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Atmospheric Chemistry and Physics Discussions

Interactive comment on “Intercomparison of ILAS-II Version 1.4 and Version 2 target parameters with MIPAS-Envisat measurements” *by* A. Griesfeller et al.

A. Griesfeller et al.

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Reply to Referee #1

First of all, we would like to thank the reviewer for his helpful comments. In the following, our reply to the specific points are listed (the italic parts are the reviewer’s comments.).

Major comments

1. *Based on ILAS averaged profiles taken between May and October 2003 and correlative MIPAS data, differences between ILAS and MIPAS are presented for both ILAS versions. I found the discussion on these differences only approximative. In addition to the description of these differences, the reader would like to know*

if they are significant or not. How do these differences compare with the error of measurements and the uncertainties of the intercomparison method? Are the differences between ILAS and MIPAS comparable to the differences between MIPAS and other correlative data? These are the questions that the users of ILAS data are asking.

ANSWER: The reviewer has suggested to compare the mean differences with the measurement errors. We have decided to compare the mean differences with the standard errors of the mean differences instead, in order to assess their significance. The reason for this decision is that almost all error sources are of random nature and average out in the calculation of the mean differences. The mean differences reflect the relative bias between the instruments under assessment rather than the random errors. The standard errors of the mean differences include both systematic errors of the ILAS-II and MIPAS data sets as well as non-random errors due to non-perfect coincidences. Thus, the standard error of the mean difference is the appropriate quantity to assess the significance of the mean difference.

In the revised version comparisons of MIPAS with other correlative data have been included.

2. *I found the description of ILAS version 2 confusing. It took me several readings of the paper to understand the differences between versions 2.0 and 2.1 (see below). It also seems that ILAS v1.4 is better than v2 in the Southern Hemisphere (SH). Why? While the new correction of the transmittance (v2.1) shows a clear improvement, I am not convinced by the improvement made in the version 2.0, i.e. the upgrade of the spectral parameters and the upgrade of the tangent height registration. I think the paper should first show how and why ILAS version 2.0 improves the previous version. Then, the discussion of version 2.1 and the related results can be presented.*

   ANSWER: To avoid the confusion, we just use Version 2 (V2) now. Differences

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between the satellite sunrise and sunset mode measurements in V2 are now clearly mentioned in Appendix in the revised paper.

We do not think that the V2 data in the southern hemisphere (SH) are inferior to the V1.4 data, considering validation results of the MIPAS data or other validation sources. Generally, both V1.4 and V2 data compare well with the MIPAS data except for H_2O. For H_2O data in SH, the current ILAS-II data has an issue regarding the presence of PSCs (i.e., below about 25 km). In both V1.4 and V2, a simultaneous gas-aerosol retrieval (Oshchepkov et al., ILAS data processing for stratospheric gas and aerosol retrievals with aerosol physical modeling: Methodology and validation of gas retrievals, J. Geophys. Res., 111, doi:10.1029/2005JD006543, 2006) has not yet applied. The effect of the PSC presence on retrieved values is most sensitive to H_2O data with a non-gaseous contribution correction scheme (Yokota et al., Improved Limb Atmospheric Spectrometer (ILAS) data retrieval algorithm for Version 5.20 gas profile products, J. Geophys. Res., 107, 8216, doi:10.1029/2001JD000628, 2002) used in both V1.4 and V2. A preliminary result suggests that there is a negative bias of up to 1-2 ppmv in the V2 data. Further, the MIPAS data itself has a positive bias of 5-10 % compared to HALOE data. Considering such backgrounds, it is difficult to decide which version is better. A further comprehensive validation analysis is needed in future, although it is beyond the scope of this paper. These discussions are included in the revised paper.

Basically, a new tangent height registration was developed to avoid using a distorted solar disk image (the lower edge of the sun) due to a presence of PSCs (so that about 200 occultation scenes were rejected in V1.4), and also avoid a combination of two altitude registration methods (Tanaka et al., Tangent height registration method for the Version 1.4 data retrieval algorithm of the solar occultation sensor ILAS-II, Appl. Opt., in press). This is also mentioned in Appendix in the revised paper. The effect of this change on retrieved values was not significant in SH data. The line parameter database change from HITRAN 2000 to 2004 also did not affect to retrieved values, as mentioned in the HNO_3 subsection. The change in the HNO_3 line parameters was the largest.

For both NH and SH comparisons, the first paragraph describes differences between V1.4 and V2, and then discussion on differences between ILAS-II and MIPAS data for each species are presented in the revised paper.

