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Interactive Comment

Interactive comment on "The influence of European pollution on ozone in the Near East and northern Africa" *by* B. N. Duncan et al.

B. N. Duncan et al.

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We wish to thank the reviewer for the many helpful comments. We have carefully addressed each of the reviewers concern below. The reviewers original comments are listed in italics type.

General Comments In this manuscript the authors have made an effort to assess the impacts of European pollution on ozone concentration in areas like the Near East and Northern Africa, focusing on the health aspect effects with the aid of a global chemical transport model. In general, the manuscript is well-written, uses an adequate number of references and substantiates the model results with comparison to several observations in the region of interest. Nevertheless there are several issues that need to be carefully addressed by the authors in order to eliminate obscure arguments and enhance the manuscript8217;s quality. First of all, after carefully reading the manuscript, *the specific aim seems to be the epidemiological study based on ozone health effects, with the aid of a modeling system that has been evaluated for its performance.*

Our assessment of the effects of European ozone on human mortality is not an epidemiological study as the reviewer claims. We do not use health data to infer the relationships between ozone and health, as an epidemiological study would. Rather, we use the results of epidemiological studies to assess the effects on mortality based on our atmospheric modeling. In this way, the mortality assessment is a natural extension of the modeling work, but our major contribution is the atmospheric modeling component, a core subject area of ACP according to the journals website.

Long-range transport of air pollution from Europe is not supported by model simulations, but is rather stated here using already published work. On that account, I suggest that the authors revise the title of the manuscript, in order to point out the scope of the presented study. Perhaps adding words like "health effects" or "epidemiological study" would support that purpose.

The reviewer is incorrect. Duncan and Bey (2004) detailed the export pathways of pollution from Europe, including seasonal variations. In the present manuscript, we conduct new simulations targeted at quantifying the impact of European pollution on surface values in the Near East and northern Africa, which the title conveys:

"The influence of European pollution on ozone in the Near East and northern Africa"

Our two simulations, with and without European emissions and aerosols, clearly show that the impact of European pollution extends thousands of kilometers downwind of Europe, which is long-range transport.

We believe the current title is appropriate.

For the introduction, I have to make some comments concerning the long-range transport of ozone and its precursors focusing on the Mediterranean Region. My comments are associated with a number of studies not referenced here that are concentrated

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in the Mediterranean Region and the transport of pollution to/from European cities. I suggest adding some of the following references: Kallos G. et al. 2007: "Long-Range Transport of Anthropogenically and Naturally Produced Particulate Matter in the Mediterranean and North Atlantic: Current State of Knowledge". Journal of Applied Meteorology and Climatology, Vol. 46, Issue 8, August 2007, pp. 12308211;1251. Kouvarakis, G. et al. 2000: Temporal variations of surface regional background ozone over Crete Island in southeast Mediterranean. J. Geophys. Res., 105, 4399-4407. Kallos, G., et al. 1998a: On the long-range transport of air pollutants from Europe to Africa. Geoph. Res. Lett., 25, 619-622. Millan, M. et al. 1996: Meteorology and photochemical air pollution in Southern Europe: experimental results from EC research projects. Atmos. Environ., 30, 1909-1924. Millan, M. et al. 1997: Photo oxidant Dynamics in the Mediterranean Basin in summer: Results from European Research Projects. J. Geophys. Res., 102, 8811-8823. Thompson, A.M et al. 2001: Tropical Tropospheric Ozone and Biomass Burning. Science, 291, 2128-2132. Zerefos, C. S., and Coauthors, 2002: Photochemical Activity and Solar Ultraviolet Radiation (PAUR) Modulation Factors: An overview of the project. J. Geophys. Res., 107, doi:10.1029/2000JD000134."

Thank you for these references. We now refer to several of them in the Introduction.

I also suggest that the authors change the first person used in all parts of the manuscript with more formal expressions (e.g. The expressions "We present. . . ", "We simulate. . . " should be replaced by "The modeling study presented. . . ", "The simulations have been performed. . . ")

The use of the first person is simply a matter of style and actually encouraged in technical writing, at least in North America.

Specific comments on each part of the manuscript are presented below. Specific comments 1. Abstract: Referring to number of deaths in the beginning of the article is not suitable for a scientific paper to appear not in a medical journal but in a journal focusing on chemistry and physics of the atmosphere.

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We do not agree with this comment and neither does the first reviewer of this manuscript. An important point of our work is that the international and atmospheric modeling communities should be aware of the long-range transport (LRT) of pollutants from Europe to northern Africa and the Near East. (Currently, the focus of LRT studies, such as those sponsored by the Task Force on Hemispheric Transport of Air Pollution, is on LRT between East Asia, Europe, and North America.) We justify our point by showing that the impact of European pollution in northern Africa and the Near East is substantial for atmospheric composition and also for the subsequent impact on human health. Therefore, a discussion of the impacts on human health is appropriate in the context of our manuscript. The main driving force of LRT and air quality studies, which are clearly appropriate topics for ACP, is ultimately to understand the impact of anthropogenic pollutants (e.g., POPs, Hg, ozone and its precursors) on human health and vegetation. Another current ACPD paper assesses future changes in ozone indicator statistics relevant for assessing adverse impacts on human health (and vegetation) (Ellingsen et al., 2008), and we extend this type of study by additionally including the health implications.

