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Atmospheric Chemistry and Physics Discussions

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

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This paper presents results from the assimilation of one year's worth of ozone, water vapour and methane data from MIPAS, using a fast, new assimilation technique. It is one of the first papers to examine a long timeseries of MIPAS tracer data. The paper investigates well known features of the annual cycle, such as the water vapour tape recorder, descent from the mesosphere, and the conservation of total hydrogen in the stratosphere. In general these results are not new, but much of the value of such a paper is its contribution to the validation of MIPAS data: Do MIPAS results agree with the existing body of knowledge? If they do not, is that because of errors in the MIPAS data, or has MIPAS identified new geophysical behaviour? As such, this work should be published in ACP, but there are two main areas which need substantial revision and more comparison with previous work.

Main comments

A) It is good to see so many different satellite data types used in validation of the MIPAS-based analyses, but there is no consideration of the different spatial and temporal sampling that comes with such diverse instruments. For example, POAM III samples two latitude bands between 55 to 70N and 65 to 85S, i.e. only at high latitudes. HALOE (and similarly, SAGE) samples two slowly varying latitude bands covering roughly 60S to 60N (with some higher latitudes). Despite this, Figures 1-6 present bias and standard deviations between the MIPAS analyses and independent data as global means. This is misleading, given the very different spatial sampling, and invites the reader to compare, say, HALOE biases to POAM biases, when these come from very different regions of the atmosphere. There is an implicit assumption in these figures that the bias of MIPAS is fixed in time and space, and does not vary with the meteorological situation through the year. This is unlikely to be the case.

There are many unanswered questions. An obvious example is the time-varying bias between MIPAS and POAM III water vapour in Fig. 2. Does this come because the two latitude bands sampled by POAM are changing in time, and we are seeing the effect of a latitude-varying MIPAS bias? Does it come from the changing nature of the high latitude atmosphere, with MIPAS and/or POAM biases varying as the vortex builds and decays again?

There is a lot of very useful cal/val information hidden within the global means. I would suggest that section 3 is expanded, because such cal/val results would be of interest to many people. At the moment, no useful cal/val conclusions can be drawn because there is a misleading use of global mean biases. To understand what we see in the global means, the study needs first to examine biases resolved by latitude (or equivalent latitude), by time and by vertical level. Such figures could be put in the online supplementary material and/or selectively included in the paper.

B) There are a lot of strange-looking features in the water vapour and "total hydrogen" fields above 30km, and MIPAS quite often shows substantially more water or hydrogen than has been seen in previous studies. There are two hypotheses: either (1) MIPAS measurements are wrong, or (2) MIPAS reveals a new source of hydrogen. Neither is discussed in enough depth. I believe that the MIPAS water vapour measurements are unreliable above 30km and this is argued in Lahoz, Geer and O'Neill (QJRMS, 132, 1985-2008, 2006, published only a few days ago so not yet seen by the author of the paper under review). In our QJ paper, we did not use MIPAS water vapour above 850K/35km because we thought the retrievals were erroneous. Particularly in the high latitude autumn and early winter, above 30km, water vapour retrievals were extremely noisy and often showed little agreement with dynamical features in ECMWF PV. In contrast, MIPAS methane and ozone appeared to agree with both well-known science and ECMWF PV fields. This is much clearer to see when looking at the original data (see Fig. 6 in Lahoz et al. QJ, 2006), rather than assimilated, data. I believe that the paper under review shows more evidence that MIPAS water vapour fields are unreliable in the upper stratosphere.

The area of most concern is located between 35 and 45km from March and May in the South Pole (Fig. 7d) and from September to November in the North Pole (Fig. 7e). Fig. 3.9 in the SPARC assessment of upper tropopsheric and stratospheric water vapour (2000) shows what would be expected climatologically, based on HALOE and MLS. The MIPAS analyses show a constrasting picture, with big oscillations in time, sometimes achieving mixing ratios of 9 ppmv, far larger than seen in previous studies. Around the same time, as pointed out in the paper under review, Fig 2 shows large rises in the standard deviation of (MIPAS - analysis) water vapour. I think this is due to poor H2O retrievals. In contrast in the corresponding MIPAS methane fields (Fig. 7g&h) there is the expected slow wintertime descent of isopleths, suggesting the CH4 data is more reliable.

