**Atmospheric
Chemistry
and Physics
Discussions**

Interactive comment on "Global model simulations of air pollution during the 2003 European heat wave" *by* C. Ordóñez et al.

Anonymous Referee #2

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General comments

This article presents a diagnostic study of the skill of global chemistry-transport models in the simulation of pollutant concentrations during the Summer of 2003 and specifically the heat wave period. The article also presents the results of sensitivity studies concerning emissions, deposition, resolution, coupling. Results presented are among the outcome of the GEMS FP7 project. The work represents a big effort to understanding the specific difficulties of global models to simulate extreme regional episodes. It pinpoints the weaknesses and points to improve. What we learn from the article is mostly the deficiencies of global models, although it is difficult from this study to have general conclusions because the three models used here have different problems. It is not clear that we learn much more, especially about the processes, but the paper is

still a valuable contribution. Thus I recommend the study to be published but I have a number of points below that must be addressed beforehand.

Specific comments

P 16857 L5-6. The following article, relating a statistical study of the combined effects of heat and air quality could be cited (L. Filleul, S. Cassadou, S. Médina, P. Fabres, A. Lefranc, D. Eilstein, A. Le Tertre, L. Pascal, B. Chardon, M. Blanchard, C. Declercq, J.-F. Jusot, H. Prouvost, M. Ledrans, The relation between temperature, ozone and mortality in nine french cities during the heat wave of 2003, Environ. Health Perspect. 114 (9) (2006) 1344–1347)

P 16860 L22: It is not clear how CTMs are forced from wind fields. Are wind fields linearly interpolated from 6-hour analysis? How does this rough interpolation influence mass conservation between interpolation times? How are vertical wind fields processed (diagnosed or forced)? What are the differences between the "mass flux method" for TM5 and the methods for other models? References to previous work should be added, at least, but it would be better to have details here.

About time interpolation: The time interpolation may be critical in particular because it may introduce noise in mass conservation with effects hard to predict. Time interpolation in morning hours between 6UTC and 12UTC may also introduce very large errors in downmixing of residual layer. If mixing is too strong too early this inhibits precursor concentration build-up with fresh morning emissions to favor chemical reactions. 6-hour interpolation can be very bad for other meteo variables like for instance radiation (if used). It is probably too late to do that but it would have been interesting to consider 6-hour analyses followed by 3-hour short-term forecasts, and interpolations in between. I strongly encourage the authors to provide an analysis of the effect of interpolation by comparing coupled and uncoupled model results if possible (that can be a case study).

P 16861 L4-5: Injecting fire emissions at ground level is a very bad choice (see e.g.

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Hodzic et al 2006 cited). It could induce wrong PM concentrations in situations like the heat wave. Again it is probably too late to change that but the choice of, at least, spreading fire emissions throughout the PBL would be better.

P 16861 – Bottom: It is not clear what the run "with assimilation" will help to understand, in addition to the IFS-coupled run. What do we expect to deduce? This run should not tell much about the model deficiencies which is the strongest focus of the paper. Why not simply omitting this simulation in the rest of the article?

P 16865, on observations used to evaluate models: Only the EMEP sites are used. However these are quite sparse over large parts of France where the ozone concentration was largest. It would have been nice to include more surface observations, in particular from French monitoring networks in rural areas.

P 16869 L5-6: the responsibility of higher reactivity is a statement (it is detailed later also), but only qualitative arguments are given. Please add a reference of a specific study which demonstrates this point. It is not clear why higher reactivity would lead to large ozone concentrations all over the Mediterranean sea.

P 16872 L6-10: There is already a discussion later about resolution, so this point is discussed twice. Probably the discussion should be removed at this place.

P 16873. Nonlinearities and lack of resolution are invoked for explaining the overestimations of MOCAGE. The argument reads like "because the RACM mechanism has more detailed NMVOCs it has overestimations at coarse resolution". This is hard to believe, since several models (not in this study) use mechanisms similar to RACM and do not show overestimations. At least to confirm such a behavior an experiment should be done by canceling emissions in reactive VOCs and replacing them with emissions in less reactive ones, in order to mimic the chemistry setting of the other models and compare. This can be done for a case study and does not necessitates a long-term run.

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P 16873 last line: "biogenic CO emissions" should be "fire CO emissions"

P 16874 L18: the fact that resolution increase does reduce the negative ozone bias is somewhat contradictory with the argument for the overestimation for MOCAGE (coarse resolution induce positive bias).

P 16878 L5-10. It is not clear what is exactly done with the swith off of off-European emissions. When are emissions switched off exactly? The results should be dependent of the switch-off time.

L 17, same page. Can we really say that "influence of non-european emissions is small", as background concentrations are important and mainly due to these emissions?

P 16879. The resistance formula (4) is wrong, it should be Vd=1/(Ra+Rb+Rc). This could influence the discussion in the following page.

P 16882 L20: The statements concerning MOCAGE should be toned down, given the above remarks

P 16885 L16-17: Whether "the development of global CTMs and the coupled system is not aimed at achieving such good performances" depends on the ambition we have for those systems. To improve emission diurnal variations does not make the computation times longer and is not difficult. Improving the fire emissions and injection heights should improve the performance with no additional computational cost. These are only examples. I seriously disagree with the statement, and believe such models should, in a few years time, give simulations at least as accurate as present days' regional models, while regional models will describe refined meso-scale effects.

Discussion Paper

Interactive comment on Atmos. Chem. Phys. Discuss., 9, 16853, 2009.