

Interactive comment on “An annual cycle of long lived stratospheric gases from MIPAS” by M. N. Juckes

M. N. Juckes

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My thanks to the reviewers for their accurate, useful and detailed comments. I would like to resubmit the manuscript with extra figures to show the latitudinal variation of the validation data (splitting it into 6 latitudinal bands) and a more detailed discussion of related recent literature.

Response to A. Geer:

Main Comments.

A) The inclusion of more validating datasets has led to a compromise, with less detailed discussion of each satellite. In the revision I will try to address to specific issues raised with some results from data averaged in latitude bands. I have added a table to summarise the amount of data from each instrument, and figures with results binned in

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6 latitude bands. These figures show the latitudinal extend of the different instruments and answer some further questions raised.

B) The peaks in the observations are clearly due to noise, but it is not clear that the bias is unreasonable. Lahoz et al. (2006) dismiss MIPAS water vapour above 850K because of its noisiness. Here, the assimilation is capable of removing some of the noise. The comparison with POAM III had been done with version 3 of the level 2 data. This has now been updated to version 4. The result is that the bias in the upper stratosphere is reduced, though the noise remains.

Pan et al. (2002) state that ILAS water vapour has a high bias compared to the UARS climatology of Randel et al. (1998). This is a little different from saying it has a high bias full stop. It appears that everything has a high bias compared to HALOE in the upper stratosphere. In HALOE's favour is the fact that it produces near constant 'total observed hydrogen' (or 'equivalent water'). However, there does not appear to be enough evidence to reject the hypothesis that there is a source of 'total observed hydrogen' in the upper stratosphere.

The Milz et al. (2005) analysis is based on only 11 days of data spread over 3 months. The water vapour concentrations in the ESA product I have used are also lower in June and August than in July, so that if a 3 month period is compared the differences to the Milz et al. data are reduced. Their figure 7, which is comparable with my figure 5, shows that they have comparable biases relative to HALOE in the upper stratosphere. The revised manuscript refers both to this and to the high noise in the MIPAS data.

Thanks for drawing these papers to my attention.

The question as to the absolute correctness is of HALOE or MIPAS is certainly not decided here, but the low standard deviations of HALOE against the analysis clearly indicate that it is possible to get meaningful information out of the MIPAS observations, even though they are noisy.

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Concerning figure 14, total observed hydrogen: this field is expected to be less variable than methane and water vapour, but it is not expected to be totally constant. This is discussed in the text. The plots are clearly more noisy than others, but still reflect some undoubtably real features. The fact that comets are hitting the atmosphere is not speculation, but the quantification of the associated mass flux is.

- (1) Dealt with in more detail elsewhere, a brief comment and reference added.
- (2) Details of interpolation added.
- (3) The reviewer is correct in that the signal comes from the Southern vortex. A new figure has been added to show this. However, the signal in the Southern vortex is not clearly distinguishable from noise, so the comment on this aspect of the POAM III comparison has been removed.
- (4) The jump is reduced in the level 4 POAM III data (I had previously used level 3, but these were superceded during the summer by level 4 – there are significant differences in the water vapour). There is also a slight modification, relative to the submitted manuscript, in the SAGE II comparison, owing to detection of a bug affecting the dating of a small number of profiles.
- (5) True.
- (6) This is a factor, but agreement with HALOE is also better at high latitudes.
- (7) It is stated in the manuscript that the effective smoothing is part of the explanation.
- (8) ECMWF temperatures (be clarified in text).
- (9) OK.
- (10) Ref. to Lahoz et al. (1996) added.
- (11) References added.
- (12) More discussion added. The descent rates have also been expressed in SI units.

(13) It should be referred to as “total observed hydrogen”: the figure legends and text have been corrected.

(14) It is noisy. There is no claim that it persists smoothly.

(15) Yes, there is a water vapour problem, but, as noted in the general comments above, this does not stop the analysis agreeing well with HALOE.

(16) These figures emphasise the region where the satellites provide most information. The shading, showing percentages, ensures that they are not misleading.

Anonymous referee #1:

The zonal mean plots have been reduced in number (now showing Jan, Apr, Jul, Oct instead of Jan, Mar, May, Jul, Sep, Nov) and slightly enlarged. New plots to show the latitudinal variation in validation data have been added. Another new plot shows the evolution in the southern polar lower stratosphere in more detail.

(1) OK.

(2) Extra figures have been added to show the validation data in 6 latitude bins.

(3) Done.

(4) “Standard error” corrected to “standard deviation”.

(5) Done.

(6) Reworded and expanded, with reference for potential smoothing effect of variational data assimilation.

(7) These were not shown for ozone because the plot becomes too noisy. In the revision they have been included for the ozone means, but not for the standard deviations. A comment to this effect has been added in the caption of figure 4.

(8) It shows added value of assimilation.

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- (9) Other estimates in literature are now referred to.
- (10) Sentence removed.
- (11) Reference to figures added.
- (12) Reference to tape-recorder clarified.
- (13) OK.
- (14) The drop in potential temperature is counter-intuitive because it coincides with rising temperatures: this is now explained in the text.
- (15) OK.
- (16) Reference for drying out added.
- (17) Discussion added.
- (18) Comment removed.
- (19) English corrected.
- (20, 21) Text clarified.
- (22) Corrected.
- (23) Corrected.
- (24) Changed.
- (25) Reworded to spell out meaning of 'it' (recovery of total observed hydrogen).
- (26) Clarify discussion of TOH: greater noise, some signal, possibly.
- (27) "Meridional circulation" is sometimes used to refer to the circulation in a height-latitude plane (see, e.g. http://nsidc.org/arcticmet/glossary/meridional_circulation.html), but changed to a more precise wording.

(28) I wouldn't say that it was fundamental. It will be interesting to look at data from other years to see if this difference persists.

(29) Corrected.

(30) Some comments on OSIRIS have been added.

(31) Thanks.

(32) Crosses removed. "Northern Hemisphere" added to caption of figure 10.

(33) Corrected.

Response to A. Dudhia

(1) Or perhaps one paper covering a range of topics. One of the reviewers of my first publication on MIPAS ozone suggested that it was pointless publishing lots of error statistics if the data did not throw light on the physics of the atmosphere. In this paper I've tried to go a bit further than statistics and start to look at the processes revealed by the assimilation. Assimilation may not be essential, but it does contribute added value by reducing the standard deviations in the validation statistics.

(2) Yes, it would be nice, but in NERC has not judged it nice enough to be worth funding (at least not in the form I proposed recently). I hope to extend the analysis in the near future, time permitting.

(3) Changed to "operational at full resolution".

(4) A reference to Vivienne Payne's thesis has been added.

Interactive comment on Atmos. Chem. Phys. Discuss., 6, 9389, 2006.

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