

Interactive comment on “Reassessment of MIPAS age of air trends and variability” by F. J. Haenel et al.

F. J. Haenel et al.

florian.haenel@kit.edu

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Replies to the Comments:

The authors thank the reviewers for their insightful comments. In the following, the comments are included in *italic face* while our replies are printed in normal face. In the resubmitted manuscript the changes are marked by colour.

Reviewer #2:

Comment:

This paper discusses revised SF₆ measurements from the MIPAS instrument and the mean age of air derived from those measurements. The most prominent change in the C7355

mean ages compared to the previous version of SF₆ is the removal of an unphysical minimum in mean age in the tropical middle stratosphere. The removal of this feature also changes the sign of the trend in the tropical middle stratosphere from positive to negative in the new version. This is a significant change and the more realistic distribution of mean age gives some confidence that this change is robust. Overall, the work done to improve the MIPAS SF₆ dataset described in this paper is substantial and of great benefit to the atmospheric science community. My only issue with this paper is the similarity in some of the discussion and figures to Stiller et al., 2012. I realize that this paper is an update of the work in that paper but the emphasis should be on the substantial differences between the new and old versions of SF₆ and mean age. Some of the figures in the current paper don't seem different enough to be shown again here, in particular Figs. 3, 6, 8 and 11. Removing those figures and some of the discussion of them would shorten and better focus the paper on the important points.

I recommend the paper be published with some figures and discussion removed as suggested above and consideration of the following specific comments.

Reply: We have decided to delete Figures 3, 6 and 8 in the main paper. For the interested reader we will provide these figures in the electronic supplement. We are reluctant to remove Figure 11. Admittedly this figure is very similar to the corresponding figure in the Stiller et al. paper. However, we discuss aspects not discussed in Stiller et al. and we want to avoid that the reader has to consult Stiller et al. to follow our discussion. The discussion is shortened where appropriate.

Specific comments

Comment:

Pg. 14687, line 7: remove “however,”

Reply: agreed and removed.

Comment:

Pg. 14689, lines 2-25: These two paragraphs could be cut down to a couple of sentences. All of the details on MIPAS and ENVISAT have been published previously and can be referenced, such as Fischer et al., 2008.

Reply: Agreed and reworded in a more compact manner.

Comment:

Figure 1: It's really hard to see any of the features described in the text on this figure. The lines are too small and the colors are too faint. There are also way too many unnecessary molecules listed in the legend on the right side since you can only see about three of them on the figure. The scale needs to be expanded, lines need to be thickened, colors made brighter and most of the molecules removed except those discussed in Section 3.

Reply: Agreed, the figure is changed.

Comment:

Pg. 14694, line 2: change to "ozone does not contribute much to the signal in the microwindow ..."

Reply: Agreed and changed.

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Comment:

Pg. 14695, lines 12-13: change to "This allowed more information from higher altitudes..."

Reply: Agreed and changed.

Comment:

Pg. 14695, lines 21-22: change to "... (upper panels) and the previous setup (lower panels)."

Reply: Agreed and changed.

Comment:

Pg. 14695, line 25: change "fitted" to "fit"

Reply: Agreed and changed.

Comment:

Pg. 14696, line 2: change "could be" to "was"

Reply: Agreed and changed.

Comment:

Pg. 14696, line 4: change "happened to disappear" with "was removed"

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Reply: Agreed and changed.

Comment:

Pg. 14696, line 26: What do the numbers 4-6, 7-10 and 12-18 represent? The vertical resolution in km in the previous and current version of SF₆? Need to be more specific.

Reply: The vertical resolution at one altitude varies because it depends on the actual atmospheric situation. The numbers reported are the ranges in which the vertical resolution varies in units of km. We agree that our original wording was ambiguous and have rewritten this statement. Missing units have been added.

Comment:

Pg. 14699, lines 1 and 27: change "more" to "longer"

Reply: Agreed and changed.

Comment:

Section 5.2: This section could be shortened considerably with the focus on just the differences from the previous version. Could combine Sections 5.2 and 5.3.

Reply: We have tried to shorten these sections, particularly where they were redundant with Stiller et al. However, whenever our discussion extends beyond that of Stiller et al., e.g. by putting the results in the context of additional literature, or where results have changed, we have left the original text untouched.

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Comment:

Pg. 14710, line 4: remove "do"

Reply: Agreed and changed.

Reviewer #3:

Comment:

The paper presents an improved data set of SF₆ retrieved from MIPAS measurements, and the derived AoA data set. The results are well presented and the paper is well written. I only have a few minor comments and can recommend the paper for publication.

General comment: My only significant objection concerns the presentation of AoA trends: I strongly advise the authors to only show the "model-error corrected" trend, as presented in Sec. 5.4. I don't see a reason to first show the uncorrected version of the trend, and I would fear that as it is now, it will mostly be referred to Fig. 9, where the significance is strongly overestimated. If there is a good reason for showing the "uncorrected" version as well, please state so, but also in this case I would advise to first show the corrected version and later the uncorrected one.

Reply: We agree and we now use the model-error corrected trends throughout the paper. This implies that Figure 9 is omitted and the discussion of the AoA trends refers to Figure 13. Consequently Figure 10, 11 and 12 have been changed and refer now to the model-error corrected trend.

