Atmos. Chem. Phys. Discuss., 7, S5401–S5410, 2007
www.atmos-chem-phys-discuss.net/7/S5401/2007/

Atmospheric Chemistry and Physics Discussions

Interactive comment on "Regional aerosol optical properties and radiative impact of the extreme smoke event in the European Arctic in spring 2006" *by* C. Lund Myhre et al.

C. Lund Myhre et al.

Received and published: 28 September 2007

We thank the reviewer for the thorough and positive review and the time spent to help clarifying the paper with valuable suggestions. All comments are hereby answered one by one:

General comment. 1. Radiative forcing estimates. The section dedicated to the calculation of the aerosol radiative forcing is quite short, and none of its results are summarised in the abstract. For example, the abstract could state that the direct forcing in the European Arctic, which is normally around -5 Wm8722;2, reached up to -35 Wm8722;2 during this specific event.

We have now included a short summary the radiative forcing results in the Abstract.

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Some details are unclear: - Is section 4 still estimating the direct forcing? It is said so in the introduction, but not repeated here.

The section 4 is direct radiative forcing and to clarify we have now included the word 8220;direct8221; in the title and in the text in section 4 when appropriate.

- Is the forcing computed in the shortwave spectrum only?

The direct radiative forcing due to aerosols are dominated by the shortwave forcing since the longwave forcing is small except for mineral dust (Schulz et al., 2006; Reddy et al., 2005). We have thus neglected the longwave forcing. We have included the reference:

Reddy, M.S., Boucher, O., Balkanski, Y., et al. Aerosol optical depths and direct radiative perturbations by species and source type, Geophys. RE. Lett. 32 (12): Art. No. L12803, 2005

- Which exact sites provided the aerosol properties? Site names for each of the needed properties should be repeated here.

This is summarised in Table 1, and we have now included a reference to this table in the first paragraph in section 4. Further more details about the forcing calculations are included in this section as described in the subsequent comments.

- It is said that the expected forcing during the season is -5 Wm-2. This value is computed by using 8220;AOD values typical for the season and the same optical properties8221;. But the paper just demonstrated that the optical properties are those of a

transported biomass-burning aerosol. Surely they don8217;t apply to the typical Arctic haze aerosol.

We agree that our text is unclear and not precise regarding this point. Accordingly, we have changed the text in section 4, last paragraph; too better explain the forcing calculations performed.

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- What is the use of ECMWF cloud fields in the calculation? Is it an all-sky forcing? How often are aerosols overlying clouds?

To clarify we have added the following text: 8220;Three-dimensional cloud fields from ECMWF of cloud fraction, cloud liquid water content, ice water content are used in the calculation of an all-sky radiative forcing. For the vertical profile of the aerosols the LIDAR observations are used. Since the transported aerosols to the Arctic investigated here, are confined to the lower troposphere the aerosols do not normally to overlay the clouds.8221;

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- What is the surface albedo dataset used in the calculations? Is it representative of spring conditions?

The following text is added to describe the surface albedo data set better: 8220;The surface albedo data used in the radiative transfer simulation is spectrally resolved and varies with vegetation and ground characteristics (Myhre et al., 2003a). Snow and sea ice content are taken from the ECMWF and are included in the calculations of the surface albedo.8221;

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Minor comments. Abstract: - 8220;Importantly, at Svalbard it is consistency between the AERONET measurements and calculations of single scattering albedo based on

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aerosol chemical composition.8221; is not properly written. I guess the authors want to stress the fact that retrieved single-scattering albedos from sun-photometer measurements match quite well those computed from chemical compositions.

Correct, the sentence is rephrased to clarify.

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- 8220;This agreement is crucial for the radiative forcing calculations.8221; It is crucial for the confidence in the forcing estimates. RT calculations can be done with erroneous optical depths!

We agree, the sentence is rephrased to clarify.

