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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Arctic smoke – aerosol characteristics during a record air pollution event in the European Arctic and its radiative impact" *by* R. Treffeisen et al.

R. Treffeisen et al.

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Anonymous Referee #2

1) abstract, line 9 - 10: The sentence "almost 1.6 for geometric standard deviation of the mode" is not easy to understand in the abstract. More specific explanation needs to be added. We like to mention at this point that we believe that this is common knowledge for aerosol related topics. Therefore we think that we might expect that the reader knows what we talk about. Further explanations should not be added to an abstract. Anyhow we slightly changed the sentence.

2) p 2278, line 22: "most severe air pollution"; can you show any reference to prove this fact. Concerning the Arctic we are sure that this was the most severe pollution ever

measured at Ny-Ålesund. For further information we like to refer to the recent published paper in ACP where already the title underlines the speciality of the observed event: Stohl, T. Berg, J. F. Burkhart, A. M. Fjraa, C. Forster, A. Herber, Ø. Hov, C. Lunder, W. W. McMillan, S. Oltmans, M. Shiobara, D. Simpson, S. Solberg, K. Stebel, J. Ström, K. Tørseth, R. Treffeisen, K. Virkkunen, and K. E. Yttri, Arctic smoke record high air pollution levels in the European Arctic due to agricultural fires in Eastern Europe in spring 2006, ACP - Volume 7, Number 2, 511-534, 2007.

3) p 2280, line 3: It is not easy to show three parameters bsca, babs and bext, only by one equation for bext, eq. (1), though we know that all three parameters are shown by the same style of formula. Eq (1) shows only bext. We did add the two missing formula and hope this will be understandable in its revised version.

4) p 2281, line 6: Why "a spectrally uniform surface albedo" was assumed? The available surface albedo measurements in Ny-Ålesund are not measured in dependence of the wavelength. Therefore, we can only take what the routine measurements at the location provide and thus we have to assume the same surface albedo for all wavelengths. We slightly change the sentence here to clear this issue.

5) p 2281, line 11 - 12: Why you can say "There was no evidence of dry or wet deposition"? Are there any evidence of "no dry and wet deposition"? In order to make our point the sentence is changed to: Along the path no precipitation occurred and therefore we conclude that there was no wet deposition.ąÉ

6) p 2282, line 6-7: What is your intension by the sentence "the smoke plume further accelerated to maximum velocity of about 48 km/h"? Our intention was to indicate that with high transport velocities in the last section of the trajectory over the North Atlantic did not allow lateral diffusion and spreading perpendicular to the moving direction of the observed smoke plume. The sentences On April 2006 and during the first days of May 2006 smoke arrived at Ny-Ålesund at the 1000 m level with an average speed of nearly 14 km hąÝ1. After 30 April, the smoke plume further accelerated to a maximum

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velocity of about 48 kmhąÝ1 on 1 May over the Norwegian Sea. are changed to: On April 2006 and during the first days of May 2006 smoke arrived at Ny-Ålesund at the 1000 m level with an average speed of only 14 km hąÝ1. The highest velocities occurred in the last section of transport, reaching a maximum of 48 km h-1 over the Norwegian Sea. Hence, the possibility for lateral diffusion and spreading of smoke was reduced.

7) p 2284, line 6-7: Why only Fig. 7 is made by ECMWF analysis? Other trajectory analyses and horizontal maps such as Fig. 2 and 3 are all from NCEP/NCAR reanalyses. Is there any special reason? Also, in Fig. 5, the ordinate should be shown in altitude (km), not in model level. We assume that this comment is related to figures 6 and not 7. Due to the lack of sufficient radiosonde data along the path and the coarse vertical resolution of NCEP/NCAR reanalyses we used ECMWF data with 25 model levels up to 4 km height to show the details in the vertical distribution of potential temperature of the path. The figure capture is supplemented by explaining corresponding mean heights a.s.l. for selected model levels.

