

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

### **Anonymous Referee #1**

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This paper details an assessment of emissions sources during an intensive field campaign held in southeastern Greece, where measurements were made concurrently at 1 field station (Finokalia) and onboard a research vessel in the Aegean Sea. The assessment uses the UAM-AERO model with RAMS meteorological input. The paper presents novel work in that source apportionment of gaseous and particulate pollution has never been undertaken for the eastern Mediterranean area. The study area is interesting because of the location, midway between the northern African coasts (subject to transport of crustal dust from the dry African deserts and plains) and southern Europe, where industrial activity is dominant.

### General comments

My main concern on reading the paper is the lack of data that is shown. The intensive

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summer measurement campaign was four weeks long, yet only 9 days are investigated, leaving 66% of the data redundant. In most modelling studies there is a great temptation to only show the periods of data where the model and measurements match very well, and to leave out those periods that don't agree so well. This is a shame, as often it is the non-correlating periods which can point the way towards why the modelling isn't working so well at times, and sensitivity studies can be performed. This work does not question the sensitivity of the model, nor give an indication of the model uncertainty. No model can provide us with 100% certainty in the results. Indeed the accuracy of the measurements is not commented upon either.

#### Comments on the model treatment of emissions

1) On page 5461 line 11, special attention is given to the emissions of biogenic NMVOC, especially isoprene and terpenes, to the model domain. These biogenic compounds are then not mentioned again (other than to say isoprene is lower in winter than in summer on page 5469), either in the results or as a source of ozone production. Indeed the major causes of the ozone produced in the model are not discussed, yet the title of the paper suggests that a study of the photo-oxidant processes in the study area will be examined. As ozone is not emitted to the model, an investigation of the sources would make interesting reading. However little chemistry is mentioned, other than the scheme used in the model, the Carbon Bond IV approach, degrading 47 species via a lumped reaction sequence. Is it therefore likely that the limited chemical scheme could be causing some of the model under-prediction? The limitations of the chemical scheme need to be tested.

2) Much of the paper is dedicated to the emissions, yet in contrast, nothing is mentioned of the deposition of the particles in the model. It is also not clear how the model has been run, other than to give a 24 hour lead-in time to allow the initial conditions to settle down. Has each day been run separately, or has the whole of the summer period been run all at once for example?

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3) It appears that the crustal dust emission inputs have been implemented using work previously completed by two of the authors (Alexandropoulou and Lazaridis, 2004). Yet one of the major conclusions in this work is that the model is under-predicting crustal dust! The reason for the model under-prediction is attributed to the Saharan dust episodes and wild forest fires, but I suspect these events are both seasonal. For example I would not expect forest fires to take place in winter. Yet in tables 1 and 2 the average modelled crustal dust is over-predicted in summer and under-predicted in winter. The winter period neither covers the dust episode nor the wild fires.

4) Gas phase NO<sub>x</sub>, SO<sub>2</sub> etc are emitted to the model, but are then treated as particles (NO<sub>3</sub>, SO<sub>4</sub> etc) later. There is no mention of a gas-to-particle conversion routine. How is this treated?

Model and measurement comparisons

5) The results are shown, though no further analysis takes place - there are no attempts at correlating the modelled and measured data statistically, or for example, to conjecture why the model did a better job on 9th Jan rather than 11th Jan in fig 10 (mentioned on page 5468). More model diagnostics and sensitivity tests are required.

6) The authors often state that they are satisfied with the model performance, even though in many cases the model has under-predicted the measurements by a factor of  $\sim 2$  (e.g. early ozone concentrations in fig 3b, where there is a model to measured difference of 20 ppb), yet nothing is discussed other than what might be missing from the emission inventories. Could there not also be a problem with the model treatment of the emissions/deposition for example? Or even the chemical production of ozone?

7) It is stated on page 5470 that the model/measured comparison is better in winter than in summer, but this is not always the case. Tables 1 and 2 detail equal model under-predictions in total PM<sub>10</sub>, by factors of 1.61 and 1.55 for summer and winter respectively.

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## Other comments

8) Approximately 90% of the modeling domain is sea, yet the research vessel measurements are only used to compare modelled data in figures 5 and 6 and in table 4. Can a comparison between the modelled and measured aerosol composition at sea be shown (as undertaken at Finokalia in figure 7)?

9) What also would have been interesting is to compare the measurements of the meteorology made during the intensive field campaigns with that of the RAMS meteorological model - but I expect that would be material for a separate paper! Could a model/measured discrepancy in the meteorology in the model account for the discrepancy on 11th January in figure 10 for example?

## Technical corrections

Page 5458 line 6. Insert “the” into “SO<sub>2</sub> levels in (the) eastern Mediterranean”.

Page 5459 line 20. remove “s” from end of “concentrations”.

Page 5460 line 23. remove “the” from “performed at the Finokalia”.

Page 5462 line 6. rewrite sentence. Try “Simulations were initiated 24 hr before the beginning of each modeling period, to reduce the effects of the initial conditions on the final results”.

Page 5464 line 11. Inset “the” into “ammonium in (the) aerosol phase”.

Page 5464 line 22. change “infiltrate” to “infiltrating”.

Page 5464 line 29. remove “The” from “The fig 3”.

Page 5465 line 5. remove “then” from “at this time then the wind”.

Page 5465 line 15. capitalize the d in Saharan Desert. Insert “region” after “the black sea (region)”.

Page 5465 line 29. remove “the” from “during the January 2001”.

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Page 5467 line 27. you mention a Saharan dust episode on 12th July, yet this day is not modelled.

Page 5469 line 11. rewrite sentence. "it can be resulted" does not make sense. Try "it can be concluded" instead.

Page 5514 figure caption. Should read "January 2001", not "January 2000".

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