Atmos. Chem. Phys. Discuss., 8, S284–S288, 2008
www.atmos-chem-phys-discuss.net/8/S284/2008/

Atmospheric Chemistry and Physics Discussions

ACPD

8, S284–S288, 2008

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.

Anonymous Referee #2

Received and published: 22 February 2008

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 manuscript'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. 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.

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 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. 1230–1251.
- 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.

ACPD
8, S284–S288, 2008
Interactive Comment

- 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.

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. . . ”)

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. 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. 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 shouldn’t appear in the abstract.
2. **Section 1 (Introduction):** Add relevant references as pointed out in the general comments section.
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.
- 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.

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 2^o x 2.5^o grid cell horizontal resolution as described in the previous section and you have specific lat-lon 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? The monthly average value comparison is logical and shows a good model performance, 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? 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.

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. Note that Figure 3 presents the comparison of model

ACPD
8, S284–S288, 2008

Interactive Comment

with observed TCO for different years (2001 and 2005) and that strikes the credibility of the result.

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.
   - 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.

7. **Figures:**

   - Fig.3: explain TCO and the units in figure caption.
   - Fig.8 and 9: If the black boxes indicate increased premature mortalities, why aren't they in the reddish part of the label bar? How increased are those mortalities?

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

ACPD
8, S284–S288, 2008