8) p 2284, line 25: What will be suspected by the 100 % humidity? The sentence ąÈMaximum values of humidity reached up to 100% in a layer extending from about 400 to 1000m a.s.l.is supplemented: Maximum values of humidity reached up to 100% in a layer extending from about 400 to 1000m a.s.l. due to the cooling of the relative moist airmass.

9) p 2285, first paragraph: It should be better to mention about the measurement site in Ny-Ålesund. Already mentioned about Mt. Zeppelin in the Abstract, there is no expression as for the actual site in the main text (all made at Zeppelin or not). We checked and believe that we pretty clear state out when the measurements are taken in Ny-Ålesund and when at Zeppelin Mountain. We do not see a necessity to bring up more specification in terms of the sites.

10) p 2285 - p 2286, 4. 1. 1: More deep discussion is needed for AOD. AOD amount

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during this plume episode is among 0.4 and 0.8, which is quite higher than normal condition including haze and dust. More over, Angstrom Exponent is also highest among all the Arctic measurements. Also, it is needed to show day to day variation of AOD (as a figure), which will be better compared to the satellite data in the next section (Table 2 is only for two occasions). We believe that the section about AOD is already pretty long. An overview on general background and Haze conditions are given in the reference Herber et al. (2002). Nevertheless we added a sentence to make this clear and put a connection to the Arctic Haze event presented in Yamanouchi et al. (2005). There is no meaning to prolong the explaination here in more detail as this would not bring up new information on Arctic Haze AOD values. That the AOD is extremely high is pretty clear and so far unique.

11) p 2288 line 25 to p 2289 line 2: It is difficult to conclude as written, because the satellite retrieval in Fig. 8 is only limited to the clear area and most part is not retrieved S878 because of cloud mask. On 1 May 2006, Svalbard area is not retrieved might be due to the cloud cover, so it is not clear that the plume had arrived at Svalbard or not. Is it also covered with cloud for the whole Scandinavian Peninsular? Figure 8 presents the results of the MERIS analysis for the period from 1 to 4 May 2006 for cloud and ice free regions, where observations can be made. While these are gratifying comparisons, it is clear that further improvements to the cloud-screening, the retrieval algorithms and connected data assimilations are needed for polar applications in order to make the best use of global satellite coverage.

12) p 2289 line 26 to p 2290 line 1: What is the meanings of "using a cyclone" and "A Fuchs charge"? We like to comment here the following. This section is part of technical information about the inversion of DMPS data into ambient size distributions. Of course, for someone not familiar with the measuring technique itself this might be difficult to follow but we referred to the corresponding references for more details. Another two pages would be necessary to explain all details and this paper cannot be used for such details. In short it deals with the treatment of multiple charged particles, which is

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necessary to consider when inverting the raw data.

13) p 2292 line 11: What is "the second period"? Is this same with the "week 2"? Yes it corresponds and we changed the wording to week 2.

14) p 2293 line 11: What is the reason for high SS in the week 3? We like to mention at this point, that a discussion about sources and sinks for SS (sea salt) is really not the topic of this study. Entering a track do discuss this would draw the reader from the issue of the paper. It is however relevant to note chemical differences with in these periods as this will impact on the radiative calculations and interpretation of these.

15) p 2294 line 9-11: It is much important to mention about each comparison than to describe "Overall, ...". In the week 3, observation shows rather closer to the internal mixture, while observation is closer to external in the week 5. It was already reported that fresh particles are liable to have much external mixture, while aged particles are abundant in internal mixture (Yamanouchi et al., 2005: Tellus 57B, 141-152; last part of p 149; Hara et al., 2003). We understand the point of the reviewer here. Therefore we changed the sentence as follows: During the studied period, with the exception of week three, the observed absorption coefficient best represented when the model calculations assume external mixing. This is highlighted by comparing the mean relative difference for between measured and modelled absorption values assuming external and internal mixtures corresponding to 9% for the external mixture and -38% for the internal mixture.

