nm wavelength. Are there any reasons why the authors use the 450 nm? This is very confusing and needs to be corrected. Additionally, using the 525 nm makes comparison with other studies more straightforward.

P3342-line25: The authors should dig deeper into the literature regarding the relationship between organic fraction and f(RH). In particular, the paper by Quinn et al. (2005) should be mentioned here.

Quinn, P. K., T. S. Bates, T. Baynard, A. D. Clarke, T. B. Onasch, W. Wang, M. J. Rood, E. Andrews, J. Allan, C. M. Carrico, D. Coffman, and D. Wornsnop. 2005. Impact of particulate organic matter on the relative humidity dependence of light scattering: A simplified parameterization. Geophys. Res. Lett., 32, L22809.

P3343-Line24: Do organics from marine origin affect f(RH) under maritime air masses?

P3343-Line24-Figure5: This problem can be avoided if only concurrent measurements of chemical constituents and f(RH) are used. Doing so, it would be easier to see any relationship between these variables and their respective spatial patterns.

P3344-Line4: The manuscript has too many graphs (15!) and some of them are not necessary. This is the case of Figure 6, the information can be given in the text.

P3344-Line25: How this comparison of the lowermost part of the profile with ground measurements is performed? The dots in figure 7b, are average values? How this comparison is done should be explained in more detail. N_{tot} Ground can be included in Figure 7a for better visualization of the agreement between the lowest part of the profile and the ground measurements.

P3345: Many assumptions are done by the authors to calculate the AOD. I suggest the authors to soft this discussion and emphasize other parts of the manuscript since the conclusions driven from this AOD-comparison are subject to many errors and depend on the assumptions made.

- The use of the scaling factor c, has been previously used in the literature? The size of the particles (which is not included in the scaling factor) as well as the chemical composition of these particles will influence the magnitude of the AOD. This should be discussed in the manuscript as an additional source of error.

- Why the SMPS size distributions onboard the aircraft are not used? Changes in the size distribution with height would affect the AOD estimation.

- Assuming that the f(RH) is not dependent on the size distribution and chemical composition is in fact not consistent with the authors results and with the literature.

- A reference for the non-dependence of the aerosol absorption coefficient with RH is needed.

- Include in the Figures the AOD (AERONET and calculated) uncertainties.

- The extrapolation to larger wavelengths is a source of uncertainty. The study should be limited to the 450-700 nm range, or at least a discussion about the errors of extrapolating from 700 to 1600 nm is needed.

I'm very surprised that all the assumptions made for the AOD calculation are not considered as hypothesis of the disagreement. This should be included and carefully addressed.

Section 6.1: This section is confusing. If you use the TSI nephelometer you are not looking for inconsistencies in your data since you used the ecotech neph as reference… The two nephs, wet and dry, were calibrated before the measurement campaign and they showed good agreement which each other (differences below 12% as stated by the authors). Therefore, this section is unnecessary. The direct comparison between both nephs is more reliable from my point of view than the comparison with the retrieved scattering coefficient using the size distributions measured in a different container (different location, inlet, and so on…).

- Do you compare the reference neph (Ecotech) and TSI neph before or during the campaign but sampling from the same inlet?

Section 6.2: To avoid this problem the integration of the extinction coefficient to calculate the AOD can be done starting at 18 m. Thus, both AOD (AERONET and calculated) retrievals start at the same height.

P3350-Line27: According to the authors, particles below 100 nm in size are optically less important. However, these particles are included in the scaling factor c. It would be better to use the size distribution as scaling factor instead of the total number concentration.

Section 6.3: How representative is the lidar data measured 200 km far from Hyytiäla? In 200 km distance differences in the vertical distribution of aerosol particles is expected.

Figure 1: Split into two graphs scaling the axis appropriately.

Figure 2: At 450 nm the f(RH) values are lower than at 525 nm, which could be partially the reason why the authors observe low f(RH) values compared to other sites. On the other hand, the "a" parameter is closer to the ideal value of 1 at the 525 nm while it is

slightly below 1 at 450 nm. This fact reinforce my opinion that the f(RH) values should focus on the 525 nm wavelength.

Figure 2c: reduce the x axis scale.

Figure 3: Figure 3b is very similar to that reported by Zieger et al. (2013) with the exception of Hyytiäla data. As I mentioned before, I think that there are many graphs. The authors should revise the manuscript and keep the more relevant ones.

Figure 4: Change to f(525nm, 85%). The regression coefficients (slope, intercept and R2) for Melpitz differ from those presented by Zieger et al., (2014).

Figure 5: I think that it would be more interesting to use only concurrent measurements. That way is easier to establish any relationship between the different spatial patterns.

Figure 6 could be omitted.

Figure 7: include the ground measurement of N_{tot} in Figure7a.

Figure 10: This graph could be omitted too; the information can be given in the manuscript. In addition, consider limiting the AOD retrieval to 700 nm. The errors extrapolating to 1600 nm may justify partially the results.