

Interactive comment on “Aerosol mass closure and reconstruction of the light scattering coefficient over the Eastern Mediterranean Sea during the MINOS campaign” by J. Sciare et al.

Anonymous Referee #2

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The paper "Aerosol mass closure and reconstruction of the light scattering coefficient over the Eastern Mediterranean Sea during the MINOS campaign" by J. Sciare et al. is well within the scope of ACP. The authors performed an extensive analysis of aerosol samples collected during the field project "MINOS" in summer 2001 on the Mediterranean island Crete. The objective of the analysis was the closure between chemical and gravimetric inferred aerosol mass, and the closure between measured and calculated scattering coefficients under ambient RH conditions. This objective was achieved, and the authors highlight the importance of separating the fine and the coarse mode, since their chemical properties are completely different. Also, the importance of the wa-

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ter uptake of the aerosol particles on the scattering coefficient is emphasized. These results are of great importance for the aerosol science community. The objectives, the methods and the conclusion drawn are presented clearly and necessary assumptions and uncertainties are thoroughly discussed.

Overall, this is an excellent paper that I recommend for publication, and I have only some minor remarks/questions:

Section 2.1 There is reference to unpublished data by Minikin et al. about two aerosol layers. Are there no published data on these layers available from the large MINOS data set?

Section 4.4 To my knowledge (I'm not a filter sampling expert), NO₃ is known to evaporate easily from filters, especially at high temperatures that prevail in the Mediterranean during summer. Does this affect the presented data?

Section 5.4 The factor analysis would benefit of a more detailed explanation. The authors give correct references, however, better understanding by the reader could be achieved if the general idea of the factor analysis was indicated. In the same section, the authors express their doubt in the validity of this method by stating that the conclusion that biomass burning is the exclusive source of the organic aerosol contradicts the results by their earlier analysis (Sciare et al., 2003b). However, the conclusions in Sciare et al. (2003b) rely on the use of the BC/nss-SO₄ ratio, which may not be conserved and therefore this method might also lead to erroneous results. Here the reader is left with the rather vague statement that both results are contradictory.

Figure 2 Couldn't the size distribution of BC and OC be included in Figure 2 (even though from a different impactor and thus at different size classes)?

Typos:

Table 1: Second line: columns 2 and 3: "size fraction" instead of " $\mu\text{g}/\text{m}^3$ ", and "samples" instead of " $\mu\text{g}/\text{m}^3$ "

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Interactive comment on Atmos. Chem. Phys. Discuss., 5, 2427, 2005.

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