16) p 2294, line 14-26: It is still not clear how the vertical extinction profile was calculated only from hygroscopic growth factors. Is this calculation based on humidity profile as in Fig. 4? The use of the word ąÈusingąÉ in this context is admittedly misleading. The relative humidity profiles used are those observed during 27 April and 2 May (see section 5.1 for details). In the revised manuscript the sentence is rewritten as: In order to represent the vertical structure of the aerosol for 27 April 2006 and 2 May 2006 we calculate the ambient extinction coefficient at 530nm from their assumed composition,

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mixture and number size distribution. The size and composition of the aerosol is further adjusted for hygroscopic growth as described in Sect. 2.1.

17) p 2296 line 24 - 25: AOD data (measured and calculated) should be shown in the figure, possibly with the figure I requested in the comment (10). As the calculated AOD is only for the two days under consideration we believe that it is not important to do this in more detailed way as we have put it in the Table 2. In the Table 2 one can clearly compare the AOD at 500 and 1000 nm. This serves to see how good the assumptions are to reproduce the AOD during the both days.

18) p 2296 line 26 to p 2297 line 4: AOD and extinction coefficients even on 27 April when plume had not arrived are still so high, just comparable to the AOD or extinction coefficients on Haziest day during ASTAR 2000 campaign (Yamanouchi et al., 2005, Fig. 6 and 7). Fig. 12 is rather similar to the Arctic haze condition of Fig. 7 by Yamanouchi et al. (2005) for the lowest 2 km. Yes, 27 of April is showing high AOD values. We added the reference of Yamanouchi et al. (2000) several points in the revised manuscript. Please check point 5.1 where we refer to the similarity to ASTAR2000 campaign.

19) p 2297 line 15-16: "the 27 April 2006 which was already influenced by smoke particles" is surely confirmed as the comment in (18). Then it is better to show the calculation for the real background condition, with no influence of smoke. It might be explained from the temporal variation of AOD, if shown, 5 whole weeks. We are not intending to perform the analysis for a background case. This is already done in Treffeisen et al., 2005 for results of the ASTAR campaign. We put our focus on the beginning of the plume and the maximum of the plume showing the upper radiative influences possible.

20) p 2297 line 25: Following the discussions above, it is not appropriate to mention 27 April as "the pre-smoke day". We see the point of the reviewer here. Most of the time we actually used the term smoke day as well for the 27 May. We therefore revised the

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manuscript and used either pollution day or as well smoke day to characterise the 27 May.

21) p 2298 line 27 and p 2300 line 19: a factor of 3 on 27 April and almost a factor of 7 on 2 May > a factor of 7 on 27 April and almost a factor of 3 on 2 May We thank the reviewer for this and we changed the wrong order.

22) p 2299 section 5. 3: Difference in heating rate profile depending on the surface albedo is not so large, only 30 - 40 %; while the difference due to the mixing state is so grate. It is true that the effect of the mixing state (i.e. internal or external mixture) include a by far larger uncertainty as compared to the surface albedo. This dependence is recognized and discussed on page 2998. Nevertheless, a poor representation of the surface albedo could impose large errors to the calculations additional to those resulting from the assumption of mixing state.

23) p 2300 line 23-24: Can you show any evidence of this gradual change to the more internally mixed condition? Since no major sources are present during the last days of transport, these processes (i.e. coagulation and condensation) must move the aerosol towards an internal mixing state.

All technical correction mentioned are changed: (Technical corrections) 1) p 2278, line 14: 2003) Northern > 2003). Northern 2) p 2282, line 13: period end > period from the end 3) p 2287, line 12 and 21: It is needed to insert "," between "event" and "the mass fraction", between "measurements" and "although", respectively. 4) p 2293 line 24: absorption coefficients and Zeppelin > absorption coefficients at Zeppelin 5) p 2301 line 2: ground also > ground, also

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