

Interactive comment on “Harmonization and Diagnostics of MIPAS ESA CH₄ and N₂O Profiles Using Data Assimilation” by Quentin Errera et al.

Anonymous Referee #1

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1 General comments

The authors are having a close look at the data from the Michelson Interferometer for Passive Atmospheric Sounding (MIPAS) as retrieved by the ESA processor (versions 6 and 7). They use data assimilation to (i) fill spatial and temporal gaps in the data, (ii) smooth the data using both averaging kernel information and a chemical and transport model, and (iii) compare the MIPAS data with other data sets (ACE-FTS for CH₄ and N₂O and MLS for N₂O). The authors are using different configurations of their data assimilation system BASCOE (Belgian Assimilation System for Chemical Observations). This helps them to better characterise the MIPAS data as the effect of the assimilation could be assessed through the ensemble of assimilation configurations. In particular, they provide useful information for the possible users of this MIPAS product, e.g. their

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recommendation to use the ESA MIPAS data (versions 6 and 7) with caution in the lower stratosphere over the tropics.

I think this study provides a good insight of the ESA MIPAS data (versions 6 and 7) and the ability of BASCOE to assimilate them and provide a better product than the data alone. For that reason, I would advise to publish it in Atmospheric Measurement Techniques with minor changes listed hereafter.

Nevertheless, my main comment is that there is a mismatch between the introduction and the options the authors took to carry out this study. My reading of the introduction is that chemical transport models like BASCOE are the only alternative to study the stratospheric chemistry as resolving "state-of-the art chemical equation systems for the stratosphere is much too expensive for NWP models" (see comments below about this statement). Curiously this study uses a version of BASCOE without any stratospheric chemistry! The impact of this choice is not even discussed. I think part of the introduction should be rewritten and the impact of not using stratospheric chemistry for CH_4 and N_2O in BASCOE should be more discussed.

My second concern is about the assimilation experiment referred to as ENS-CR. In this experiment the correlation of the observation error are accounted for. It is well-known that for any assimilation system assuming zero correlation in the observation error when the correlation exists, one has to "tuned" the observation error variance. As a consequence, re-introducing the observation error correlation reduce the weight of the observations and the observation error variance should be "re-tuned". This was not done in this study where the observation error variance is the same for the assimilation experiment with and without observation error correlation. Figure 6 is a good illustration that the observation error variance should be changed for the ENS-CR experiment. I would recommend to have another ENS-CR experiment with an adjusted observation error variance to make more sense of this experiment.

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2 Specific comments

- page 1 line 5: "The CH₄ and N₂O profiles can be noisy". I am still not convinced by this statement after reading the whole text as there is no figure to really prove this or we can not get easily this information from one of the figures.
- page 2 line 30: Sentence starting with "An accurate representation ...". I do not see the link between this sentence and the rest of the paragraph. Please rephrase.
- page 2 line 35: To my knowledge (<http://www.geosci-model-dev-discuss.net/gmd-2016-40/>), the European Centre for Medium-Range Weather Forecasts (ECMWF) is having a version of their Numerical Weather Prediction (NWP) system with a state-of-the art chemical equation systems for the stratosphere (the same as BASCOE) as part of the Copernicus Atmosphere Monitoring Service (CAMS). Even if the CAMS resolution is lower than the operational version of ECMWF's NWP, the paragraph should be rewritten as the statement that resolving state-of-the art chemical equation systems for the stratosphere is much too expensive for NWP models is not completely true any more.
- page 2 line 44: Similar comment as before. I think CAMS operational real-time analysis and CAMS reanalysis are also starting points for a full present-day stratospheric composition analysis and reanalysis. This should be mentioned.
- page 3 line 73: Maybe it worth mentioning in that paragraph if BASCOE accounts for the cross-correlations between CH₄ and N₂O in the background errors.
- page 6 line 184: In the introduction it is claimed that NWP can not afford a detailed stratospheric chemistry (which is not completely right as discussed before) and that chemical transport models are the alternative for that. But in this study, no stratospheric chemistry is considered which would mean that BASCOE can not afford a stratospheric chemistry too? And the horizontal resolution of BASCOE for that study (3.75° × 2.5°) is much coarser than the CAMS operational system (TL511 or about 0.3° × 0.3°). The au-

thor choice not to have a stratospheric chemistry in this study should be more justified and harmonized with the introduction.

