

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

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General comments

This paper describes the validation of middle atmosphere ozone and nitrogen dioxide analyses based on nearly two years of MIPAS observations. As well as being useful to anyone who uses the BASCOE analyses, the paper has a number of other interesting results. Using the assimilation system as a transfer standard, MIPAS is compared to HALOE and POAM, in general confirming what was shown in the MIPAS ozone and NO₂ validation papers, but using a much larger dataset. Also, the study helps to indicate the areas where MIPAS data assimilation is able to improve on a free-running chemistry transport model: for ozone, in the lower stratosphere and the polar regions; for NO₂, the upper stratosphere and lower mesosphere. There are also a few areas

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identified where future improvements will be needed, such as where the assimilation system cannot capture the observed enhancements of NO₂ coming from energetic particle precipitation events.

There are a a number of specific comments that need to be addressed, but otherwise I would be happy to see this published in ACP.

Specific Comments

1 - p8011 l22: "for the first time" - this is probably the first time for a CTM with detailed chemistry, but Juckes (2007, ACP, "An annual cycle of long lived stratospheric gases from MIPAS") assimilated a year of MIPAS ozone, water vapour and methane with a no-chemistry model. Juckes also compared MIPAS ozone with HALOE and POAM using assimilation as a transfer standard, so it would be be worth mentioning his results in section 5.3.

2 - p8012 l19: "The monitoring procedure ... optimal" - it might be better to explain what this is and why it is 'optimal', e.g. the observations are passively monitored within the assimilation system, using a model-to-observation operator, as is often done in NWP.

3 - p8014 l1: I'd be interested to know how this system is different or similar to the one evaluated in the ASSET intercomparison, and this information would also be useful when the ozonesonde results from that study are mentioned on p.8025.

4 - p8015 l28 "minimization is not attained" - in my experience, though in a very different system, when the M1QN3 fails to minimise the cost function, the 'analysis' state remains very close to the background state. Is that the case here? If so, that would suggest that for these 44 days your analyses are really just a free model run, and do not properly assimilate the MIPAS observations. If so, I'd be worried about keeping these days in the comparison with independent data (p8016 l13). Can you also reassure the reader that when the minimisation fails the analyses are left in a geophysically sensible state and not, for example, left in some kind of completely erroneous mess?

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5 - p8020 I19 I think this paragraph on the MIPAS observation error needs a bit more explanation, e.g. what is the total error, what is it based on, how does it differ from other kinds of error mentioned in the text, particularly the observation error used in the data assimilation scheme?

6 - p8020 I26 "MIPAS total errors .. are ... close to the total errors" - I'm confused! It seems you are using one name for two different concepts.

7 - p8021 I10 "A better filter..." - I can't see how this proposal would allow observations of enhanced NO₂ coming from SPEs to be assimilated?

8 - p8021 Section 5.1 - It's worth making clear that comparing MIPAS analyses to MIPAS is basically a test of the quality of the assimilation algorithm, and an opportunity to demonstrate the areas where the data is of benefit to the system - not an independent verification.

9 - p8022 Fig. 2 and MIPAS total error. Which "total error" is being shown? (See point 8).

10 - p8022 Fig. 2 and MIPAS total error. It's good to have a reference point by which to judge the size of the biases and standard deviations of (MIPAS - analysis) statistics, but can you justify using total error here? There are a number of things that worry me about this. First, if total error is made up of systematic and random components, surely you should be showing just the systematic part on the bias plots, and the random part on the standard deviation plots? But a still better measure of self-consistency of analysis, observation, observation error and background error would be to do something like the chi squared test, e.g. Menard et. al (2000, Mon. Weath. Rev, "Assimilation of Stratospheric Chemical Tracer Observations Using a Kalman Filter. Part II ...") who looked at observation minus first guess statistics. See Rodgers et. al. (2000, World Scientific, "Inverse Methods for Atmospheric Sounding") eq. 12.9 for the equivalent expected covariance matrix for observation minus analysis departures.

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11 - p8023 I1 - would not the errors of MIPAS (e.g. when clouds influence the observation) also be a factor in the troposphere?

12 - p8024 I12 - why would extra model layers improve the photolysis calculations? Because the UV radiation reaching 0.5hPa would be more accurately modelled?

13 - p8029 I14 - "rejected data are filtered out due to their variability and not because ...the conditions are not modelled". I don't understand how this statement is justified.

14 - p8030 I7 - "this is due to the .. low amount of NO₂" - but please remind us of the link between the low amount of NO₂ and the fact that the observations are rejected.

Technical corrections

15 - p8015 I4 "background covariance" - it is the background ERROR covariance

16 - p8020 I23 "important" -> "high"

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