Furthermore, if the authors insist on using the number of additional deaths, they should accompany them with a number of uncertainty (or error) based on their calculations.

We clarified this by adding to the text of Section 5: "The uncertainty in ozone-related mortality calculations is substantial, as the uncertainty in beta alone causes an uncertainty of a factor of 1.7 in similar estimates of total mortalities, with additional uncertainties due to the ozone modeling, baseline mortality rates, and the low-concentration threshold (West et al., 2006; West et al., 2007)."

The last sentence of the abstract concerning the ozone column data has two problems that make it rather complicated and maybe useless for this section: OMI and MLS should be clearly defined because the reader might not be aware of their use and characteristics. Secondly, the conclusion for TCO given in section 3.2 is rather weak and it shouldn8217;t appear in the abstract.

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We deleted the sentence from the abstract.

2. Section 1 (Introduction): Add relevant references as pointed out in the general comments section.

Done.

3. Section 2: *Indicate what kind of biogenic emissions and natural emissions (dust, sea salt) you have used for performing simulations with the GMI CTM.*

We did not use emissions of aerosols (e.g., dust) in this simulation as we read in aerosol fields from the GOCART model for 1991 as discussed in Section 2.

For biogenic emissions, we added the following text: “Biogenic emissions of isoprene, monoterpenes, methanol, and acetone are simulated as described in Duncan et al. (2007). Isoprene and monoterpene emissions are based on a modified version of the inventory of Guenther et al. (1995).”

In the part of the shipping emissions (page 1918, lines 8-14) you should clarify if the production rates you have given for ozone and nitric acid (equations 1 and 2) are used only for the emissions from ships and not for the entire emission inventory.

We have clarified this in the text by saying that we only apply Eq. 1 and 2 to shipping emissions.

4. Section 3.1 (Model evaluation): Indicate the procedure you have used to compare in-situ observations with model values. In particular, you are using a 2o x 2.5o grid cell horizontal resolution as described in the previous section and you have specific latlon coordinates of each EMEP station you have chosen. Did you take the model value of the grid cell in which the specific lat-lon is located?

Yes. We now say “For our evaluation, we choose model ozone from the gridbox in which each EMEP station lies.”

The monthly average value comparison is logical and shows a good model perfor-

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mance, but the maximum 8h-average ozone values (Fig. 2) raise several questions. One question is on the type of the EMEP stations chosen for the comparison. Did you include urban, suburban, and rural stations?

We included all available EMEP data (67 stations in Table 1) for 2001 in our evaluation as we say in the manuscript. Concerning Figures 1 and 2, we had to select a subset of stations as we cannot show all 67 stations. As we say in the manuscript, we chose the stations shown in Figures 1 and 2 as they represent a range of environments.

If a model with such coarse resolution can reproduce the maximum 8h-average ozone values in an urban station, then the use of regional air quality models is automatically turn to obsolete! Please clarify the above comments on the manuscript.

A considerable amount of text in Section 3.1 is devoted to explaining the limitations of CTMs with relatively coarse resolutions. However, we make the point that our CTM does a reasonable, though not perfect, job of reproducing observations for the purpose of our study. That is, our CTM is adequate for our research application. We do not believe or imply in our manuscript that regional models are obsolete.

5. Section 3.2 (Tropospheric column ozone): In my opinion the use of satellite retrieved ozone values is a subject well known to the scientific community concerning the restrictions and the advantages for their use in air quality studies. This paragraph does not provide any new knowledge on that subject, and the authors should either remove it or shorten its size.

Since the discussion is only one paragraph, we are mystified why the reviewer feels that we devote too much text to this subject. There are only two paragraphs in Section 3.2 with the first discussing model evaluation.

At any rate, we think that it is important for us to mention why we cannot use the TCO product in the observation-sparse regions of northern Africa and the Near East.

Note that Figure 3 presents the comparison of model with observed TCO for different

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years (2001 and 2005) and that strikes the credibility of the result.

This is untrue. The basic chemistry and dynamics of ozone in our study region does not vary so dramatically from year to year (e.g., Ziemke et al., J. Geophys. Res., 2006). The fact that the model (2001) and the observations (2005) agree well support this statement. Of course, there are regions where there is significant interannual variability (e.g., near biomass burning events in the tropics).

6. Section 5: Equation 3 is used for the calculation of the change in human mortalities and is based on rates and coefficients for each region, the total population and the change in ozone concentration. There is a great amount of uncertainty in the above calculation, as the authors correctly point out in the manuscript. It would strengthen the concluded results if the authors could provide uncertainty values on their calculations (relative or absolute error etc). In that way the 50000 additional deaths, for example, will gain more solid mathematical and physical explanation.

We modified the text as discussed above.

Using a different algorithm together with equation 3, or even change some factors used in equation 3, would again strengthen the sensitivity of the concluded results.

This may be true, but our conclusion that European pollution impacts human health in northern Africa and the Near East will remain.

7. Figures: Fig.3: explain TCO and the units in figure caption.

Done.

Fig.8 and 9: If the black boxes indicate increased premature mortalities, why are not they in the reddish part of the label bar? How increased are those mortalities?

The caption text is correct, but we clarified this with:

"Black boxes represent regions with increased premature mortalities (i.e., fewer mortalities in the simulation with European pollution than without it)."

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Interactive Discussion

Discussion Paper

Interactive comment on Atmos. Chem. Phys. Discuss., 8, 1913, 2008.

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