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Interactive Comment

Interactive comment on "Comparisons between ground-based FTIR and MIPAS N_2O and HNO_3 profiles before and after assimilation in BASCOE" *by* C. Vigouroux et al.

Anonymous Referee #1

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Review of the manuscript entitled " Comparisons between ground-based FTIR and MIPAS N2O and HNO3 profiles before and after assimilation in BASCOE" by Vigouroux et al.

The manuscript by Vigouroux et al. presents a statistical comparison between ground-based FTIR data located around the world, namely five stations from high northern downward to high southern latitudes, and the measurements V4.61 from the MIPAS instrument aboard the ENVISAT ESA platform. It focuses on two species: nitrous oxide (N2O) and nitric acid (HNO3). Vertical profiles and partial columns of N2O and HNO3 are also compared with the output from the assimilation model BASCOE. Different

EGU

criteria are defined in the selection of coincident profiles both in time and spaces. Only the year of 2003 has been considered in the present analysis.

It appears that, basically, biases, standard deviations and error on the mean of the difference between MIPAS and FTIR data, are better when considering N2O (better than 5%) than when considering HNO3 (better than 12%). The use of an assimilated data set (BASCOE model constrained by MIPAS data) gives encouraging results since bias is negligible for N2O while it is still present for HNO3 (˜5%). A systematic spectroscopic error in the MIPAS HNO3 data is pointed out, as widely accepted by the community. Measurements at high latitudes usually give less convincing results because of the presence of the vortex in winter-spring seasons, and particularly for the station of Arrival Heights, thus showing the limitations either of the methodology itself and/or of the assimilation model. As a conclusion, BASCOE N2O profiles are presented as good proxies of MIPAS N2O profiles.

The manuscript is very well written, the numerous Figures and Plates are well presented, the methodology is clearly explained, the abstract and the title are meaningful, references are correct. The scientific significance of the paper is certainly not that high, but the use of an assimilation scheme in order to improve the statistical comparisons between two different data sets is certainly worthwhile publishing. I am more sceptical about the use or, at least, about the understanding of the model and/or of the assimilation scheme employed, and of the minimalist interpretation of the biases between data sets. For these reasons, detailed below, I would recommend publication of the paper only after revisions have been performed.

Major points.

1. BASCOE

The model BASCOE, including its assimilation 4DVAR scheme, is very rapidly presented. Several issues will need to be clarified. When considering the observations of MIPAS (H2O, NO2, O3, CH4, N2O and HNO3) are they (yes or no) correlated in the

ACPD

6, S3807–S3811, 2006

Interactive Comment

assimilation scheme? If yes, how does HNO3 and N2O behave knowing that MIPAS HNO3 is biased because of spectroscopic parameter systematic error? If no, what are the advantages/caveats of assimilating all these species in your particular study? What is the spin-up period of your run? Is it long enough? What are the horizontal and vertical correlation functions, if any? Is the model error fixed or does it depend upon the considered molecule, or chemical family? You also mention in the conclusion a “convergence criteria”. Could you define it and explain why this parameter can play in the assimilation procedure? When there is a bias between FTIR and BASCOE (e.g. for HNO3) is there a way to differentiate sources of the bias within BASCOE, namely is it due to the model, to the assimilation technique used, or to the satellite data?

2. FTIR.

Contrarily to N2O, the HNO3 averaging kernels do not peak at the expected height with the exception of the 33-km As. Note also strong negative values for the 21- and 25-km As. This is not really satisfactory, since the retrieval layer is much wider and does not represent the altitude you are supposing sounding. In that context, are FTIR vertical profiles very meaningful when comparing with MIPAS and could that cause the systematic bias between FTIRs and MIPAS (measured and assimilated)? Lastly, remember that FTIR DOFs never exceed 3, thus do not overinterpret the vertical biases between MIPAS and FTIR although presented profiles have high vertical resolution.

3. Transport vs. Chemistry

The fact that N2O is well reproduced by a model constrained by satellite measurements is encouraging. This certainly means that the dynamics is correct because of the lifetime of tropospheric origin N2O. For HNO3, it is rather different since chemistry and dynamics participate to its temporal and spatial distributions. So we could anticipate much better results in N2O than in HNO3. What could you do to improve HNO3 assimilation? For instance, using a full PSC microphysics instead of the PSC parameterization might probably improve the model output.

ACPD

6, S3807–S3811, 2006

Interactive Comment

4. Error.

Error on the mean should statistically decrease in sigma/sqrt(N), N being the number of points and sigma the random noise, while there is absolutely no reasons for the standard deviation to decrease when considering more points in the coincidence exercise. Nevertheless, P. 8347 L. 25, you write that "error on that mean should be larger than 3*sigma/sqrt (N), with sigma the standard deviation." Could you clarify this point?

5. Winter-Spring disagreements

In winter-spring season, the comparisons between FTIR and MIPAS are poor. This is probably due to the presence of the vortex. In addition to the difficulties of tracking the vortex with the model considering its own resolution, how is the vertical descent actually represented in the model, and could that be another possible reason for explaining the systematic bias since it is known that models have great difficulties to reproduce the actual degree of vertical descent? And for HNO3, what about denoxification/denitrification in the model? This also might produce biases? Why not also using a criterium based upon Potential Vorticity values for comparing within/outside/vicinity of the vortex? This should also reduce the biases.

6. Spectroscopy issues.

Why not showing the MIPAS-FTIR HNO3 results directly considering the same spectroscopic parameters? Is this linear regarding the retrievals?

7. Smearing effect.

Could you quantify the "smearing effect"? And what could be done to reduce this effect?

8. Arrival Heights.

Comparisons considering FTIR in Arrival Heights are systematically poor compared to all other stations. Could you comment? Could that be a problem of measurements, of

ACPD

6, S3807–S3811, 2006

Interactive Comment

modelling, of assimilation, of coincidence criteria?

Minor points.

1. P. 8338, L. 21 “FTIR technique is the only available ground-based source of data for N_2O and HNO_3”. FTIR is not the only instrument tracking stratospheric HNO_3 and N_2O. There were already some ground-based microwave measurements of N_2O and HNO_3 performed by the de Zafra’s team and instruments in Arctic and Antarctic.

2. P. 8339, L. 24: “lower limit varies”: the latitudinal dependence of the lower limit in MIPAS retrievals is certainly related to the height of the tropopause. Could you comment on that?

3. P. 8346, L. 22. “Degradation”? I would use the term of “lowering” instead “degradation”.

4. Typo in Eq. (4). “Bf” for bold I guess.

5. P. 8350, L. 11: Could you add references for baseline errors affecting N_2O and HNO_3 retrieval?.

6. P. 8340, L. 2: Isn’t it “lower” instead of “greater”?

7. P. 8338, L. 20: I would write “tropospheric source species and a stratospheric source species”

Interactive comment on Atmos. Chem. Phys. Discuss., 6, 8335, 2006.