Atmos. Chem. Phys. Discuss., 3, S204–S206, 2003
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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Distribution and origin of ozone in the eastern Mediterranean free troposphere during MINOS (August 2001)" *by* G.-J. Roelofs et al.

Anonymous Referee #1

Received and published: 24 March 2003

1. General comments

The paper "Distribution and origin of ozone in the eastern Mediterranean free troposphere during MINOS" by Roelofs et al. is a very well written and insightful modeling study analyzing data from an airborne field experiment in August 2001. The authors have used a state-of-the-art general circulation model with a tropospheric chemistry module to simulate ozone and ozone precursor concentration sin the Mediterranean region, and they have performed a number of sensitivity experiments with different emission sources turned off in order to quantify the contributions from individual source regions to free tropospheric ozone over Crete. The simulated ozone profiles agree well with the observations shown, lending some confidence in the model's ability to simulate

tropospheric ozone concentrations under specific weather conditions.

There is only one major problem, which I have with this kind of study: the reader is easily led to the assumption that the numbers given in the paper correspond to reality. The authors state several times, that e.g. the stratosphere-troposphere exchange is likely overestimated in their model (as in most models today). Yet, this statement does not translate into an error margin for the source quantifications given in table 2. Similar uncertainties exist in the parameterisations of boundary layer exchange and convection, which can easily lead to erroneous source attributions. In order to make these numbers robust, one would have to see a more vigorous comparison between simulated and observed precursor concentrations. At a minimum, this caveat should be emphasized stronger, and the often qualitative discussion of errors should be made more quantitative.

2. Specific comments

page 1253, l.13: here, the coarse model resolution is used to explain the underprediction of ozone - a few lines above, it was used to explain an overestimate. This is irritating to the uninitiated reader.

page 1254, l.19ff: first, it is said that the small-scale flow is not represented realisitcally by the model, but then back trajectories (which will likely suffer from the same limitations) are used in order to assign the origin of an air mass, where the model does not reproduce the observed concentrations, to a specific source region. This may need to be expressed with more caution.

page 1255, l.9ff: again, only the coarse resolution of the model is blamed for a discrepancy between model and observations. Here, it would be interesting to see, how other chemical tracers behave. E.g. would CO indicate a pollution plume?

page 1256, l.15: it i snot clear to me, why a lower tropopause alone should result in higher ozone levels in the UT. This may be true in a model world, where much of

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this UT ozone can be due to numerical diffusion, but there is to my knowledge no direct relationship between these two quantities – except that statistically, tropopause foldings will have a larger impact closer to the tropopause of course.

page 1256, l.26: Lelieveld and Dentener are not the only ones who observed an important impact of lightning on tropospheric ozone - at least they should be referred to as (e.g. ...)

page 1257, l.18: in order to asses the importance of lightning on ozone, one needs to know the assumed NOx source strength in the model and the vertical profile, which is used to distribute the computed column NOx emissions. Please specify these.

page 1259, l.14: please specify the annual cross-tropopause ozone flux for the model used.

Figures and tables:

The figures and tables are clear and of good quality. However, because the title is "Distribution ....", one would also expect at least a monthly mean chart of the ozone distribution over the Mediterranean as simulated by the model.

Interactive comment on Atmos. Chem. Phys. Discuss., 3, 1247, 2003.