

Interactive comment on “An empirical model of optical and radiative characteristics of the tropospheric aerosol over West Siberia in summer” by M. V. Panchenko et al.

Anonymous Referee #2

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The manuscript, “An empirical model of optical and radiative characteristics of the tropospheric aerosol over West Siberia in summer” by M. V. Panchenko et al., provides the description of an interesting empirical aerosol model that was derived based on several years of ground-based and aircraft optical and microphysical aerosol data over West Siberia. The model was tested using radiative flux calculations during the summertime over Siberia. I believe the study does contribute to our understanding of the influences of aerosol parameters as inputs to radiative transfer models, but I recommend the following major changes before publication.

General Comments:

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1. The grammar needs to be cleaned up considerably prior to publication. There were too many individual grammatical errors to be listed in this review.
2. The abstract should be expanded to include some specific results and details. Currently, it is difficult to glean any real information from the abstract.
3. I’m not sure what is meant in the last line of the abstract that the regional features of West Siberia are taken into account. This should be clarified or removed.
4. The introduction should be expanded to include a greater review of the current literature regarding the subject.
5. Even though references are given for the methods, it would be nice to have some information – even if it’s just a list of the instrumentation and measurements, etc. in a table.
6. The first paragraph of section 2 refers to methods (although this should be expanded – see above comment), but the rest of this section should be combined with the introduction section.
7. The discussion of the results of the radiative flux simulations should be expanded. It would be nice to see summertime radiative forcing calculations using the empirical model input parameters and how these compare to current estimates over this region.

Specific Comments:

1. Figure 1: change km⁻¹ to Mm⁻¹ and remove the logarithmic scale – it is misleading. Also, it is difficult to tell which error bars go with which measurement in the figure. They should be color coded to make it clearer. What do the error bars represent in this figure? It is impossible to say whether the profiles from the two time periods agree without this information.
2. I don’t think that just because the scattering profiles for the two time periods agree (if they even do – see comment above), the aerosol absorption properties necessarily

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would as well, as I believe is being stated starting on line 11, section 3.1, pg. 141. Also, where does the value τ come from without mass concentration measurements of the aerosols?

3. Section 3.1, line 23, pg. 141: what does 0.05-0.7 μm represent? The aerosol diameter? This should be clarified.

4. Section 3.1, line 5, pg. 142: what is the meaning of the following, 0.2 (0.3)–5 μm ? Why is 0.3 in parentheses?

5. What aerosol diameters are capable of being sampled through the aircraft aerosol inlet? Again, this information should be tabulated and included in this paper. It is briefly mentioned in the last paragraph of section 3.1 that larger particles are not sampled as well. What is the collection efficiency with size? Has this been measured?

6. Please use a more conventional symbol for single scattering albedo than ω —such as ssa or ω .

7. Section 3.3, line 8, page 143: the bracket after 2002 should be removed.

8. The literature on which the assumption is based that 90% of black carbon is in the submicron fraction and 10% is in the coarse fraction seems rather limited. I have a hard time believing that assumption based on such limited references.

9. Section 3.3.1, second paragraph: The Terpigova, 2005 reference is for a conference abstract. This is an important parameter, the mean seasonal values of the black carbon mass concentration, and there should be a proper, peer reviewed reference for these data.

10. Page 145, line 9: Why was the activity parameter different at the surface and above the surface? Were you encountering different aerosols at the surface and aloft?

11. Figure 3: Why are the Angstrom exponent values listed on the AOD plot? Also, please be consistent with using either Angstrom exponent or Angstrom parameter

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throughout.

12. Figure 3: Were the samples dried prior to your nephelometer measurements? It appears that there may be a strong RH dependence in the single scattering albedo measurements as the altitude approaches the atmospheric boundary layer. Large changes in RH (as near the boundary layer) will cause a strong dependence in the scattering coefficient values if the samples are not dried. For example, in the Andrews et al. (2011) paper cited, as you noted, the single scattering albedo does not have a large increase near the boundary layer. That is because their samples are dried prior to measurement.

Interactive comment on Atmos. Meas. Tech. Discuss., 5, 137, 2012.

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