

***Interactive comment on* “Comparisons between ground-based FTIR and MIPAS N₂O and HNO₃ profiles before and after assimilation in BASCOE” by C. Vigouroux et al.**

Anonymous Referee #2

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The paper by Vigouroux et al. reports on the retrievals of HNO₃ and N₂O profiles from ground-based solar absorption FTIR spectra and on the comparison of the latter with the MIPAS satellite data (V4.61). An assimilation model (BASCOE) is used to get around collocation issues.

The analysis is workmanlike and the paper presents a series of interesting results, which are of general significance for the readers of Atmospheric Chemistry and Physics. The manuscript is well-structured and well-written and the earlier works are adequately referenced. Although some points require clarifications and revisions (see my list of comments hereafter), I recommend that the paper be published in ACP.

General comments

The actual task of collecting data at different sites is arduous but a key piece of research in the frame of satellite validation activities. The authors have done efforts to homogenize the results at best. However, the retrieval method employed at the different sites show important specificities, which renders the statistical analyses difficult. At least some information are missing for a proper understanding of the results (see specific comments).

Specific comments

- Page 8338: The manuscripts published in the ESA Special publication (ESA Conference Proceedings SP-562) are, as far as I have found, not easily accessible. I therefore wonder on the usefulness of having as many references to this special publication in the paper.

- Page 8339, Section 2, second paragraph: “MIPAS data are valid over variable altitude range”. What does ‘valid’ refer to? Is it related to large errors; Missing data? This should be clarified.

- Page 8340, Section 2, last sentence: “Beyond the limits of MIPAS measurements, the MIPAS profiles are extrapolated using the MIPAS initial guess profile”. It is not clear to me what has been done (what are the MIPAS limits), nor what impact this could have on the comparison.

- Page 8342, section 3.2.2.: A priori information is of course crucial when trying to retrieve profile information from integrated measurements. The fact that these information vary from site to site may be subject to criticism if the authors do not at least clarify some issues. In particular, the Sa covariance matrix is not defined at any stage. Is it the same at the different sites? How much does it vary from site to site? Without this type of information it is very difficult to judge on the results (DOFS, sensitivity range) and also on the statistic relevance of the comparison. Also the impact of using different

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microwindows (see Table 1) is unclear.

- Table 1: It is striking to note that same microwindows do not necessarily include the same interferers. Obviously, this has to do with the interfering species for which a column is simultaneously fitted. This should be at least stated but it also raises the question of the possible impact of the fixed model parameters on the retrievals.

- Page 8343, second paragraph: It is not clear to me why the DOFS for N₂O is larger at the Jungfraujoch. The altitude of the site is given as explanation but is it the physical reason? It is also the reverse for HNO₃. Is it due to the fact that one is a typical tropospheric source species and the other a stratospheric source species? Some explanations would help.

- Table 2: It is not clear at this stage how the sensitivity range is defined. Reference to the text should be made. Also, as stated above, it is hard if not impossible to compare the results without information on the a priori variability.

- Figures 2 and 3: Both figures could be limited to the altitude range 0-60 km. For HNO₃, the analysis of the averaging kernels is not straightforward. Table 2 reports a mean DOFS of 2.8 but one can hardly see where these informations are located, especially as there are important anti-correlations.

- Page 8344, last paragraph: The partial columns are not defined on the same altitude ranges for the different station: does that not impact at all on the statistics? Indeed, the ranges have been defined in terms of sensitivity without considerations of the possible error profiles.

- Figure 5: For N₂O, is it correct that the errors of the FTIR are close to 10 % in the troposphere? If yes, how does that compare to the prior uncertainty? Is a smaller variability for N₂O not expected?

- Page 8356-8357: The bias observed for HNO₃ is explained in terms of different spectroscopy. The differences in intensities are pointed out but the most recent databases

also differ, at least, from the point of view of the number of lines. Does this not affect the retrievals?

Technical corrections

- Page 8342: The term 'scaling factor' has not been defined; it is probably not necessary in the sentence.
- Page 8345, second paragraph, second sentence: "the biases are lower than $\pm 5\%$ ". Would "the biases are within $\pm 5\%$ " not be more appropriate?
- Page 8346, section 5.1.: The term 'smoothing error' has not been described. It should be either avoided, either briefly explained.
- Page 8345, second paragraph. Reference is made here to Figure 8, which appears much further in the discussion. Could this not be avoided?
- Page 8346, title of section 5.1. Would "Smoothing" not be more appropriate than "Degradation".
- Page 8357, third paragraph: It is not explained where the factor 0.863 exactly comes from (mean value of the intensity ratios?). To be consistent with the rest of the discussion, could it not be rounded off to 0.87?

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

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