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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Mesoscale modeling of combined aerosol and photo-oxidant processes in the eastern Mediterranean" *by* M. Lazaridis et al.

Anonymous Referee #2

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The paper presents a comprehensive modelling study in the Eastern Mediterranean using the UAM-AERO model initialised with CTM-NILU outputs and compared with field observations from the SUB-AERO series of field investigations, for July 2000 and, with fewer observation data, January 2001. Specific attention was paid to the ability to predict ozone and a variety of aerosol components. The stated aims were to improve understanding of the background gas & aerosol atmospheric composition, and to compare the behaviour of model outputs and measurements between summer and winter. Various model sensitivities were noted, particularly that to resuspended mineral aerosol components. Overall, the modelling study is described fairly clearly and the methodology is reasonably appropriate. I believe the paper could benefit from attention in both presentation and discussion of the model performance. Once these are addressed, the paper should be publishable.

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The issues are wrapped up in the general comments below:

p5466, line 20 to 26, Figure 6 shows EXTREMELY loose relationship between the model and the measurements for O3. To claim that the model captures the characteristics and features of the measurements and shows quantitative or qualitative agreement, must be backed up with some statistical analysis. It is not good enough to state that there is agreement, particularly when this agreement looks so poor at first sight. It is not sufficient to state that a statistical analysis of model performance is beyond the scope of the study; this would imply that an assessment of the model suitability is also out of scope, but the conclusions state that the model is an appropriate tool for the application. Indeed, an attempt to analyse and explain in the text the systematic differences between preicte an modelled ozone should be made with particular reference to the likely limitations of the CB-IV mechanism and the likely huge contribution to the ozone forming potential of the biogenic emissions so high in this region.

p5466-7, Figure 7 shows some interesting results. Firstly, it would be useful for all panels to have the same formatting, preferably not the 3-D Excel style which adds no information. It is obvious that the predicted summer crustal contribution to PM10 is greater than the winter levels, presumably due to the source term dependence on soil state / ground dryness. The third panel shows very elevated seasalt PM10 components on 10th and 14th January. Can the authors explain the to elevated seasalt to crustal ratio on 10th Jan, compared to the same ratio on 14th Jan given both components of PM10 have wind-driven mechanical generation as a source?

Do the authors think that the majority of the problems with total loading are due to poor representation of the crustal source term? How do they know? There is not enough information in the tables or figures to explain the discrepancies in loadings. How do the other individual components compare? Which components are underestimated most (given there is a little underestimation of sulphate and a slight overestimation of seasalt)?

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The tables should give the range (or some other indication of variability) and possible uncertainties as well as the average values for the measurements (and predictions). It is not possible to see in which components the discrepancies in the PM10 aerosol mass loadings in figure 8 arise. It is noted that the authors state that comparison of RAMS output with measured meteorology is outside the scope of the paper. However, since meteorological factors are directly responsible for seasalt and dust loadings, this aspect should at least be given passing appraisal.

p5469 Based on the above presentation of the data, it is not possible for me to assess whether the conclusions (that UAM-AERO/RAMS is an appropriate tool and that the importance of photooxidant and fine aerosol dynamics is important) follow from the work. In particular, p5466-5468 needs to present a fuller discussion with clearer figures / tables to enable the conclusions to be assessed. For example, nowhere other than the conclusions is it stated that unresolved particulates are the major contribution to the discrepancy (nor is it stated elsewhere that these unresolved particulates are Saharan dust or forest fires). In addition, the last paragraph of the conclusion states that the main contributors to particulates are sulphate, seasalt and crustal. Is this based on the composition from the modelling shown in figure 7 or is this based on measurements. This should be clarified and the unresolved mass put into perspective.

Presentation issues:

p5460, line 3-4 - inner black rectangle - the innermost one corresponds to Crete, should colour this one red, for example, to distinguish it from the complete model domain.

p5461, line 23 - which are the "initial" concentrations? - needs rephrasing to specifically refer to the CTM-NILU output concentrations as inputs to UAM-AERO.

Figure 1 caption and elsewhere - ppb is not a unit of concentration - it is a mixing ratio and must be referred to as such.

p5462, line 11 - it is stated that UAM-AERO uses CB-IV with 47 species, but on p5461,

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line 6, "...for the 24 gaseous species considered in the model..." Presumably this refers to the CTM-NILU model later in the sentence, but it should be rephrased to avoid confusion. Readers do not know to which model "...the model" refers.

p5463, line 5 to p5464, line 9 deserves and requires a regional map showing the locations mentioned. Whilst a broad appreciation of regional geography should be expected of the reader, the exact relationship of the model domain to the discussed features is hazy, particularly for a reader (and reviewer!) of non-Mediterranean origin. A discussion of broad synoptic conditions is valuable, but requires graphical illustration.

p5464, I agree wholly with the discussion of the mixing ratios (NOT concentrations) in Figure 2, but was extremely confused for some time by the label at the top of the panel in figure 2g) which states that the figure show EC not resuspended dust and obviously couldn't understand why Crete was such a source of EC without significant CO or NOx emissions! This label must be removed / replaced.

p5464, Figure 3 is an entirely unclear way to present daily variation, particularly since the concentration scales vary. It would be better to use line plots of concentration vs. time of several representative marine, island and continental locations.

p5465, Figure 4 is an incredibly space intensive way to show regional transport of pollutants and I'm not sure what its purpose is (the exercise described on p5466 lines 8 to 19 provide a much better discussion of long-range transport). It is good to know that the model moves pollutants around satisfactorily, but I'm not sure the large number of panels adds to the science in proportion to the space taken up.

Both figures 3 and 4 appear relatively clumsy. As an online journal, does ACP not have the capability to use animations? This would solve both problems of representation and save considerable space.

p5465, Figure 5 - the captions state that the ozone profiles are from CTM-NILU. Is this correct? If so, where is the output from UAM-AERO and how does it differ? Or

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Interactive Comment

does it possibly mean that the model output is from UAM-AERO with initialisation from CTM-NILU? This should be addressed in all figure captions.

Technical corrections:

p5460, line 15 - "quite interesting", subjective & non-specific, rephrase

p5461, line 23 - The "also" refers to "in addition to" what?

p5462, line 5 - clumsy sentence - "for having minimum effect on..." is not English. Presumably this refers to the model spin-up time and the minimisation of the model sensitivity to the initial conditions.

Interactive comment on Atmos. Chem. Phys. Discuss., 4, 5455, 2004.