More generally, MIPAS shows upper stratospheric moisture mixing ratios of 7-8ppmv

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(Figs. 12-13), higher than seen in many previous studies. In contrast, the HALOE/MLS climatology of Randel et al. (1998, JAS) shows H2O peaking around 6.6 ppmv in the upper stratosphere. Pan et al. (2002, JGR) did find WV mixing ratios as high as 7.5ppmv in the upper stratosphere, using the ILAS instrument, but considered this to be a bias. Also, Milz et al. (2005, JGR) show research retrievals of H2O from MIPAS (done at IMK), rather than the ESA retrievals used in the paper under review. Their Fig.8 can be roughly compared to Fig. 13a in the paper under review. The IMK retrievals do show mixing ratios peaking at 7.5ppmv but certainly not the roughly 8.5ppmv at southern high latitudes in Fig. 13a. This would suggest the high latitude ESA WV retrievals are wrong, but leaves open the question of the high values at other latitudes in the upper stratosphere.

It is also possible to evaluate the quality of the MIPAS data by examining the quantity 4[CH4] + 2[H20] (often assumed to approximate total hydrogen), which numerous modelling and observational studies have shown is relatively constant through the stratosphere outside regions of dehydration or descent from the mesosphere. See e.g. Nassar et al. (2005, GRL), Engel et al. (1996, JGR) and Le Texier et al. (1998), and references therein. If we expect this quantity to be constant, Figs 12, 13 and 14 would indicate substantial problems in the MIPAS H2O or CH4 retrievals. Figs. 12 g,h,i and 13 g,h,i show rather higher total hydrogen in the upper stratosphere than the lower stratosphere, suggesting MIPAS H2O or CH4 retrievals are not consistent with the large body of previous studies. Further, total hydrogen appears to fluctuate wildly in time and in height in Figs. 14a and b. From the previously discussed evidence, I think a lot of the features in these plots are again down to poor quality H2O retrievals.

These criticisms should not prevent such figures being shown and discussed in this paper, but I think the most reasonable conclusion would first be to reinforce existing concerns over MIPAS H2O retrievals, rather than to briefly speculate on new sources of hydrogen reaching the upper stratosphere.

Detailed suggestions

1) p.9392, Eq. 1: Please explain the significance of the constants c_1, w_ap and w_num, and how they have been chosen (presumably to give a certain spatial / temporal smoothness to the analyses, and to adjust the balance in the analyses between observations and isentropic advection?).

2) p.9393, l.10-16: It would be useful to describe the spatial and temporal coverage of these instruments (see point A). Also, how were these retrievals mapped onto isentropic levels, e.g. what pressure and temperature profiles were used?

3) p.9393, l.23: The "slight shift relative to POAM" - presumably this can be traced to a particular geographical region (point A again)?

4) p.9394, l.9: Again point A - is it possible to explain the jump at the start of August?

5) p.9394, l.17: The increase in WV standard deviations (see point B). I suspect these can be traced back specifically to the high latitudes of the autumn/winter hemisphere.

6) p.9395, l.10: I thought POAM was a reasonably accurate instrument. Why are POAM WV standard deviations so large compared to HALOE? Going back to point A, a plausible hypothesis (though one that needs checking) would be that POAM is sampling mainly in the region of the vortex, where synoptic WV variability is very large, whereas HALOE is capturing a lot more of the tropics, where day-to-day variability is very small. What would you see if you used samples of POAM and HALOE with similar spatial/temporal characteristics? Also, can some of these differences be related back to known features reported in any Cal/Val papers published on SAGE II and POAM?