3. *I found the style of the text somewhat laconic. The paper also lacks precision and I found that the introduction as well as the conclusions are weak. Why do the authors not advice the user about the ILAS versions? Which ILAS version do you recommend? Where can the data be downloaded? These are other questions that the users are asking and to which the paper should answer.*

   ANSWER: The style of the text now became not to be laconic. Introduction and Conclusions are now significantly re-written. In Conclusions the recommendation of the ILAS-II version has been included as well as the source for downloading the data.

Detailed comments

1. *Lack of precisions:*

   - *Many times, the reader has to deduce information from the text. For example, the labels '2', '2.0' and '2.1' are used to point out the ILAS version 2 (as written in the title). After several readings of the paper, I deduced that the v2.0 is the one used for the SH retrieval, i.e. a version based on HITRAN 2004 and the new tangent height registration. The version 2.1 is the version 2.0 plus the new correction of the transmittance. Am I right? The versions 2.0 and 2.1 must be clearly presented.*

     ANSWER: The differences between satellite sunrise and sunset mode measurements are now mentioned in Appendix in the revised paper. Both

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sunrise and sunset measurements used the same line parameter database (HITRAN 2004) and a refined altitude registration. Only for sunrise measurements, a transmittance correction was applied since a signal distortion (Nakajima et al., 2006) was found significantly in their atmospheric measurement part. Only for sunset measurement, an updated trend correction for 100 % level part was applied, since the signal distortion seemed to appear in their 100 % level part.

- *I understood the paper to be an intercomparison of two ILAS-II retrievals, MIPAS being used for validation. However, in the abstract and further in the text, this turned out like being a comparison between MIPAS and ILAS (e.g. P9320-L2, P9321-L27). Again, an increase of precision would be welcome.*

  ANSWER: We used the MIPAS data as a validation source. Expressions like "comparison between MIPAS and ILAS-II" were deleted. We have unified them as expressions like "comparison between ILAS-II and MIPAS" throughout the paper.

- *Another example of this type is found on P9325 (L11-12) where the LTE assumption is mentioned. Do the authors mean that one of the retrievals, ILAS- MIPAS, is based on this assumption, the other on non-LTE assumption?*

  ANSWER: This sentence has been deleted as it only confused the readers.

- *The differences between versions 1.4 and 2 (should I say 2.0 and 2.1) are too quickly presented. Why do not SH data need to be improved regarding the correction of the transmittance? For which reason does this problem arise only for the NH data? How was this correction improved? This could interest the data retrieval community. About the registration height, what kind of improvement have you made? On P9323 (L5), it is mentioned that 'there are many differences between v1.4 and v2'. Is there any paper or report which describes the v2? If no, an appendix describing these differences*

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would be appropriate.

ANSWER: The ILAS-II instrument used a sun-edge sensor in which a reflection light by the entrance slits was detected to make an accurate altitude determination (Tanaka et al., in press), but a plastic deformation of the entrance slits occurred in orbit due to solar heat energy, so that a light coming to the secondary (spectrometers) slits changes depending on degree of changes in the plastic deformation. Because of a different measurement sequence between sunrise and sunset mode measurements, the transmittance correction was only applied for the sunrise measurements (NH). For the sunset measurements (SH), the trend correction for 100 % level part was newly applied, but the impact of this revision on retrieved values was not so significant; but for H_2O and CH_4 data, the values in V2 became smaller than those in V1.4. Thus, we do not think this could interest the data retrieval community, since it is just related to the ILAS-II hardware issue. But we believe that altitude registration using the sun-edge sensor was a robust way, if no plastic deformation occurred owing to solar heat energy.

For a new altitude registration applied in V2, it does not use both of the transmittance method in which a molecular oxygen A-band absorption was used and the sun-edge sensor method (Tanaka et al., in press). It uses an angle information from a gimbal mirror. The gimbal mirror is arranged to guide an incident sun light to the spectrometers and the angle of the gimbal mirror is recorded during measurements of ILAS-II. By using the position of the satellite, the earth, and the gimbal mirror angle, we could calculate the optical path of IFOV center, then the TH was derived. This is also given in Appendix in the revised paper.

Since no paper that deals with the retrieval algorithm for V2 has been published, the descriptions mainly on differences between V1.4 and V2 algorithms are now presented in Appendix in the revised paper.

- *As mentioned in sec. 2.2, MIPAS data are retrieved by IMK and IAA. This*

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important information should be also given in the abstract in order to make quickly the difference with the ESA retrieval. About the same point, why not choose MIPAS ESA? Is the MIPAS retrieval based on the HITRAN 2004? If no, what kind of differences between MIPAS and ILAS version 2 are to be expected?

