

Interactive comment on “Formation and transport of photooxidants over Europe during the July 2006 heat wave – observations and GEM-AQ model simulations” by J. Struzewska and J. W. Kaminski

Anonymous Referee #2

Received and published: 6 September 2007

This paper deals with the occurrence and impact on air quality of heat wave phenomena in Europe, which is a quite interesting and critical topic. It proposes a meteorological and chemical description of a specific 2006 episode, first through observations and then by the use of a 3D-model. The paper is globally well constructed and written, but complementary informations and details are required in the different paragraphs, and overall, there are figures and parts of the text that appear inconsistent : those points need to be cleared or discussed in more details, as developed below.

P1 right column: What does "averaged mean temperature" refers to? Global temperature? Mean ground-level European temperature? Summer temperature? It has to be

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precised.

P2: The description of the two previous heat-waves are quite interesting but do not bring anything to the rest of the paper if their specificities are not discussed with regards to the 2006 episode. They should be better involved in the understanding of the studied phenomenon or strongly shortened.

P2-3, description of the 2006 HW: There is no reference to the origin of the data that are commented. First, the sources (especially in Figure 1) should be mentioned. Second, we don't know if those comments are the "official" description of the phenomenon, and extracted from national weather services reports, or if this is a data analysis provided by the authors of the paper. The same questions raise for the ozone episode: "High ozone concentrations transported northward" "High pressure ridge enhanced the local ozone production". There are interpretations of local ozone measurements in terms of air mass transport and processing : are they provided by a model study? What does support the enhancement of local production versus the hypothesis of continental transport?

P6 : Why is there no measurement station selected in the Nordic countries? As they are at the same longitude as Central Europe, they could have brought an interesting comparative point.

P6-8: The ozone statistics and comparison of modelled ozone time-series to measurements are interesting but some aspects of this model evaluation study are not discussed. 1) The 4 graphics (Fig4 to Fig7) show that the model well captures the day-to-day evolution of the HW, in Western Europe as well as in Central Europe. But only the 4 stations that show the lowest MBE combined with the highest Correlation Coefficient are shown. How does ozone behaves in the model relatively to measurements in Purnay or Widochowa? And what does it imply for the ability of the model to reproduce means and maxima of ozone all over Europe? 2)The paper focuses on ozone maxima in Central Europe but the 2 graphics shown for Central Europe (Fig6

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and 7) do not show any peak above $160\mu\text{g}/\text{m}^3$. Is it representative of what happened? There are many points above this value in Figure 8. Stations where peaks were observed should be better illustrated in the time series. 3)The authors explain that the underestimation of ozone maxima between 11 and 13 of July in Central Europe are due to an overestimation of the cloudiness in the model, that in turn underestimates temperature and the biogenic VOC emissions. But the only comparison of modelled ground-temperature with measurements exactly shows the contrary on July 11 and 12. Is the station well chosen? The temperature vertical profile in Figure 15 also shows that temperature at ground level on July 12 is overestimated at the selected station of Central Europe, although the text mentions the contrary (P10, right column). Furthermore, the "emission" aspect is not discussed here: How well are the VOC emissions in Central Europe understood by inventories? It remains strongly possible that the highest peaks are not reproduced by the model due to an underestimation of the VOC emission by the inventory.

P9: The differences in the scaling of the graphics make it difficult to compare. The "bad restitution" of NO_2 in figure 12 does not seem to be a real problem as the absolute difference is rather low, especially considering the resolution of the model and the possibility of local perturbations at any rural station.

P11: The discussion of air quality indices is interesting but as the discussion is based on modelling results, the words "recorded", "occurred" or "levels did not exceed" should not be used as it is only a model point of view. Along this paragraph, the text maintains a confusion between what actually happened and what was modelled. The word "modelled" should be added in all the related figures captions.

P13 : There seem to be a wrong legend in Figure 16b, this cannot be temperature.

P14: The conclusions are a little bit repetitive, they can be shortened. The sentence about "biogenic VOC flux enhanced (that) did not have a significant impact on ozone production" is not supported by any evaluation of emissions in the text of the paper,

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nore by ozone production calculation. This should not be affirmed in the conclusions without initial analysis and discussion in the paper.

Interactive comment on Atmos. Chem. Phys. Discuss., 7, 10467, 2007.

ACPD

7, S4694–S4697, 2007

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