7) p.9395, l.20: The finding that standard errors are larger in nearest-neighbour comparisons is an imporant one, that would underline the usefulness of data assimilation for cal/val, so perhaps this should be mentioned. However, this should be qualified by the fact that the analysed datasets may have lower standard errors because in the analyses, the MIPAS observations have effectively been averaged and smoothed.

8) p.9396, l.19 (Fig. 8/9): Which temperatures are used here? ECMWF or MIPAS? I

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can't find any explanation of the data shown in these figures.

9) p.9397, l.11,15, p.9398, l.20, etc.: It would be useful to give an indication of what the "mid-stratosphere" or "upper stratosphere" etc. means in each particular context, in the vertical coordinate relevant to the figure being discussed, i.e. "upper statosphere (35-50km)"

10) p.9398, l.12: Where does this differ from the usual process of top-down break-up of the polar vortex (e.g. Lahoz et al., QJ, 1996), which results in vortex air (i.e. low CH4, high H2O) being overlaid by midlatitude air with high CH4 and low H2O?

11) p.9399, section 5.1, Figs. 10-11: This sequence shows very clearly the formation of a Frozen-in anticyclone (FriAC). It would be useful to give a brief mention of the FriAC and reference Manney et al. (2006,GRL), who first identified the phenomenon, looking at the 2004 northern spring. The particular one shown in Figs. 10-11 was tracked for several months afterwards (Lahoz et al., QJ, 2006).

12) p.9400, section 6.1: This section would be better if it could provide a summary of some more of the many previous works on the subject (see comment B above), with a particular emphasis on comparing previous stratospheric total hydrogen estimates to those from MIPAS. This would show that MIPAS estimates, particularly in the upper stratosphere, are very high compared to previous work. It would be worth considering showing the quantity 2[CH4]+[H2O] for compatability with these previous studies.

(see point B for general criticisms of this section: I think that MIPAS error is by far the most likely explanation for the high total hydrogen and other strange features in the field)

13) p.9400, l.18: It should be made clearer in the text that the "total hydrogen" shown here is an approximation: it does not contain a contribution from [H2].

14) p.9402, section 6.1, l.9-11: "The persistence of high total hydrogen in the vortex core suggests there is some truth in the high values". It does not persist. Fig. 14a

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shows clearly that at around 40km in the centre of the SH vortex, what was apparently a descending blob of high total hydrogen suddenly vanishes in June, round the time of an instrument outage. That would also suggest instrument problems.

15) p.9402, section 6.1, Figs. 15-16: In my own experience with the un-assimilated MIPAS data at these time periods and levels, the noisiness in the total hydrogen field comes mostly from the water vapour observations (though not exclusively - MIPAS methane also suffers spurious vertical oscillations). The noisiness in Fig. 16e looks to be further evidence of a MIPAS WV problem.

16) Fig. 4: The region 400-850K (i.e. the lower half of the stratosphere) is badly served by these figures, because it is both squashed vertically and has small ozone mixing ratios. Perhaps consider a ln(potential temperature) vertical axis, or displaying errors as a percentage of the mean ozone amount.

Technical corrections

i) p.9392, Eq. 1: Be consistent with differential notation: suggest "chi_t" replaced by "dchi/dt"

ii) p.9393, l.17: Commas need better placement in this sentence

iii) p. 9394, l.27: "Mean differences are low": Would use of the word "small" make the meaning clearer here, compared with the next sentence: "MIPAS is measuring low".

iv) p.9398, l.10: "descend" should be "descends"

v) Caption Fig. 4: The explanation of the "1-5% range" seems unclear in its meaning. Presumably 5% refers to 5% of the mean assimilated profile but it took a while to work this out.

vi) Caption Fig. 8: It would be helpful to restate the latitude range of each panel.

vii) Figs. 10 and 15: Are the crosses mentioned anywhere in the text? If not, they should be removed. Also, it's impossible to see what colour they are.

viii) Caption Fig. 12: last line -> "isentropic levels"

ix) Caption Fig. 15: Please add (a) and (b) etc. to the caption.

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