- page 6 line 186: With an assimilation window of one day and no chemistry in BASCOE I would expect to have model errors. Are these errors accounted for in the assimilation (weak-constrain algorithm)? If not, the authors should justify their choice.
- page 8 line 227: If σ_o and σ_b are vectors, then they should appear in bold. The computation of σ_b should then be detailed as it would be in the model space (37 levels) while σ_o would be in the observation space. The square in the left-hand term should also be detailed. If the square is an inner-product, then σ_o and σ_b are scalars and then we should know how they are computed as they are errors on a retrieved profile and a model profile respectively.
- page 8 line 258: I find the term “noisy” not specific enough. What the authors want us to see in this figure? Despite the usage of the “noisy” term, I found this paragraph confusing: CTR is noisy, BASELINE reduces the noise but the noisy structure in BASELINE is also present in BASEv7. Maybe this could be clarified.
- page 9 line 293: I do not see in Fig. 7 that ENS-CR has a high variability. I also disagreed with the following statement: "This suggests that the observational error covariance matrices provided by the MIPAS ML2PP retrieval are not optimal for data assimilation." This could also suggest that the observational error variance used in BASCOE are not correct. For example, one could inflate the observational error variance provided by the MIPAS ML2PP retrieval to account for the representativity error. Increasing the observational error variance could lead to reduce $J(x_a)$ (see general comment too).
- page 10 line 324: I think this should be investigated in particular by changing the observational error variance in ENS-CR.
- page 11 paragraph starting line 353: I have some issues with the discussion of the

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differences between MIPAS and ENS when MIPAS are assimilated and the title of the section is "Validation Against Independent Observations".

3 Minor revisions/comments

- page 1 line 4: "The retrieved CH₄ and N₂O profiles" instead of "The CH₄ and N₂O profiles" to avoid the confusion as it could be the profiles from BASCOE.
- page 1 line 9: "independent observations". Maybe you could provide the list of independent observations (ACE-FTS for CH₄ and N₂O and MLS for N₂O).
- page 1 line 14: "CH₄ and N₂O observations". Strictly speaking, these are not observations but retrieved data. Please be careful in the text with the usage of "observation".
- page 3 line 56: "CH₄ and N₂O are both emitted at the Earth's surface" instead of "CH₄ and N₂O are both produced at the Earth's surface" as there is some (chemical) CH₄ and N₂O production higher up in the atmosphere as well.
- page 3 line 60: "these retrievals". It is not clear "these" is referred to. Please detail.
- page 3 line 62: Do the issues are related to these particular retrievals or are they general issues? Please precise.
- page 3 line 66: "In their study" instead of "In that study"?
- page 5 line 142: What are "OR measurements"?
- page 10 line 9: "MIPAS v7 does not improve" at all or significantly "the quality of v6 for CH₄ and N₂O"? Please detail.
- page 11 line 339: sentence not clear. The ranges found by De Mazière et al. (2008) are the ranges of ACE-FTS incertitude?
- page 11 line 347: please give details on what Sheese et al. (2016) compared to have

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similar values.

- page 11 line 351: please provide values of these uncertainties.
- page 11 line 353: please precise if MIPAS averaging kernels have been used to compute the figure.
- page 12 line 374: I am confused. MIPAS discontinuities are illustrated Fig. 10 but the next sentence says that "the figure presents time series of daily averaged MLS N₂O", not MIPAS. I understand what you mean but the paragraph should be rephrased.
- page 12 line 386: you should detail the grid you used to compute these statistics (30° latitude band on the horizontal, and in the vertical?).
- page 13 line 414: you could detail **B** with "background error" here, just for the conclusion. Please rephrase as not all experiments are using a **B** matrix calibrated using an ensemble method. Moreover, you could also add that models (BASCOE for this study) are additional information to the raw observations in assimilation systems.
- page 13 line 432: could you please precise how this study shows that data assimilation can be considered a useful validation tool for geoscientific datasets?
- page 21 figure 5: why showing only the tropics? Maybe this could be discussed in the text. For the caption, I would use "scatter-plot" instead of "correlation" as the figure do not plot correlations. I would also add the labels (a) to (f): "observed by (a) ACE-FTS and from (b) to (f) for five BASCOE experiments".
- page 25 figure 9: it would be nice to have the labels (coloured line and legend) inside one of the subplots as for other figures.

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