**Atmospheric
Chemistry
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Discussions**

Interactive comment on "Comparison of OMI NO$_2$ tropospheric columns with an ensemble of global and European regional air quality models" *by* V. Huijnen et al.

Anonymous Referee #2

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This paper presents a comparison of NO2 columns simulated by 9 regional air quality (RAQ) and 2 global models used in the EU project GEMS with one year (Jul 2008 – Jun 2009) of OMI observations over Europe, as well as a comparison with surface observations for the same period.

The GEMS project is a major step forwards towards a "chemical weather" prediction system with routine forecasts of air quality much alike operational weather forecasts. Therefore an evaluation of the GEMS models is very valuable and will help further improve the models. Since the regional models were principally forced in the same way (lateral boundary conditions, emissions) and were covering the same domain and

time period, the comparison between the models is (or rather would be) meaningful, but I agree with all statements made by the other referee on this point.

The paper is generally well written and I therefore recommend publication with minor changes. Nevertheless, I am not really happy with the outcome of the study but at the same time I don't see an easy and quick way for improving this situation.

General comments:

Despite the similar setup the models often show large differences. A clear weakness of the study is that the reasons for these discrepancies remain mostly unclear and are not further investigated. I acknowledge though that the specific setup of the models in an operational environment did not allow for an in-depth analysis of such differences which would require further sensitivity studies. Nevertheless, this is a significant drawback that needs to be considered in future model evaluation studies.

The bulk of the regional models actually performs quite similar (see e.g. Figure 7 and 9) with MATCH and SILAM often showing up as negative and positive outliers, in particular during summer. The CHIMERE model simulates very low concentrations over Eastern Europe which suggests a problem in the implementation of the TNO emissions inventory, but I don't think this is mentioned anywhere in the paper. I think these facts should be stated more explicitly, rather than trying to defend individual model problems.

The study further suffers from the fact that both validation data sets, i.e. OMI columns and surface NO2 observations, both suffer from significant uncertainties. The observations therefore only serve as guidelines rather than as validation data sets and no firm conclusions can be drawn regarding good or bad model performance. The authors are not to blame on this but it clearly demonstrates that further efforts are need to enhance the accuracy of the observations. As a consequence, the reader is left with many comparison figures and numbers but doesn't know which one to take as the reference against which the models are to be tested. Unfortunately, only little more can be learned from this analysis than that there are significant differences between

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models and between models and observations. We do not even learn whether or not the TNO inventory is more realistic for Western Europe than the RETRO inventory, despite the factor of 2.5 difference in emissions. Probably the most interesting result of this study is that whether applying the averaging kernel to the regional models or not did not make a significant difference. However, it would probably be dangerous to generalize this result.

The OMI data might have a bias (0-40% in summer) but the relative differences between different regions are probably more reliable. Thus, in the comparison of the models with OMI the authors should put more emphasis on relative differences between OMI and models (such as the low values in CHIMERE over eastern Europe).

Minor points:

- Abstract: The sentence mentioning the small bias when neglecting the averaging kernel is not understandable to the reader in this context. It should first be mentioned more generally what the differences between models and observations are (which is only mentioned later in the abstract) and only then the effect of applying the averaging kernel (or not) may be stated.

- Page 22273, line 13: Change to "We attribute these low free tropospheric NO2 concentrations .."

- Introduction: It is not necessary to list all regional models including references here. They will be introduced one by one in Sect. 2.1.

- Page 22276, line 4: "remain unclarified" -> "remained unclarified"

- Page 22278, end of Sect. 2.1: The description of the different models is quite unbalanced, in particular regarding the different chemistry schemes.

- Page 22279, line 13: The publication of Holtslag and Boville (1993) is on turbulent not on convective transport.

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- Line 18: In what form is NOx emitted in the RAQ models?

- Line 25: "This inventory provides " -> "The TNO inventory provides .."