Comment:

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Another general comment is the question, whether the “overaging” by the SF₆ sink in the mesosphere could influence trends, in particular the strong positive trends in the SH polar region. A very valuable addition to the paper (that answers this question) would be to show, next to the trend in KASIMA AoA derived from SF₆, the trend in an idealized AoA tracer (i.e. no sinks, perfect linear increase). This would allow to better evaluate how strongly and in which region the SF₆ sink (and possible artefacts due to the non-linearity) influence the trends.

Reply: This is an interesting issue indeed but we think it is outside of the scope of this paper where we focus on the presentation of the new results of observed SF₆ using the modified retrieval setup. For the period 2005/6 2010/11 KASIMA model results of an idealized tracer have been presented in Mahieu et al. 2014 (Appendix), showing “trends” of the order of 0.4 y/decade in the SH polar upper stratosphere, much smaller than the trends found here. Since mesospheric loss is caused at higher altitudes where the nudging of the model to the analyses is weak, there is additional uncertainty in the model results which deserves a detailed analysis. A dedicated paper on the related model evaluation is in preparation.

Specific comments:

Comment:

- page 14687, line 8: the BDC is not only the residual circulation, but is the mean transport circulation through the middle atmosphere. (I.e. it also includes mixing effects, diffusion, as you state later).

Reply: We agree; this error has been corrected.

Comment:

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- page 14690, line 12: Why is the new ESA version of the data they superior? Include a reference?

Reply: This is a good point indeed, because the improvements are particularly relevant to SF₆. The older versions suffered from baseline oscillations which caused an additional uncertainty in the SF₆ retrieval and needed special treatment in the retrieval as published by Stiller et al. (2008). Stiller et al. 2012 used better calibrated data for the period 2005-2010 but had to rely on the old calibration for 2002-2004. In our data set we use the better calibrated ESA version 5 level 1 data throughout, which no longer exhibit these oscillations. This is now discussed in the revised version.

Comment:

- Section 3.5 / Figure 2: Is Fig.2 a good example of how the residual was reduced? I.e. is it typical for other heights / regions / times? Is the RMS given in line 1, page 14696 the one for this example or for all data? If the former is true, it could be worth mentioning the improvement for all data, and possibly the improvement as function of region (height, latitudes)?

Reply: Yes it is a good example. The RMS given in the text refers to the example shown in Figure 2. A table containing the improvement of the RMS for different latitudes and altitudes for typical cases has been added. It is not possible to show the improvements for the whole data set and it should be sufficient to present only a few examples.

Comment:

- page 14696, line 25; This information would be appropriate already in Sec. 3. What

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does "4-6 in 20 km" refer to?

Reply: We agree and have moved this sentence to Section 3. As said above, the altitude resolution varies with latitude. The range given represents this variability in units of km. This has been clarified in the text and missing units have been added.

Comment:

- page 14699, line 26: "maybe"? → "only slightly..." (if it's true, otherwise delete).

Reply: Since this figure has been removed, this has become obsolete.

Comment:

- Section 5: Is there a reason you do not allow for seasonal variation in the regression coefficients for QBO and the trend (via a Fourier Expansion of those coefficients, as you include for the mean annual cycle (c_n and d_n))? Please comment.

Reply: For the QBO we use proxies, so all seasonal variations should be included implicitly. The seasonal variation of the trend is an interesting issue indeed, however, the inclusion of further fit variables was avoided in order not to destabilize the fit. Currently there are still technical problems to be solved to do such an analysis. Therefore we want to address this issue in a future paper.

Comment:

- page 14702 top / Fig. 8: I would move the discussion of Fig. 8 to page 14701, line 20 (i.e. before Sec. 5.1), as it is relevant for the whole regression fit rather than a specific

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topic for the trend.

Reply: Since this figure has been removed, this has become obsolete.

Comment:

- page 14709, line 15: the region of the "negative tongues" in KASIMA AoA are not significant in MIPAS trends (and only in the SH in KASIMA) - So there is no actual disagreement, is there?

Reply: This is not quite true. The reason is roughly this. KASIMA is a nudged model, i.e. in wide parts of the atmosphere it represents the real atmosphere. This implies that the atmospheric variability patterns which are responsible for the error of the multilinear model is the same between KASIMA and MIPAS, or at least it can be assumed highly correlated. Thus, this error characterizes the expected difference between the regression function and truth, but cannot necessarily account for the differences between MIPAS and KASIMA. For this comparison the figures without consideration of the model errors are more adequate. These figures are attached in the supplement and show that the region of the "negative tongues" is significant in KASIMA whereas it is significantly positive in MIPAS. A note on this has been included in the text.

Comment:

- page 14711, line 14: Another important conclusion is that the ERA-Interim data, used to nudge KASIMA, apparently are able to reproduce the observed transport trend, which validates their usage for studies of (even trends in) the BDC.

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Reply: We agree; this conclusion has been included in the paper.

Technical/ Language suggestions:

Comment:

- page 14695, line 2: *"improve retrievals..." (remove first "other")*

Reply: Agreed and changed.

Comment:

- page 14698, line 10: *change sentence to: "Stiller et al. (2012) estimated the global effect of overaging to about for the Northern Hemisphere."*

Reply: Agreed and changed.

Comment:

- page 14699, line 22: *(their Fig. 4) (Add "their" to avoid confusion).*

Reply: Agreed and changed.

Comment:

- *Fig.1: labels are too small, and colors of individual lines are hard to distinguish*

Reply: Agreed and changed.

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Further changes:

During revision of the results presented in our paper we have noticed a small bug in our trend program. This implies that the Figures 9, 10, 11, 12, 13 and 14 have slightly changed. However, the general statements in the discussion of these figures remain valid.

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