

## ***Interactive comment on “Validation of a 3-D hemispheric nested air pollution model” by L. M. Frohn et al.***

**Anonymous Referee #2**

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### General Comments:

The paper describes the comparison of measured concentrations of air pollutants (EMEP network) with the results obtained from a hemispheric nested air pollution model (model domain: northern hemisphere; nest 1: Europe).

The extension from Europe to the global scale (Northern hemisphere) allows a more consistent treatment of the lateral boundary values for Europe (or any other limited area on the regional scale). The validation of such models is an important scientific issue. REGINA also has the potential to study intercontinental transport. Therefore the REGINA model as presented here might be a valuable and interesting contribution to the hierarchy of models. It can also be used in the context of the recently established EU directives on air quality which prescribe limit values at least partially on an annual

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time scale.

However, the presentation of the physical concepts and parameterizations used in the model and the comparison between measured and calculated concentrations could be considerably improved. Therefore I recommend major changes in the manuscript before publication. In principle, as mentioned above, the contribution might be interesting after this substantial improvement. In its present form the paper gives a good description what has been done (or which results have been obtained) but explanations why its has been done (or a detailed scientific discussion of the results) are missing sometimes.

Nevertheless I am convinced that this paper after major improvements will be an important and valuable contribution to the science of air pollution modeling. I would like to encourage the authors not to restrict themselves to much to technical descriptions of the model (which are detailed and important) but also to add more scientific discussions on the results. Their thoughts are also interesting for the scientific community even if they might be more on unsafe or hypothetical.

Specific Comments:

Comments to the Abstract:

The abstract, as the paper in general, contains a good technical description what has been done. A good and detailed technical description is important and valuable. However, I would like to encourage the authors, who evidently have a good knowledge of atmospheric processes, to focus also on scientific questions and to describe the scientific goals of the contribution clearer. It might also help to improve the paper if more arguments are given why a specific improvement or development has been done. The abstract can provide some arguments why it is interesting and important to extend a limited area air pollution model for Europe to the Northern hemisphere (e.g.: impact of intercontinental transport on ozone or other air pollutants to Europe and vice versa (two way nesting); import and export of air pollutants; contribution of global pollution to

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European pollution levels and vice versa).

Some typical examples in the abstract:

Why are only EMEP stations selected for the comparison ? There was no information given before given that the focus of the study is on Europe (and why ?). Are there similar studies for other continents (or Europe)? (as e.g. Langmann et al., 2003). Do measurements show the importance of intercontinental transport to Europe? (e.g. Stohl and Trickl, 1999; Stohl et al. 2003).

There is no general conclusion given in the abstract: What are the main results of the sensitivity studies of the model performance with respect to resolution in emission and meteorology? (It is mentioned only that sensitivity studies have been undertaken). Which scientific questions have been solved? What has been improved (or not improved)? What can be improved in the future? (based on the conclusions).

What can be concluded for different species? It might also be an important (although not satisfying) result that mean SO<sub>2</sub> concentrations are considerably overestimated in August.

In general, I suggest to shorten the technical part in the abstract and to focus on the scientific goals of the study, the scientific questions to be answered and the reasons for doing this study. It is not so important if the grid size is 150 km, or 250 km, or ...; if the nesting ratio is three or five or whatever. These informations are important but I suggest to give them in the general text (e.g. model description). They must not be part of the abstract.

Comments to chapter 1: Introduction

There is no doubt that the REGINA model is embedded in a sophisticated set of models at NERI (and this is valuable and interesting). However, there are also other regional models embedded into a global to mesoscale model chain (e.g. Langmann et al., 2003) which are not mentioned in the manuscript. Also the results obtained with the EMEP

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models are not mentioned. I suggest to refer also to other model chains from global to regional/local scale and to some results of the EMEP modeling work in the discussion of the results.

The authors should try not to mix up the terms horizontal grid size and scales. The spatial scale of the hemispheric model domain is global (about 10000 km), the horizontal grid size is 150 km, which is an appropriate state of the art horizontal resolution for the global scale.

It is mentioned in the introduction that the overall model performance depends in general on the numerical schemes employed, the nesting techniques, initial and boundary conditions, quality of input data as well as chemical parameterizations.

But what are the lateral boundary conditions of REGINA, what are the initial conditions? (e.g. for ozone). What are the upper boundary conditions/values e.g. for ozone? How is ozone treated in the free atmosphere?

The example of NO<sub>2</sub> transport is very good for motivation but the sentence "Episodes like this will not be included in the results if the domain is limited" is not correct: The episodes are included also in regional models but the impact of intercontinental transport can not be included in a consistent way. It would be nice if the example shown in Fig.1 (or another one) will be used to compare limited area results for Europe (nest1 domain without the hemispheric domain) with the results obtained with REGINA. At least for one episode the impact of intercontinental transport could be illustrated in this way.

Comments to chapter 2: Model description

The meaning of sigma is not explained (minor point).

How are the upper boundary values (e.g. for ozone) specified?

How are the initial values specified?

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How is dry deposition calculated? Which landuse data set has been used for the calculation of the dry deposition? (Wilson and Henderson-Sellers (1985)?). What is the reason to introduce an additional landuse data set? There are landuse data available and used within MM5.

What is the temporal resolution of the output of the model? (daily average or hourly values?)

Comments to chapter 3: Emission and meteorology scenarios

Why are the seasonal and daily variations of the emissions not included? Is this due to a lack of information? or is the focus of the paper just on monthly or daily averages and the daily variation of the emissions is considered not to be important in that case?

How are the biogenic VOC-emissions calculated? This might be an important point because isoprene and terpene emissions depend strongly on meteorological parameters and landuse. There seems to be some inconsistency in the landuse data set used in the MM5 (meteorological calculations), the deposition fluxes (Wilson and Henderson-Sellers (1985) and the biogenic emission fluxes. It would be helpful to clarify that point because landuse characteristics control the exchange of mass (water vapour), momentum and energy in the near surface layer. It is difficult to understand the reason for the use of different land use data in different parts of the modeling system REGINA without additional information.

The set of emission scenarios is difficult to understand without further explanation. What are the questions which will be addressed by the different sets of emission and meteorological scenarios?

What are the "other VOC" in table 2?

Comments to chapter 4: Results

If model results for ozone in February are mainly background values (are they?): How are the background values treated in the model?

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Why are the stations Oulanka and Langenbruegge selected?

Is it really justified to exclude all stations above 500 m only due to the coarse resolution of the model? These stations might represent the background concentrations and can be compared to the results for more elevated layers of the model. This might also help to interpret the underestimation of the higher ozone values in February.

Overestimation of SO<sub>2</sub> is partially due to the same stations (in February as well as in August: HU2, PL2, AT2; might this be due to a general overestimation of sulfur emissions in parts of Eastern Europe?)

Is it possible to compare the measured and observed daily maxima (e.g. for ozone)? This might be interesting in particular for the summer season (August 1998).

Typing errors:

Introduction, second paragraph

"In most cases there is no need fo describing ...", r is missing.

References:

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Interactive comment on *Atmos. Chem. Phys. Discuss.*, 3, 3543, 2003.

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