

Interactive comment on “Seasonal variation of aerosol water uptake and its impact on the direct radiative effect at Ny-Ålesund, Svalbard” by N. Rastak et al.

Anonymous Referee #2

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Rastak et al review, “Seasonal variation of aerosol water uptake and its impact on the direct radiative effect at Ny-Ålesund, Svalbard”

This paper utilizes aerosol measurements along with several different models to investigate the effect of aerosol hygroscopicity on direct aerosol radiative forcing. The authors observed a seasonal cycle in both the dry aerosol measurements and the hygroscopic growth data. They found that the influence of hygroscopic growth on aerosol forcing was weakest (lowest hygroscopicity) during the haze season in the springtime and strongest in late summer/early fall.

C1002

This is a well-written paper, although not particularly groundbreaking. Most of my comments are editorial in nature and are listed below.

Scientific questions: Figure 2 suggests that you aren't really using the nephelometer measurements other than as a check on the size distribution measurements. Didn't Zieger et al (2010) already show that the calculated/measured scattering was in good agreement at the site so you could skip all the mention of the nephelometer measurements and calculated/measured nephelometer comparisons and just use the size distribution measurements to do what you've done. Similarly don't think the $f(\text{RH})$ discussion provides anything really new. Alternatively, why do you need to do the size distribution+Mie theory part when you are measuring wet and dry scattering (and absorption) and can determine other parameters (asymmetry parameter, SSA) from the measurements? Doesn't the radiative transfer allow for input of vertical profiles of aerosol optical properties without the added complication of using size distribution to calculate the optical properties? If you have the measured optical properties why would you introduce additional error by calculating them from size distributions?

Editorial

P 7068

line 25 “A better understanding on the radiative...” → “A better understanding of the radiative...”

P7069

Line 5-6 “. . .aerosol particles, thus affecting ADRE. . .” → “. . .aerosol particles, and thus can affect ADRE. . .”

Line 26-27 “. . .depending on the location and atmospheric conditions.” → “. . .depending on the location.” Comment: Table 1 doesn't provide any information about atmospheric conditions. Really, I think that the point is that aerosol properties in different at places.

Line 27-29 “Fierz-Schmidhauser et al. (2010c) measured the hygroscopic growth and

C1003

optical properties of 100nm ammonium sulfate and sodium chloride particles in laboratory and found values of f (85 %) ranging from 6 to 18.” Comment: I personally would probably delete this sentence since citing a single lab study of non-ambient particles is not as relevant as the field studies listed in Table 1. Otherwise I would rearrange the sentence to contrast it with the ambient measurements mentioned in Table 1: “Even higher values (f (85 %) ranging from 6 to 18) have been found for 100nm ammonium sulfate and sodium chloride particles in the laboratory (Fierz-Schmidhauser et al., 2010c).”

Page 7070

Line 8-9 “. . .contributed in a continuous reduction. . .” → “. . .contributing to a continuous reduction. . .”

Line 13 “. . .both by e.g. altered. . .” → “both by altered. . .”

Line 20-21 “. . .campaignwise data. . .” → “. . .campaign data. . .”

Page 7072

Line 3 “. . .water and produce saturated aqueous solutions. . .” → “. . .water, thus becoming saturated aqueous solution droplets. . .”

Line 16-19 need to define ‘T’ in equation 1.

Page 7073

Description of Mie model should clarify whether it is for homogeneously mixed spheres or shell/cores (I know it’s described later, but that’s something that should be mentioned here.)

Description of input to the radiative transfer model should also refer to section 4.1.3 for information on how the vertical profiles of aerosol properties were determined. (Currently in section 3.3 you just explain how you come up with values for the aerosol properties using Mie code, but not how you get the aerosol profiles.

C1004

Page 7074

Line 9 I think you need a space: “. . .calculations(see. . .” → “. . .calculations (see. . .”

Line 9 “. . . 8.00 to 12.00 a.m. . .” 12 am is defined as midnight. Do you mean 12 pm (noon)? Also, should clarify that it’s local time.

Line 12 “. . .10.00 to 12.00 a.m. . .” see comment above.

Line 14 “. . .sufficient sunlight . . .” clarify – how many hours is ‘sufficient’

Page 7075

Line 2 “The RH measurements. . .” → “The surface ambient RH measurements. . .”

Line 13 “Twelve Whatman. . .” → “Whatman . . .” Comment: 12 sheets were used because there was 1 sheet/month? I don’t think you need to include the 12. If you want to clarify the number of samples you could change the previous sentence “. . .on a monthly basis from 1 September 2007 to 9 September 2008. . .” → “. . .on a monthly basis from 1 September 2007 to 9 September 2008 (for a total of 12 filter samples). . .”

Line 25 “MWS-OS was. . .” → “MWS-OC was. . .”

Page 7076

Line 4-5 “. . .determind. . .” → “. . .determined. . .”

Page 7077

Line 19 “This lead to. . .” → “This leads to . . .”

Page 7078

Line 16-17 “. . .do not represent the size-dependent chemical composition at the Zepelin site, but rather an average bulk composition. . .” Comment: In section 4.1.2 you’ve already noted that you are not dealing with a size dependent chemical composition, so maybe add something here reminding readers of that?

C1005

Line 22-24 Comment: have you compared the asymmetry parameter and single scattering albedo from the Mie model output with the calculated values from the measurements? You can calculate an asymmetry parameter from the nephelometer and SSA from the nephelometer + aethalometer.

