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Interactive comment on “4D-Var Assimilation of MIPAS chemical observations: ozone and nitrogen dioxide analyses” by Q. Errera et al.

Q. Errera et al.

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We thank Referee #2 for her/his constructive comments. Our responses are preceded by the referee's comments given in italics.

General criticisms

1) I am unconvinced by the use of the MIPAS data to assess the quality of the analyses, since MIPAS data are assimilated to produce the analyses. I would think this is more a consistency check than an evaluation of the quality of the analyses. It is also not clear what information we gain out of comparing the analyses with "assimilated MIPAS" or "all MIPAS" data. I would like the authors to clarify this point.

Sect. 5.1 and 6.1 (i.e. BASCOE vs. MIPAS) are intended to evaluate to which degree BASCOE is close to MIPAS. Indeed, this is a consistency check, not a validation of

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the analyses. The new version of the paper clarifies this. The information gained by comparing stat1 (including assimilated MIPAS data only) and stat2 (including all MIPAS data) concerns the data filter: BASCOE could be very close to MIPAS assimilated data but far from the complete MIPAS dataset. This would mean, for example, that assimilated data are not representative of MIPAS in general. This last point has been explained more clearly in the revised paper.

2) The error analysis is a little rushed. I understand that the authors have made an effort to determine the reasonable estimate of total error for MIPAS data, but I am quite surprised that MIPAS data are not well characterised. As far as I know, a full error characterisation was performed in order to select the MIPAS spectral regions for retrieving the target species. This was done at the beginning of the mission. I would like the authors to acknowledge it when they explain MIPAS total error and compare their estimated total error for each species with the one estimated by the MIPAS team (e.g. www.atm.ox.ac.uk/group/mipas/err/). About the background error covariance, there is no explanation about how it has been characterised and why spatial and species to species error correlations are negligible. I would like the authors to address this point.

MIPAS errors are now described in more detail with additional reference to www.atm.ox.ac.uk/group/mipas/err/.

The background error covariance matrix (B) has not been characterized. The diagonal has been set empirically to 20% which, on the basis of our experience in chemical data assimilation, was looking reasonable. Spatial correlations help to spread the information from the data into the model. They could be neglected in a first approximation if the spatial coverage of the assimilated observations and their vertical resolution are comparable to the model resolution. This is the case here since a maximum of three days of MIPAS observations is sufficient to constrain all BASCOE grid points.

On the other hand, to our best knowledge, taking into account the species-to-species correlation in the formulation of B (which means doing multivariate assimilation) has

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never been attempted. For species that interact together with a time scale of the order of several days, their correlations are implicitly set thanks to the chemical coupling present in the model and its adjoint. Multivariate assimilation could be useful to constrain one unobserved tracer (e.g., N₂O) by an observed tracer (e.g., CH₄) using their compact relationship. The set-up of B has been discussed in more detail in the new manuscript.

3) I have found the paper a bit long with a lot of descriptions and not much interpretation of the results. It would be more easy to read if the results, that are already shown in the figures, are summarised in tables and give more space to the reasons or possible causes of disagreement.

Sect. 5.1 and 6.1 have been shortened and provide more interpretation of the results.

4) Finally, although the dataset produced takes into account a larger variety of atmospheric conditions than the MIPAS dataset, the paper does not make it clear what are the benefits of using this dataset instead of the MIPAS dataset itself, when for example it confirms the quality of the MIPAS data but it omits some scientific data (e.g. NO₂ data during periods of Solar Proton Events or Energetic Particles Precipitation events). It would be useful to mention also who are the possible users or customers for this dataset.

Compared with the observations, assimilated fields have an advantage by being given on a regular grid with a regular frequency. This should ease the interpretation of the state of the atmosphere. Model validation should also be easier when comparison is done against assimilated fields than against real observations. As mentioned by the referee, our assimilation system is not perfect and fails to reproduce some of the conditions observed in the data, like NO₂ observations during EPP events. So, in any case, assimilated fields can not be used blindly to replace observations in scientific studies.

In addition to instrument validation, assimilation could be used, for example, to (1)

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provide improved climatologies for NWP systems or inversion algorithm, (2) provide initial conditions or boundary conditions to modellers, (3) assess model performance as it is done in the paper, (4) provide information on unobserved species from observed species (mainly with 4D-Var; this is not discussed in the paper). Finally, (5) BASCOE analyses will be used by the SPARC CCMVal initiative to assess Chemistry Climate Models (see T. Erbertseder et al., The Promote Ozone Profile Service - Long-Term 3d Ozone Reanalysis Of Ers-2 And Envisat Data Sets, ESA Special Publication SP-636, 2007.)

Specific Comments

p8011, line 8: One of the goals of assimilation systems based on chemical transport models is the validation of satellite data. I imagine though it is not the main goal. Additional examples of the goals of DA systems based on CTMs might help in understanding the usefulness of your specific work.

See previous answer: these other goals of chemical data assimilation are now mentioned in the introduction.

p8011, line 23: It would be useful to mention here which of the MIPAS Level-2 products are assimilated and in which altitude range.

