Atmos. Chem. Phys. Discuss., 8, S998–S1001, 2008
www.atmos-chem-phys-discuss.net/8/S998/2008/

Atmospheric Chemistry and Physics Discussions

Interactive comment on "The role of sea-salt emissions and heterogeneous chemistry in theair quality of polluted coastal areas" *by* E. Athanasopoulou et al.

Anonymous Referee #2

Received and published: 26 March 2008

This manuscript presents a regional nested model study over the Greece area, in which new sea-salt emission algorithms have been taken into account. The aim was to have a better representation on the sea-salt emissions, especially over regions close to the coastline. Gas-to-particle conversion via heterogeneous processes has been included in the model and its results were compared with measurements, but from different time periods from that the model simulated.

The paper is well written, the methodology followed is clear and the approach used is original. The main flaw of the paper is the comparison with measurements: for the period that was selected to be simulated no measurements are available, which led

the authors to compare their results with measurements with other time periods. Although this can be acceptable, no arguments exist on why that period was selected for modeling, since the selection of another year and/or season could coinside with measurements and whould had made the comparison straightforward. There are some intensive campaigns throught Greece, why they didn't select one of them? For that reason (to my opinion), the performance of the model over previous modeling approaches is difficult to support, since the comparison with measurements with the "order of magnitude" agreement is very weak. At the same time, it is difficult for one to claim that the nested approach improved the model performance.

As a conclusion, I favor the publication of the present manuscript in ACP, after improving the model vs. measurements comparison, and after taking into account the minor comments that follow.

Detailed comments:

1) p. 3813, l. 21: "urban characteristics" should be specified; is it emissions, elevation, meteorology, others?

2) p. 3815, l. 3-5: How about domestic emissions? Further, were there any biomass burning events during the studied period?

3) p. 3817, l. 1-3: This is a real result, or it is just the effect of the grid-size? If there was no nesting over that domain, would that result remained the same or not?

4) p. 3818, l. 23-24: There are HNO3 measurements aroung Greece that the authors can compare their model with. As an indication, see references 1-5.

5) p. 3819, l. 7-8: Why not making a simulation with double or triple ammonia emissions and see how this affects the results? This will be a good indication on the author's conclusion on the ammonia emissions, and it would be interesting to see how the nitrate partitioning will change, already mentioned on p. 3818, l. 24.

6) p. 3820, l. 17-28: It would be interesting to have a figure with the daily variability of

ACPD

8, S998–S1001, 2008

aerosol composition over certain sites.

7) p. 3822, sect. 5.4: This is a scary result, it means that in order to have a correct sea-salt representation in models we need to have very high resolution? How this will affect models with not so high resolution? A comment about it would be very interesting.

8) p. 3822, l. 10-12: Led to grated PM10 levels compared to what? A simulation without sea-salt? A simulation with "traditional" sea-salt sources? A simulation without gas-to-particle conversion? I guess the first, but it has to be clear in the text.

9) p. 3822, l. 22: At Thrakomakedones, the concentration dropped to 1ug/m3 of the xxx ug/m3: xxx should be added here.

10) p. 3823, l. 3-5: It is not possible to say that the finer grid performs better than the coarse one, since no proper comparison with measurements was made (see general comments) in order to validate whether this is an improvement or not.

11) Figure 5: CL06 and G-M03 are very discontinuous (this is a log-scale), how does this affect the results, especially close to the discontinuity? Further, how did the authros selected the size threshold of the two parameterisations used?

Technical corrections:

1) p. 3809, l. 5: "life" should be "lifetime".

2) p. 3809, l. 22-23: "describes" should be "describe".

3) p. 3816, l. 2: "combines" should be "combine".

4) p. 3822, l. 19 and 23: "on land" whould be "above land".

5) Table 1 legend: RH is in percentage or franction from 0 to 1?

6) Figure 4 legend: SSA emissions are in strange units, why per cell instead of a more comprehensive per surface area?

References:

ACPD

8, S998–S1001, 2008

1. Anatolaki, C; Tsitouridou, R, Atmospheric deposition of nitrogen, sulfur and chloride in Thessaloniki, Greece, ATMOSPHERIC RESEARCH, 85 (3-4): 413-428 SEP 2007

2. Metzger, S; Mihalopoulos, N; Lelieveld, J, Importance of mineral cations and organics in gas-aerosol partitioning of reactive nitrogen compounds: case study based on MINOS results, ATMOSPHERIC CHEMISTRY AND PHYSICS, 6: 2549-2567 JUL 3 2006

3. Ferm, M; De Santis, F; Varotsos, C, Nitric acid measurements in connection with corrosion studies, ATMOSPHERIC ENVIRONMENT, 39 (35): 6664-6672 NOV 2005

4. Danalatos, D; Glavas, S, Reactive nitrogen compounds at a Mediterranean coastal site, FRESENIUS ENVIRONMENTAL BULLETIN, 11 (11): 1008-1017 NOV 2002

5. Kouvarakis, G; Mihalopoulos, N; Tselepides, A; et al., On the importance of atmospheric inputs of inorganic nitrogen species on the productivity of the eastern Mediterranean Sea, GLOBAL BIOGEOCHEMICAL CYCLES, 15 (4): 805-817 DEC 2001

Interactive comment on Atmos. Chem. Phys. Discuss., 8, 3807, 2008.