Page 7079

Section 4.2.3 – I think you should mention $f(RH)$ in this paragraph just to clarify in the reader's mind the difference between GF (with a TDMA) and $f(RH)$ (with a nephelometer).

Page 7080

Line 3-5 “The large deviation for the June sample is probably due to the fact that during the period with high sea salt concentrations, the high volume sampler was not operating.” Comment: need to clarify – what does the hi-vol not working have to do with high seasalt concentrations?

Line 12 “. . .of 80–120nm the filter samples. . .” → “. . .of 80–120nm, while the filter samples. . .”

Line 14-15 “Previous studies on the seasonal trends of chemical composition at several monitoring sites in. . .” Comment: do you need to specify ‘coastal’ monitoring sites here? (Summit in Greenland probably doesn't get much sea salt)

Page 7081

Line 4 “. . .the modeled coefficients. . .” → “. . .the modeled scattering coefficients. . .”

Line 18-21 “The main reason for this difference is probably related to somewhat different dry references used in the studies (see Zieger et al., 2010 for details). The different dry reference values can be partly due to the different operating conditions, and partly due to e.g. different inlet structures and resulting losses.” Comment – this seems vague – are you talking about assumed dry RH? Assumed dry scattering? Other? You didn't

C1006

discuss inlets/losses at all here – did those change between Zieger and your paper or is there another paper describing those changes that should be cited?

Line 23-25 “. . .enhancement factors and size distribution together with an assumption on dry refractive index to retrieve the apparent hygroscopic growth factor.” → “. . . $f(RH)$ and size distributions together with an assumption about dry refractive index to retrieve the apparent hygroscopic growth factors.”

Page 7083

Line 3-4 “Seasonal variations of aerosol number size distributions, scattering coefficients σ_{sp} and enhancement factors $f(RH)$ are presented in Fig. 9.” → “Seasonal variations of measured aerosol number size distributions, and modelled scattering coefficients σ_{sp} and enhancement factors $f(RH)$ are presented in Fig. 9.”

Line 22 “The enhancement factor $f(RH)$. . .” → “The enhancement factor $f(RH=ambient)$. . .”

Page 7085

Line 15 “RH values about 50% . . .” → “RH values of about 50% . . .” Comment: This profile RH in Figure 11a is a lot lower than the RH values depicted in Figure 8a. Is there an RH profile that is more representative?

Line 18-21 “In contrast, a significant difference between the scattering coefficients calculated for the dry and wet cases is predicted, especially below 2 km (about 50%), where both RH and the aerosol particle concentrations are high.” Comment: I am surprised at the amount of scattering enhancement ($f(RH)$) at $RH \sim 50\%$. It seems high compared to lots of other studies on hygroscopic growth, even at clean marine sites which should be similar to the high hygroscopicity sea salt aerosol measured at Ny-Alesund. I think this is worthy of more comment/discussion (or cite other Ny-Alesund $f(RH)$ papers if they discuss this).

Page 7086

C1007

Line 1 "...(Bohren and Huffman., 1983), ..." → "...(Bohren and Huffman, 1983), ..."

Line 18-19 "...while after the haze period there are a large number of smaller particles ..." → "while after the haze period the size distribution shifts to primarily smaller particles ..."

Line 25 "...which expectedly peak during..." → "...which peak during..."

Page 7087

Line 6 "...vertical distribution of the aerosol particles and their chemical composition..." → Comment: there's also uncertainty in the aerosol size distribution with height – it seems likely that the size distribution would look different within and above the boundary layer.

Line 14-15 "The dry size become more significant compared to RH when calculating the ADRE instead of the surface layer scattering coefficients..." → "The relative importance of RH and dry particle size are reversed for ADRE and surface layer scattering coefficients." Comment: I think my suggested change is what you were trying to say?

Line 23-24 "Figure 13 also demonstrates the importance of knowing the surface albedo for accurate predictions of ADRE, particularly during the early spring months." Comment: You should expand on this – the importance of surface albedo is almost as large as the dry size for some months so give a range of the effect similar to what you did for RH and dry size. I am assuming the surface albedo at Ny-Alesund changes because of snow melting and exposing bare ground? You could even include a statement saying something along the lines of: transitional months from snow cover to (rock and vice versa) might have the most uncertainty and that if this corresponds to the haze season (does it?) then things are even more uncertain.

Tables/Figures

Table 1 – Sheridan measurements were made in Oklahoma (delete 'South Kansas')
Add the results from this current Ny-Alesund study to the table either at the top or
C1008

the bottom of the table. If Zieger et al (2013) repeats results from Fierz-Schmidhauser (2010ab) for Mace Head and JFJ, then maybe don't repeat here – it's like double counting. I'm assuming that the mineral dust $f(\text{RH})$ mentioned for Zieger was for JFJ and the Arctic aerosol was for Ny-Alesund, then just list those two sites for that paper?

Table 2 Aren't some of the chemical components only known on a monthly basis (e.g., as noted in Figure 3)?

Table 3 in the table notes, wavelength should be one word. Figure 3 In caption MWL-OC → MWS-OC

Figure 5a – does this really need to be on log scale? I feel like log scale hides things sometimes. How does it look with linear axes?

Figure 6a – are these for ambient RH or a set RH (e.g., RH=85%)?

Figure 6b – if you split the histogram by season do you see a shift in the curves?

Figure 13 – In caption "(new cases)" is confusing – perhaps change caption to "The sensitivity of the ratio between the calculated ADRE and the ADRE (wet base case) to..." or something like

Interactive comment on Atmos. Chem. Phys. Discuss., 14, 7067, 2014.