The BASCOE assimilation cycle used in this paper has assimilated all six retrieved chemical species. This is now stated in the paper. Level-2 version numbers of assimilated MIPAS observations are already given later on in the introduction (p8012, line4-5). The introduction is not a good section to mention the altitude range of the assimilated observations, because the observed altitude range differs from species to species and a full description would be too long in the introduction. The altitude range is described in the appropriate sections.

p8012, line 9-11: Could you describe a bit more the monitoring procedure?

This has been done.

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p8017, line 3: What do you mean by MIPAS observation space? Radiance space?

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In the sense of the assimilation, the observation space is the space where assimilated data are provided and the observation operator maps the model value to the observation space. In the case of MIPAS level 2 assimilation by BASCOE, the MIPAS observation space corresponds to the positions and time of the observed vmr, i.e., their coordinates (time, longitude, latitude, pressure). This has been be clarified.

p8018, line 7: Are the ozone error values estimates of precision or accuracy?

While for HALOE NO and NO₂ and POAM-III, we give agreement with correlative data, here we provide error estimates of HALOE Ozone. Thus HALOE Ozone quality will be specified regarding agreement with correlative data.

p8018, line 17-18: This sentence is not clear to me, could you explain what is the error at the terminator?

The sentence should read: " This is done to minimize the error of interpolation which could be non negligible at the terminator due to the maximum time shift of 15 min between BASCOE analyses and observations"

p8019, line 20: What is the observation space of the instruments? radiances?

See reply of p8017, l3

p8020, line 15-18: How do you characterise the background error covariance? Could you give an explanation on the 20% setting for all species? Explain also why neglecting the off-diagonal elements of the background error covariance is a reasonable approximation or give the reasons for choosing this simplification.

See reply to general criticism 2.

p8020, line 21-25: See general comment about MIPAS error characterisation and if possible add a reference to the error analysis done by the MIPAS team.

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See reply to general criticism 2.

p8020, line 25-27: This sentence is a bit confusing, could you please explain it better?

Indeed, this is confusing and the sentence has been replaced.

p8021, line 26 - p8022, line 4: As I mentioned in the general comment, I do not understand well the difference of these tests and how useful they could be since you have used MIPAS data in the assimilation system to get the analysis. I see these tests more as consistency checks instead of tests for assessing the quality of the analyses. I think that comparisons with independent measurements or MIPAS data that have not been assimilated is a better validation tool.

See reply to general criticism 1.

p8022, line 16-28: I do not clearly understand the meaning of this bias and standard deviations since as I said before the paper uses the MIPAS data in the assimilation system. It seems an incestuous comparison. This paragraph would be more clear if instead of describing the results that are visible in the Figure, you explain if these results are expected and possible reasons for disagreement (same for p8023, line 4-15).

See reply to general criticism 1.

p8021-p8024: It would be helpful to add a table summarising the results and where possible reduce the text.

See reply to general criticism 3.

p8024, line 21-24: There is a repetition of what already said in paragraph 3.3. Maybe this information could be repeated in Table 1 instead.

We have removed the repetition. Rather than repeating the information in Table 1, which would make it less clear, we now simply refer to Sect. 3.3.

p8025, line 28 - p8026, line 1 and p8026, line 10-12: It would be helpful to have a figure

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showing the absolute difference instead of numbers in the text, maybe you could add another panel in Figure 6.

Adding a new panel to Figure 6, that shows POAM-BASCOE, would not help. Showing the differences in absolute units would highlight other region of interest than the Ozone hole period, showing the differences in % is meaningless during that period. Instead, the sentence has been rewritten.

p8027, line 4-7: I am slightly confused about using MIPAS data to validate the analyses (as you describe in section 5.1) and now using the analyses, where MIPAS data have been assimilated, to validate MIPAS data. Is it not using the same information twice? Could you clarify this point?

But MIPAS data are not used to validate the analyses, they are used to check their consistency (see reply to general criticism 1). So, we don't see any problem here.

p8042: The table caption is not that clear: are you reporting biases with standard deviations or estimated uncertainties with their errors? In the table it is not clear which column reports HALOE and which POAM-III comparisons.

p8043: Same comment as for Table 1 caption.

The table caption has been rewritten.

p8045: Why are there zero observations for high altitudes? It would be helpful to add a comment about it in the caption or in the text.

At high altitudes, the vertical resolution of MIPAS is lower than the UARS pressure grid and for some pressure layers there are no observations. This is now mentioned in the legend of Figure 2 and 10.

p8049: Is it maybe clearer to say "at POAM-III locations" and to add what the red dots are?

This has been done.

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p8053: Same comment as Fig. 2 (p8045).

See reply to p8045

Technical Corrections

p8017, line 16-21: In summarising Cortesi *et al* results, it would be useful to report bias and precision/accuracy for the same altitude ranges and use consistently pressure (hPa) or altitudes (km) to describe the vertical range.

p8018, line 15: Again the vertical range is expressed in km although before pressure ranges have been used.

p8019, line 16: Spelling wrong for “version”.

These three points have been addressed.

Interactive comment on Atmos. Chem. Phys. Discuss., 8, 8009, 2008.

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8, S4581–S4588, 2008

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