- Page 22280, line 10-15: The TNO and RETRO inventories differ strongly over western Europe but are more alike over eastern Europe. So why does this suggest that the RETRO inventory is outdated over eastern Europe (or what do you mean by "these regions")?

- Line 18: It would be useful to know how much ship emissions add to the total NOx emissions in Europe and thus to know how much the inventories in models with and without ship emissions differ.

- Line 21: What about a seasonal cycle in emissions?

- Page 22281, line 8. Remove the sentence "The OMI datasets are publicly available .." here. I think you should first introduce the DOMINO data set and only then mention that this data set is available publicly through TEMIS.

- Line 10: "The retrieval algorithm for the DOMINO product () has been". Since there are different OMI NO2 products available you should first state that for this study you used the DOMINO product and only then start describing DOMINO.

- Page 22282, line 19: "Here the uncertainty .." -> "The uncertainty due to cloud fraction (and aerosols) was estimated to be .."

- Line 23: What do you mean by "under-representation"?

- Line 26: Change to "and is estimated TO BE of the order of 10%"

- Line 27: "Recently it WAS shown"

- Same line: eliminate comma after (Kleipool et al., 2008)

- Page 22283, line 3: "This WAS attributed .."

- Line 6: "TM4 a priori profiles WERE replaced"

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- Line 8: "Zhou et al. (2009) reportED"

- Page 22285, line 11: "determined by NOx chemistry (reaction of NO2 with OH, conversion to PAN, nighttime chemistry) and dry and wet deposition." Photolysis does not determine NOx lifetime (only indirectly).

- Page 22286, line 19: Why were emissions "underrepresented by about a factor 2"?

- Line 26: What is the "GEMS-RETRO" inventory? It was not introduced before.

- Page 22287, line 7: "from 1.9x10e15 molec/cm2 in JJA to 2.8x10e15 molec/cm2 in DJF.

- Line 20: I think you should generally use Eastern Europe rather than East Europe or East-Europe. Same for Western Europe.

- Page 22288, line 2: I am a bit confused: A few lines earlier it was mentioned that OMI data in June 2009 are much lower than in July 2008.

- Line 25: "range between 0.6" -> "range from 0.6"

- Page 22289, lines 6-10: I don't think this can be concluded given all uncertainties.

- Page 22290, line 11: In meteorology September is not a summer month. So why don't you take August or July rather than September? Same line: "september" -> "September".

- Line 16: Increased photolysis is not relevant here. Increased OH is the point.

- Line 24: "lesser extend" -> "lesser extent"

- Page 22291, first line. The ratio 0.15 NO2 to 0.85 NO is not a deficiency of this model but it is close to currently accepted emission ratios (today the ratios may be even closer to 0.2:0.8).

- Page 22292, line 15: "In the following, the integrated partial columns . . ."

- Line 26: What do you mean by "other months"? Most months?

- Page 22293, line 3: From a RMS difference you can not infer whether there is a cancellation or not, since the RMS sums up the squared differences (which are always positive).

- Page 22294, line 12: "ARE more alike to TM4".

- End of Section 8: Probably a conclusion should be that differences between models are much larger than differences in columns with or without the AK applied.

- Page 22295, line 26: HNO3 photolysis is very slow (timescale of the order of a week) in comparison to the reaction of NO2 with OH (which limits the lifetime of NOx to only a few hours in summer) and is thus unlikely to make an important contribution to the NO2 column (is HNO3 photolysis not considered in the other models?). The high SILAM columns in summer seem unrealistic and can not be defended in this way.

- Page 22301, line 9: Which photolysis are you referring to? The higher levels of OH in summer are the main point.

- Figure 1: The two panels should be enlarged

- Figures 7-13: Axis labels tend to be too small.

- Figure 9: Why don't you show a similar figure for surface NO2 in comparison to the ground-based observations?

- Table 4: What do you mean by "spread"? 1 sigma, 2 sigma, full range?

Interactive comment on Atmos. Chem. Phys. Discuss., 9, 22271, 2009.