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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Size-segregated mass distributions of aerosols over Eastern Mediterranean: seasonal variability and comparison with AERONET columnar size-distributions" *by* E. Gerasopoulos et al.

Anonymous Referee #1

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This paper is clear and well written. The authors present data from long-term aerosol mass size distribution data from an SDI impactor on the island of Crete, Greece. The data is evaluated against data from a PM_{10} monitor. Additionally the mass size distributions from the impactor are compared to volume size distributions retrieved from an AERONET station in the area to evaluate the correlation between surface and columnar average concentrations. Various modes of the size distribution were examined to determine the likelihood of various particles sources affecting the region (e.g. European pollution outflow and Saharan dust). The paper is of high academic quality,

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contributes important information regarding aerosol size information in Southern Europe to the atmospheric research community and deserves to be published. I have several comments that should be addressed.

1. Proofread for spelling and grammar. Make sure you catch that there is a "Rondriquez et al., 2001" that is cited throughout the paper; however, there is a Rodriguez 2001 in the references.

2. Page 472 lines 5-7. Specify if this dry diameter or ambient diameter (or something else).

3. Page 472 lines 24-26. Please clarify this sentence. Does this mean that the error in dV/dD$_p$ is 10% at D$_p$ values where dV/dD$_p$ is at a peak and 35% at D$_p$ values where dV/dD$_p$ is very low, or something else?

4. Page 473 lines 14-15. "PM$_{10}$ and PM$_{2.5}$ show greater sample to sample variability..."

5. Section 4 or maybe Section 2.1. It would be informative to know how close to the impactor the nearest anthropogenic aerosol sources are and what their effects might be on the data. The reader could just look in Mihalpolous et al. (1997) cited in Section 2.1; however, it would be nice if this was discussed briefly.

6. Section 4.1 and Figure 5. The fit of the impactor data to the four modes for Autumn and Summer seems not optimal (e.g. the extreme artificial minima in dm/dD$_p$ in between modes). I'm sure this is due to the cited inversion code and perhaps due to the code preventing modes from having GSD values larger than 2. It seems as if the overall fit of the Autumn distribution would benefit from the two more minor modes having larger GSD values and the larger of the two coarse modes having a smaller peak value. This does not, however, greatly detract from

ACPD
7, S93–S95, 2007

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this section because it is somewhat tangential to the main points of the section, though it may have an affect on Figure 10 (addressed later).

7. Figure 8. Change y-axis to D_p, rather than log(D_p) if possible.

8. Page 481 line 9. The distributions derived from AERONET were volume size distributions, not mass size distributions.

9. Page 481 lines 19-23. In the assumptions about particle density, the reduction of density due to water uptake is not discussed. It might be interesting to add some discussion on the sensitivity of the results of Section 4.3.2 and Figure 9 on the density assumptions.

10. Page 482 lines 19-22 and Figure 10. In this comparison are you using the modal fits of dm/dD_p like in Figure 5 and Section 4.1? It seems like using the peak values of the modes in these fits might add error to the comparison in Figure 10. For example, the peak of the largest diameter mode in the Autumn panel of Figure 5 is much greater than what the impactor measured. Another option for this (Figure 10) comparison might be to compare the total supermicron (or some other size cutoff) mass from the impactors versus the total supermicron volume from AERONET as I would guess that integrated size distributions would have less error than the peak values.

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7, S93–S95, 2007