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Introduction: - [Bellouin et al.] 8220;calculate a clear sky direct radiative forcing of -0.8 Wm8722;2 with a standard deviation of ś0.18221;. According to that paper, that value is the all-sky forcing. The clear-sky forcing is -1.9 ś 0.3 Wm8722;2.

Correct, the text is changed to 8220;calculate a all sky direct radiative forcing of -0.8 Wm8722;28221; as the comparing AeroCom value reported by Schulz et al., (2006) is for all sky forcing.

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- 8220;whereas water has a low albedo8221; should read 8220;liquid water8221; or, better, 8220;oceans8221;.

The text is changed to 8220;whereas liquid water and ocean has a low albedo.8221;

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- 8220;critical turnover value8221;: this expression is new to me. I guess it refers to the critical single-scattering albedo, at which the direct radiative forcing changes sign.

Correct, the sentence is slightly rephrased to 8220;Haywood and Shine (1995) showed

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that the critical turnover value of the SSA, where the DRF changes sign, is largely dependent on the surface reflectance.8221;

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Section 2: - 8220;in this study is shown Fig. 18221; should read 8220;in Fig. 18221;.

Done

——————— - 8220;Sodankyla Observatory, northern Finland (179m a.s.l.)8221; and 8220;Zeppelin Mountain (478m a.s.l.)8221;. The altitude is only given for those two sites, and not for the others. Are we to conclude that it is an important feature of those two sites? Yet, it is not discussed later in the study.

It is assumed that this of minor importance for the study, and the specified altitudes are removed for these to sites.

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- [Different] "automatic cloud-screening algorithms have been used in this work." If different cloud-screening algorithms apply to different instruments, it should be made clear which algorithm is applied to each kind of measurements.

The paragraph is rephrased and details for every instrument are added to clarify.

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Section 3.1: - MODIS collection 5 retrievals should be referenced, using Levy et al. JGR D13211 (2007).

The reference is included.

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- 8220;We have combined the data from Aqua and Terra8221;. How is this combination made, especially when both instruments provide retrievals in the same gridbox? A simple average is made?

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To increase the data coverage we have combined the data from Terra and Aqua by including Aqua data when there are no data from Terra available. We have not made an average. This information is now included in the text.

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- 8220;For [Sodankyla] we can see three peaks; the first elevated AOD peak on 27 April, which is also present in Ny-Ålesund, and a second peak on 2 May.8221; So written, the reader is under the impression that the second peak is not present in Ny-Alesund. In fact, only the third peak is not present in both sites.

The paragraph is rephrased and slightly changed to give a better and more accurate description of the time evolution of the AOD data at the Arctic sites. The peak on the 3 May is actually present at all the Arctic sites. However, due to the weather condition this peak is not as evident at Svalbard as in Sodankylä. The maximum value at Hornsund was 0.44 and at Ny-Ålesund 0.64 on this day.

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Section 3.2.1: - 8220;Note in this case the different scales compared to Figs. 4a and b)8221;. I fail to see the point of having different scales in Fig. 4. All data points can very well be presented on the same scale.

The Figure is changed in accordance with the suggestion.

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- 8220;with less than 0.1 absolute errors in for the nominal AERONET absolute AOD errors, 0.01-0.028221;. I don8217;t understand this statement. Are those the uncertainties in both Angstrom exponent and aerosol optical depth retrievals, or is it more complicated?

The error in the Ångström exponent depends on 2 factors: the errors of the AOD (around 0.01-0.02), which depend on the AERONET calibration, and the AOD itself.

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For high AOD (as it is in this case) the errors of the Ångström exponents are small. We have rephrased the text to try making this clearer.

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Section 3.2.2: - 8220;high surface albedo (snow)8221; in UVSPEC radiative transfer calculations. What specific value (or model) is used?

The wavelength dependent albedo for water is used (for example 0.06 at 500nm, 0.01 at 1000 nm). The data is taken from Wendisch et al. (2004). For snow is the wavelength dependent albedo (for example 0.89 at 500nm, 0.66 at 1000 nm taken from Bowker et al. (1985) used.

