Interactive comment on

„ Low hygroscopic scattering enhancement of boreal aerosol and the implications for a columnar optical closure study" by Zieger et al.

Using a WetNeph (measurement campaign in Mai-August 2013) the authors first calculate the scattering enhancement at Hyytiälä (Finland) and compare it with the one found at other stations over Europe. A clear correlation was found between the scattering enhancement and the organic mass fraction. Combining in-situ surface measurements with particle number concentration and humidity profiles measured by an aircraft, an estimation of the AOD was calculated at several wavelengths and compared to sun photometer measurements. The inconsistencies of this columnar optical closure study are then discussed in the light of an optical closure study for in-situ instrumentation and of the difference between in-situ and aircraft measured size distributions. Finally the contribution of high altitude aerosol concentration to AOD is estimated from aerosol backscatter coefficient profile from a lidar.

The paper is well written and scientifically very clear and the figures are self-explaining. It should be published after a major revision:

Main comments:

- Results: 5.1, figure 4: you only plot the data of 2 over 6 sites. Taking into account all the sites, is the correlation between f(RH) and the organic mass fraction unique ? if yes is it possible to give a general rule f(organic mass fraction)?
- Results: 5.2: The AOD measured by the Sun photometer is a measurement of the whole atmospheric column beginning at the instrument altitude (18 m agl) above the canopy of the forest (§ 3.7). The calculated AOD begins at ground (It is not clear in your paper what is the beginning point for that integration) and end at the maximal altitude reached by the aircraft. If not done, you can probably begin the integration of equation (5) at 18 m with the extinction coefficient measured on the 17 m mast in order to have the same beginning point. I do not think that this will change your results that much, but the integration will at least begin above the canopy. This will however change part of discussion about the particle losses problem in the first § of 6.2.

  Secondly, you never estimate the part of the AOD difference that is due to the different end points h1 of the integral (Eq. 5). Referring to fig. 8b and 15b, it seems obvious that the aerosol load over 3000 m is not always negligible. Would it be possible to fully integrate the calculated AOD on the whole atmospheric column by fitting a decrease of the extinction coefficient as a function of altitude ? This would be a valuable approach at least for the first time period (21-28 May).

- Results: Figure 10: it seems to me that the data taken to calculate Fig 10 are not mentioned. Since the measurements from the 30 May to the 3 June are clearly influenced by SDE at high altitude, I hope that this figure is produced with only the first time period (21-28 May). If it is not the case, the discussion concerning the coarse particle losses has probably to be changed.

I suppose that you use the extinction Angström coefficient to interpolate to all the CIMEL wavelengths. Due to the restricted wavelength range of the Nephelometer, what is the uncertainty in the interpolation up to 1600 nm ? Could this have an effect on the discussion 6.2 ? It seems from Fig. 11 that the measurements at the greatest wavelength are more dispersed than at 450 nm. This is also probably the case for the AE-31. Are therefore the uncertainties of the nephelometer and the Aethalometer similar at all the wavelengths ? If not, what is the effect of these uncertainties on Fig. 10 and on the discussion about coarse particle losses ?

- Discussion 6.1: without referring to other papers, it is not clear if the operational measurement (Neph cottage) are done at the same altitude than the container measurement or on the mast above the canopy. If the operational measurements are done above the canopy, it changes several points of your discussion.
- Discussion 6.3: I would take the lidar data as measurement results influencing the analysis (see previous comments on the exclusion of these data for Fig. 10) and not really as a discussion point.
- Is the size distribution at the maximum fly altitude similar to the one at the minimum altitude plotted on Fig. 12a ? If no, can the difference give you indication on fine/coarse particles losses as a function of altitude? If no and taking into account the CPC measurement, would it be possible/valuable to separate the scaling factor c into fine and coarse contributions ?

Minor comments:

- Abstract: p. 3329 line 27: The sentence is not clear, since it seems that the "direct measured values" are not the same than the "Sun photometer AOD". When speaking of the Sun photometer AOD, I would emphasize the fact that it measures the whole aerosol column instead of using the adjective "direct".
- Introduction p. 3332 lines 1-5: I find the questions not really pertinent. For the first question, the main point is more to compare the scattering enhancement at Hyytiälä with the ones measured at other sites than only to measure the value (what we know that you are able to measure regarding your previous publications). The second point is more about the quality and limits than about the feasibility of the columnar optical closure.
- Instrumental 3.7: p. 3339 line14: is the "level 2.0 data" self-explaining ? A reference could perhaps be added to avoid a description.
- Results: 5.1 p. 3342 first §: It seems curious that the maritime influence is not mentioned for Cabauw, but the answer is probably in your 2013 paper. However for Mace Head, it is not clear if the value f=2.08 is a mean for the station of if it corresponds only to time with anthropogenic emissions. Perhaps you could add f values for both maritime and anthropogenic influences. Finally it is written that Mace Head showed a "large variation in f(RH)". The variation (i.e. the f(RH) range with non zero PDF) is not larger than at Ny-Alsund or Cabauw, but is a convolution of three components.
- P. 3343 last §: did you see any deliquescence with air from maritime origine ?
- P. 3344, line 9: the "slightly increase" is not obvious.
- 5.2: p. 3345 line 11: replace "α the Angtröm exponent" by "α_{sp} the scattering Angström exponent".
- P. 3346 lines 4-8: I suppose that you assume that the absorption coefficient is not changing with RH also because it was already shown in previous publications that the humidity impact

on the absorption coefficient is negligible or at least far lower than on the scattering coefficient.

- P. 3346 line 21: the value at the surface is also higher than on the 23 May and the ML is probably also higher. Or is the N concentration particularly high at 2700 m for that day ? It can be valuable to correlate this profil with Fig. 7a. It is however quite difficult to distinguish the profiles for that 2 days. Could you choose particular colors that evidence the 23 may and the 2 June profiles in Fig. 7a?
- P. 3347 line 15: really unexpected ?
- P. 3349 lines 7-9: Do you have an explanation for these larger variations of the WetNeph reference nephelometer due perhaps to the location on a parking place (traffic, wind, convection due to the ground material)?
- P. 3351 line 6: does linear regressions have a spectral variability? What does mean the "optically decrease of a correlation coefficient"?
- Table 1 and Figure 2 are redundant. I prefer the figure. Fig. 2c could have a smaller x scale.