Atmos. Chem. Phys. Discuss., 9, C3252–C3256, 2009
www.atmos-chem-phys-discuss.net/9/C3252/2009/

Atmospheric Chemistry and Physics Discussions

Interactive comment on "Decadal regional air quality simulations over Europe in present climate: near surface ozone sensitivity to external meteorological forcing" *by* E. Katragkou et al.

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Received and published: 24 July 2009

General comments:

The choice of proper model(s), as well as its proper configuration is of the utmost importance in the context of the subject of this study. While the models selected are contemporary and respectable atmospheric models, their particular suitability in the presented configuration is neither discussed nor demonstrated by the authors. Unfortunately, this issue is critically important, and lack of proper selection and justification may invalidate the work.

The authors should have presented scientific justification for using these models. Are

these models compatible? Why was the WRF/Chem model not used in place of RegCM3 and CAMx?

While the applicability of CAMx to run with different meteorological inputs (including meteorology in future climate) for regional air quality studies is not disputed, the particular application in the reported study seems to be seriously flawed.

According to the last paragraph on page 10679 (line 25), the model top was set at 6.5 km, where "boundary conditions were kept constant, corresponding to a clean atmosphere". Whereas such an assumption may seem suitable for studies involving primary pollutant dispersion on local and short time scales, it is hardly justifiable for ozone in the context of a continental-scale climatic study. There is an abundant body of research in which the issues of stratospheric ozone intrusions, upper troposphere - lower troposphere exchange and the role of large-scale transport of ozone precursors in the upper troposphere are addressed. These processes cannot be considered as negligible and must not be excluded from a modelling system. Given the fact that the "uppermost layer is 1.2 km deep", one might well suspect that the model actually describes not much more than just the boundary layer which, in the context of a climatic study, seems to be a gross simplification and misunderstanding.

Furthermore, photolysis rates used in CAMx are pre-calculated by the TUV radiative transfer model (Madronich, 1989) and stored in a multi-dimensional lookup table by surface albedo, total ozone column, haze turbidity, altitude, and zenith angle. (CAMx v5.00 User's Guide, March 2009. Same method was used in the presented version 4.40). It should be clear that ozone column abundance is above the model top and thus has to be prescribed for each chemical time step, or at least on a monthly basis, and for each year of simulations. The authors have not addressed how the column ozone was determined by the model for the past conditions and how it could be calculated/prescribed for future climate simulations.

In my opinion, these flaws may considerably affect the results and put the final conclu-

sions in doubt.

Notwithstanding the apparent lack of references, a very simplistic description of climate simulations and modelling tools that served as a basis for the presented study, the authors have attempted to establish the credibility (page 10678, starting on line 11) of the constructed modelling system by stating that "The wider scope of this work is to investigate the credibility of our modeling system which is driven by the ECHAM5 general circulation model (GCM) in order to further use it for future scenarios including climate change." As in any complex interaction, the overall value of a system is determined by its weakest component. Thus, it seems the presented system does not offer much credibility.

It is my belief that in view of a worldwide vigorous debate on climatic changes, their possible mitigation and their impact, papers submitted for publication in leading, highly cited scientific journals should be carefully scrutinized in order to eliminate presentation of highly uncertain and questionable results, and to reduce the risk of error propagation in the scientific literature. In my opinion, the identified design flaws of the research reported in the paper by Katragkou et al., makes it bear attributes of highly uncertain with questionable results. In order to obtain acceptable results, at the very least air quality computations should be re-worked with acceptable vertical grid extent and resolution, taking into account the variability of ozone (and other absorbers including clouds and aerosols) amount above the model top.

The following are additional comments that, if fully addressed, would shed light on the quality of the climate simulation and compatibility of models used in this study. Comments and questions are organized by model and process.

ECHAM5:

Simulations have no corresponding reference. The authors should have informed the reader about the horizontal and vertical grids and parameterizations selected in performing climate simulations with the selected model. Was there an ocean model con-

nected to the ECHAM5 model?

ERA40:

There is no proper reference to the ERA40 dataset. The only passing comment on page 10680 line 23 "...ERA40 reanalysis project is a global atmospheric analysis of observations and satellite data streams..." is simplistic and inaccurate. For clarity and completeness the authors should have stated resolutions (horizontal and vertical) and a list of available fields should have been provided. In addition, it is not clear to the reader what are the differences between ERA40 and ECHAM5 datasets used for input to RegCM3.

RegCM3:

Simulations have no corresponding reference. The authors should have informed the reader about the horizontal and vertical grids and parameterizations selected in performing climate simulations with the selected model and its scientific compatibility and relationship to ECHAM5. Was there an ocean model connected to the regional climate model?

Nesting of models:

What were the nesting time intervals for ECHAM5 to RegCM3, ERA40 to RegCM3 and RegCM3 to CAMx? In addition, a list of levels and fields used for nesting, together with the nesting methods including the extent of the nesting band should have been provided.

Authors should explain and further clarify statements on page 10679 lines 11 to 20. It is hard to imagine that the CAMx model would require fields not available from a regional climate model - from an advanced atmospheric model.

The sentence on line 17 on page 10679 is surely an error: "Pressure and geopotential height is obtained using the hydrostatic formula...". Why and how would one be able to recalculate pressure and geopotential from the hydrostatic equation? After all, these

fields are produced by RegCM3. The authors should have stated which fields from RegCM3 were used as input to CAMx.

Technical/Editorial comment:

It appears that the Technical Editor missed a reference to a publication in preparation (Tegoulias et al. 2009) that should have been included as a footnote and not in the list of references. It is rather unfortunate that a large part of this paper hinges on the non-existent publication which was referenced 3 times.

Interactive comment on Atmos. Chem. Phys. Discuss., 9, 10675, 2009.