Bowker, D.E., R.E. Davis, D.L. Myrick, K. Stacy, and W.T. Jones, Spectral Reflectances of Natural Targets for use in Remote Sensing Studies, NASA Ref. Pub., 1139, June 1985.

Wendisch, M., P. Pilewskie, E. Jauml;kel, S. Schmidt, J. Pommier, S. Howard, H. H. Jonsson, H. Guan, M. Schrouml;der, and B. Mayer (2004), Airborne measurements of areal spectral surface albedo over different sea and land surfaces, J. Geophys. Res., 109, D08203, doi:10.1029/2003JD004392

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- 8220;It might be explained the deposition of the large aerosols during the transport8221; should read 8220;explained by8221;.

The sentence is corrected.

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- 8220;leading to the high the SSA in this region8221; should read 8220;the high SSA8221;.

The sentence is corrected.

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Section 3.2.3: - 8220;Only the peak value observed [on] 8 May at Hornsund is not captured by the satellite measurements. However, this peak is not detected at Ny-Alesund and it could be due to high cirrus.8221; Should this peak be due to cirrus clouds, I would have expected satellite retrievals to be affected as well, since cirrus screening of satellite observations is quite difficult to achieve properly.

Correct, and there are no MODIS data for the area on this particular day and this seems to be due to clouds.

Section 3.3: - 8220;Based on NOAA8217;s Hysplit model back trajectories8221;. It could be more efficient to include those back-trajectories as figures, instead of describing them in the text.

We prefer not to add an additional figure, as the general circulation patterns, the meteorological conditions along the route and the transport into the Arctic have been thoroughly described in detail by Stohl et al. (2007) and Treffeisen et al. (2007).

- The analysis of the aerosol vertical profiles seems to focus on the magnitude of the aerosol extinction. It is good to know that the extinction varies accordingly to the column-integrated optical depth measured in another manner, but changes in the altitude of the aerosol layer are not discussed. Are they related to changes in boundary layer height?

We feel that a more in depth study of the distribution of the altitude of the aerosol layer in relation to the boundary layer altitude is beyond the scope of our publication. The profiles have been taken at different times during the day as well as during night-time, which would require a much more complex analysis of the daily cycles and additional information than has unfortunately not been available in the frame of this study. From

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our lidar measurements at Andenes, we have not been able to derive the boundary layer altitude unambiguously at all times. This is due to the relative high lower cut-off altitude related to incomplete overlap during April/May 2006 and lack of simultaneous launched radio-sondes. Therefore we mainly focus on the variation of the extinction. In addition, information on the vertical stratification of the atmosphere has been given by Treffeisen et al. (2007). Also in Figure 11 of Stohl e al. (2007) the development of the normalized relative backscatter above Ny-Ålesund is shown. Few elevated layers (above boundary layer) can bee seen, on 3 May, and we estimate that they do not appear to have contributed significantly (> 10

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- 8220;British Isle8221; should read Isles.

The sentence is corrected.

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Figure 5: - Mention the role of the red curve (8220;normal conditions8221;).

This is explained in section 3.2.2, at the end of paragraph 2. We have now included a description of the red curve in the Figure caption as well.

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Figure 7: - Make clear that the comparison over Sodankyla is made at 3x3-degree resolution.

We have included this information about Sodankylä the Figure caption.

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Figure 8: - Would have been better to put Minsk at the top and Ny-Ålesund at the bottom, since the text describes the evolution from source to remote regions.

Done, and the figure caption is updated.

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References: Aoki et al. (2000), Bond et al. (2005) and IPCC (2001) are referenced but not cited in the text.

These references are now removed.

Birch and Cary (1996) is cited but not referenced.

This is now included.

Myhre et al. (2003a) and (2003b) are both numbered 2003a.

This is now corrected.

Shiobara et al. (2003) and (2006) are not listed in alphabetical order: I missed them at first!

This is now corrected.

Thank you!

Interactive comment on Atmos. Chem. Phys. Discuss., 7, 9519, 2007.