Atmos. Chem. Phys. Discuss., 12, C667–C669, 2012
www.atmos-chem-phys-discuss.net/12/C667/2012/

Atmospheric Chemistry and Physics Discussions

Interactive comment on “A model study of the Eastern Mediterranean ozone levels during the hot summer of 2007” *by* Ø. Hodnebrog et al.

Anonymous Referee #2

Received and published: 16 March 2012

This is an interesting model study about surface ozone levels as well as emissions and concentrations of air pollutants from wild fires in the Mediterranean region. The paper is well presented and most of the conclusions seem sound. The following general and specific comments should be considered before publication in ACP.

The paper includes substantial discussion on the evaluation and results of model simulations of fire emissions. This would merit a reflection in the title of the paper.

The authors suggest that the CO/NOx ratios in the fire emission data need to be revised. However, this is mainly based on the results from the WRF-Chem model which also seems to put most of the emissions close to the surface which means that the concentration contribution compared with the satellite data is minor due to the averaging kernels. Could it not be that underestimation of vertical mixing could explain the discrepancey between observed and simulated CO columns? The EMEP model is not used in this evaluation with the argument that CO concentrations in the upper troposphere are not realistic. What would be the result if a composite of EMEP results for the lower 2-3 km and WRF-Chem for the rest of the atmosphere were compared to the satellite columns? Would the same conclusions hold?

The difference in simulation surface ozone between the models merits some more discussion. The authors rule out differences in emissions. Could the higher surface ozone concentrations in WRF-Chem be due to less vertical mixing? Can inspection of vertical profiles of ozone or intercomparison of boundary layer heights or similar give some clues? This should be discussed.

Dry deposition is a very important loss process for ozone. It is unclear from the paper what is meant by the temperature effect on dry deposition. Do any of the models directly account for soil water availability in their calculation of stomata conductance? What is the difference between this effect and the temperature effect? This should be explained better.

Specific comments

p7618 l9 Suggest the wording "climate change impact research" instead

p7619 l25 What is meant by temperature dependency of dry deposition? Please specify. This is also needed in the abstract.

p7623 l12 Was Oslo CTM2 driven by the same meteorological data as WRF-Chem? Please help the reader although this information is available in the given reference.

p7623 l10 Which meteorological variables were nudged?

p7624 l13 Consider mentioning the chemical boundary conditions in the text for the EMEP model although it is given in table 1. Inconsistent to only mention boundary

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conditions for one of the models in the text.

p7637 l13 "very similar" is a too strong statement here since the results from WRF-Chem are clearly higher.

p7640 l13 Did Im and Kanakidou use the same isoprene emissions in their study? Please help the reader.

p7644 l16 I suggest writing "model results using WRF-Chem." Since the EMEP model was not fully compared to the CO measurements.

p7667 Fig 7 Why are results shown only for part of the domain for the EMEP model? According to fig 1 the EMEP domain covers the whole area displayed in the figure.

Interactive comment on Atmos. Chem. Phys. Discuss., 12, 7617, 2012.