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Atmospheric Chemistry and Physics Discussions

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

Anonymous Referee #1

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Referee comments regarding the manuscript:

"Validation of a 3-D hemispheric nested air pollution model"

by L.M. Frohn, J.H. Christensen, J. Brandt, C. Geels, and K.M. Hansen

General comments:

This study is (probably) interesting enough to merit publication *in completely rewritten form*. I suggest that the paper is rejected and I encourage the authors to rewrite it "from scratch" before resubmitting it.

As written, the manuscript does not contribute much to *the scientific progress within the scope of Atmospheric Chemistry and Physics*. There are no substantial new concepts, ideas, methods, or data included.

Potentially, the study could give information about the usefulness (or perhaps uselessness) of nesting hemispheric coarse scale models with finer scale continental models. In my opinion this is what the authors should focus on when rewriting the article.

Specific comments:

The model description is insufficient.
E.g., There is almost no description of the modifications done to the Strand and Hov (1994) chemistry, which, e.g, does not include any sulphur chemistry. The ammonium chemistry is not described sufficiently (in Hertel et al. (1995) I can only find a description of the NH_3 - HNO_3 chemistry and no details about ammonium-sulphur chemistry). There is also no description of the biogenic VOC chemistry in the model. The dry deposition scheme is not discussed at all. What wet deposition parameters are used? What boundary concentrations are used for the large-scale domain?

I find the description of the eight model scenarios slightly confusing. The model has been run "with no nests for the entire year 1998"; I interpret this as running the model over the Northern Hemisphere domain with the coarse (150km) resolution. This, however, seems strange when considering the differences between model scenarios 1 and 2 (as well as 4 and 5); Using a nest over Europe with the same resolution (both in meteorology and emissions) as the "mother domain" seems very strange to me; My conclusion is that I must have misinterpreted the "with no nests for the entire year 1998" part and what has been done is a whole year simulation of the European domain and that the "Northern Hemisphere" simulations were only done for the two chosen months (February and August)? The authors should clarify this point. If my revised conclusion is correct the authors also must specify what spin-up times have been used and how initial conditions and boundary concentrations were handled.

The results of the study are not described/discussed in a very good way in the text. Some of the results in the tables look interesting but there is very little discussion about them in the text (especially *explanations* are lacking).

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One example (out of many): Why is there such a big difference in the bias between model scenario 4 and 5 (in February)? (And why does the bias increase for SO_2 and decrease for SO_4^{2-} when going from model 4 to 5? I guess it has to do with different amount of oxidants in the two different models.)

Personally, I do not think the ranking method used by the authors is very interesting in itself. The interesting thing is to point out and try to explain significant differences in results between different scenarios.

If possible, there should also be some comparisons with other published studies, using similar models (I guess there must be a number of studies that this one could be compared to, e.g, EMEP-studies?).

The potentially most interesting results from this study would be some information about how large influences the non-European parts of the Northern Hemisphere have in different parts of Europe. I believe that studying only two months will only give very limited information about this (partly because of the long transport times) but it may still be interesting. Can the authors show any case were the inclusion of the non-European part of the Northern Hemisphere gives an improvement in modelled concentrations/depositions in Europe, compared to using only the "European" domain?

On page 3551 the authors suggest that Correlations for O_3 are bad due to the fact that the model results corresponds to background concentrations, "which are better represented at the stations located at higher altitudes". Firstly, the EMEP stations are supposed to be representative for rural conditions (although not all of them are ideal, of course). This means that most of them are likely to be fairly representative for a 50km resolution model. Secondly, if the authors believe that their results are better represented by the high-altitude EMEP-stations why not compare to these stations too?

On page 3553 the authors state that some of the stations have high measured concentrations of nitrogen dioxide and they believe that the reason is that they are "prob-

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ably corresponding to stations located in or close to urban areas". Have the authors checked the quality of the measurement stations in the EMEP/CCC reports and the site descriptions on the EMEP web pages? If some stations are of "poor quality", either because of local pollution or for some other (technical) reason I belive that they should be excluded from the validation.

Could the authors motivate the choice of the two stations Langenbrügge and Oulanka for showing time series of different compounds? Were this stations chosen randomly or because they were considered representative samples of the ca 100 stations available? Maybe it would be more interesting to show timeseries for some Irish, British, and/or Norwegian stations where the influences of North American emissions may be expected to be larger than in Germany and Finland? For the "Atlantic" stations time series could be compared for runs with and without taking North America into account.

Technical corrections:

The $^-$ is missing in NO_3^- in many of the tables.

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