Atmos. Chem. Phys. Discuss., 3, S1854–S1858, 2003
www.atmos-chem-phys.org/acpd/3/S1854/

Atmospheric Chemistry and Physics Discussions

Interactive comment on "Climatological aspects of aerosol optical properties in Northern Greece" *by* E. Gerasopoulos et al.

E. Gerasopoulos et al.

Received and published: 10 November 2003

The authors are grateful to the referees for the constructive and insightful comments on the manuscript. Minor comments and corrections of technical type have been taken into account and the text was changed accordingly. Moreover, interesting comments were made during the quick review process, which are also included below.

Reply to reviewer 1

1) Title is misleading: climatological aspects suggest periods of 10 years and more... Appropriate title: Particle optical properties in northern Greece

Even though the reviewer's point is in general correct, we believe that the title is not misleading since very few studies run this long in our area. Nevertheless, if the editor also thinks that it has to be changed we could use "A long-term study of aerosol optical properties in Northern Greece"

Full Screen / Esc
Print Version
Interactive Discussion
Discussion Paper

2) page 2060, 19: aerosol scale height is defined as that height of an exponential profile at which the value is decreased to 1/e of the ground value. But I guess this scale height is not ment. Please improve!

In our paper we use the term aerosol scale height, as the equivalent depth of the optically active aerosol layer of the atmosphere at an assumed constant pressure equal to the surface pressure, which can be statistically estimated by combining the extinction coefficient, σ_{ep}, and the AOD at MAO, as also referred by Formenti et al., 2001. This has been clarified in the text following the suggestion of the reviewer.

3) page 2061, 15: How is the hemispheric background troposphere defined? An average from the pole to the equator, over oceans and continents. Who has measured such a value? Is it derived from satellite observations? If not, please avoid such notion.

As mentioned in Lelieveld 2002 paper values over North Pacific Ocean have been used as hemispheric background troposphere since it is expected to be the least polluted environment in low northern latitudes. An appropriate change, clarifying this definition, has been included in the manuscript.

4) page 2061, 23, Lelieveld 2002 paper is related to gaseous pollution, not to aerosols. So, please change. I have never seen any paper concerning aerosol transport from southern Asia to Europe because that has never been found by observations and satellite remote sensing. Are there no lidar-related papers dealing with long-range transport of particles in the free troposphere that could be cited?

In this paragraph, an attempt to reveal the special role of Eastern Mediterranean is made in the general sense of pollution either gaseous or aerosols. That's why Lelieveld et al. 2002 paper is extensively cited. Furthermore, as shown by models, aerosol transport follows the same patterns as gas transport. However, a number of references one of which is related with lidar measurements have been added according to the reviewer's suggestion.

ACPD

3, S1854–S1858, 2003

5) page 2063, section 2.3: What's about artifacts in the nephelometer observations by humidity effects? The particles get heated in the nephelometer and dry. Isn't that a problem? The scattering coefficients may be smaller than the ones for ambient conditions. One may discuss that problem when trying to estimate the PBL height from photometer/nephelometer comparisons. 20 percent reduction caused by particle heating may result in 20 percent lower PBL heights (after the correction of the 20 percent reduction in the scattering properties).

The reviewer is absolutely correct. Actually, particles are drier in the nephelometer than outside and they are also drier at the surface than at higher levels in the mixed layer. This may cause a decrease in the scattering coefficients which is difficult to quantify with the available data. Thus, the estimated value for H should be considered as the upper limit of aerosol scale height. This consideration has been pointed out in the manuscript.

6) page 2065,19: How can comparisons of nephelometer data with photometer data give an idea about high thin cirrus? One can never be sure that ground observations are unbiased (local pollution impact can never be excluded). So, there is no link between ground values and high aerosols or cirrus layers. Leave out!

The reviewer is correct that there is no direct link between these two and it could be used just as a first indication needed further investigation before becoming a conclusion. Thus, this part of the discussion is removed as suggested by the reviewer.

7) page 2066, 14-20, Please specify in detail how the Angstrom values are calculated, what wavelengths are explicitely used. The German Weather Service published a ten year observation of optical depth (Weller et al., Ten years of aerosol optical depth observations .... at the Lindenberg Meteorological Observatory, Contributions of Atmospheric Physics, 71, 387-400, 1998) should be cited. They discuss the Angstrom exponent in detail.

As suggested by the reviewer the equation that the Angstroem coefficients have been

ACPD

3, S1854–S1858, 2003

calculated from and the wavelengths used were specified in the text. The recommended reference has been also cited.

8) page 2073: I have my doubts that RH values measured at ground can be used to interprete the diurnal cycle of optical depth. Nothing is known about the humidity conditions in the upper part of the PBL. But the moisture in the upper part may have the strongest impact on the optical depth.

The reviewer's point is right and that is why the correction for RH has been focused on the nephelometer observations. Also the fact that the diurnal amplitude in AOD is not smaller than in scattering coefficient suggests that additional processes are at work, such as the see-breeze effect alluded to in the manuscript. To clarify the difficulty of interpreting humidity effects in the column based on surface measurements an appropriate statement has been included in the text.

Reply to quick review

1) Aerosol optical properties are strongly dependent on relative humidity and thus on solar heating of the surface layers and boundary layer. However, vertical mixing (dilution, stagnation) and photochemical production of aerosol particles and particulate mass are also solar related and should be analyzed as well in the context of diurnal variability.

We certainly agree with the reviewer but we had no data available on which to base this type of analysis.

2) The RH effect is strongest at the surface where the nephelometer measurements were made and less of an influence on the column integral optical depth measurement.

As in question 7 of referee 1.

3) I doubt that "real background" in a geophysical sense is present for extended periods of time as implied by the statement. The LAP and MAO AOD data should be presented in geophysical context with comparisons to other long term data from European or

ACPD

3, S1854–S1858, 2003

Northern Hemisphere sites.

We used the term "real background" in the sense of regional background, thus we have now changed the definition "real" to "near background" to solve the misunderstanding. In the first paragraph of section 3.1 there is a statement that our AOD values can be compared with literature values classified in Formenti et al. (2001b). Also, there is some discussion in section 5 (Conclusions). However, after the reviewer's suggestion, we have now added in section 5 (Conclusions) a number of additional references and commendation to facilitate comparison of our AOD with other data.

Interactive comment on Atmos. Chem. Phys. Discuss., 3, 2059, 2003.