ANSWER: We now mention in the abstract that MIPAS-IMK data have been used for intercomparison. The reasons why these have been chosen instead of the ESA data are:

- ESA data do not cover all species considered in our intercomparison; particularly $ClONO_2$ is missing in the ESA data set;
- While during the course of our study a part of the IMK data already had been validated, with validation published in peer-reviewed journals; this was not the case for the ESA data.
- The IMK data often cover a wider altitude range, often are less oscillatory, and use a more rigorous cloud rejection threshold. Furthermore, more detailed diagnostic data are available.

The database used for IMK MIPAS retrievals is largely identical to HITRAN 2004, and no spectroscopy-related differences are expected unless explicitly mentioned in the paper. Differences should be consistent with the standard error of the mean difference plus any error contribution caused by systematic coincidence errors, plus any residual systematic errors due to spectroscopic data in cases when the retrievals use different spectral bands.

- *Differences between ILAS and MIPAS are given following von Clarmann (2006). Can you summarize this method? Why did you use this method instead of the classic one?*

ANSWER: The reference was misplaced, and the method chosen to calculate the differences is not in conflict with the classical one. The reference was meant to refer to the way the percentage differences were calculated.

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The related text has been modified accordingly.

- *The results section is mostly a description of what is shown on the plots. Most of the time, the differences between ILAS and MIPAS are not explained. When they are, this is often confusing. For example, in sec. 3.1.1, why v1.4 is wrong above 20 km? In the same section, why ILAS v2 is lower than MIPAS above 45 km (I would say 40 km)? In sec. 3.1.2., the authors find a good agreement between ILAS v2 and MIPAS for methane. I found that ILAS and MIPAS show different vertical structures on the averaged profiles around 20-25 km. Why? In sec. 3.1.6 you mention that the difference between the two ILAS versions can be due to the diurnal cycle of ClONO2. I do not understand this comment and further explanations are necessary.*

  ANSWER: We have rearranged the Results section in order to avoid a redundant description of what is shown on the plots. Now the Results section is changed to: Coincidence criteria, Monthly averaged profiles, and Hemispheric averages and differences between ILAS-II and MIPAS. Then, differences between the two ILAS-II versions and between ILAS-II and MIPAS are discussed in detail in the Discussion section.

  The signal distortion seems to impact depending on the species and altitude, although the cause of this is unknown. The H_2O value is affected at the most, so that it was wrong above 20 km in V1.4. Although the transmittance correction was applied in V2, still it might be not enough for H_2O data for altitudes above 40 km. Therefore, we do not recommend using the NH H_2O data above around 40 km even for V2.

  ILAS-II has a difficulty (apparent some high bias) for retrieving CH_4 profiles at altitudes around 20-25 km (Ejiri et al., 2006), which is still an open question in the ILAS-II retrieval and has to be investigated. Therefore, different profiles between the two might be due to an inferiority of ILAS-II.

  In sec. 3.1.6 it was not supposed to be understood that the difference between the two ILAS-II versions can be due to the diurnal cycle of $ClONO_2$.

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The difference between the two ILAS-II versions in NH might be also due to the inclusion of the transmittance correction in V2.

2. *The text is laconic, especially the results section and the conclusions:*

   - *Can you drop the term 'Envisat' when you mention MIPAS? I think there is no possible confusion with the other MIPAS instruments.*

     ANSWER: The term 'Envisat' has been dropped in the new version of the paper.

   - *Each section 3.1.x has an identical introduction which gives the MIPAS versions and the related reference. Why not give this information in a Table? Also, it is not necessary to remind the number of coincidence in the text if it does not help the authors to explain the results. The readers interested by this information could find it in Tables 1 and 2.*

     ANSWER: The MIPAS versions are given in the new Table 1. The number of coincidences has been omitted from the text.

   - *The conclusions almost repeat what is written in the results section. The authors should try to be more concise.*

     ANSWER: The Conclusion section has been re-written.

3. *The methodology is approximate:*

   - *The differences between v1.4 and v2 are derived from the same kind of plots: (1) monthly average of profiles of each hemisphere, (2) global average over the period from May until October and (3) differences between ILAS and MIPAS based on this global average. The kind of plot chosen by the authors would be sufficient if the data were collected for the same physical conditions. This is not always the case, especially for the South Pole data. For example, the ozone evolves from values outside ozone hole conditions (May) to ozone hole values (October). Furthermore, HNO3 is perturbed*

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in July by the production of mesospheric NOx transported downward in the upper stratosphere. Do the differences between MIPAS and ILAS remain the same when the conditions change? A plot of time series would help to show it. Another way to evaluate the ILAS versions could be done using tracer-tracer correlation plots. For example, is the CH4-N2O correlation compact? Is there an improvement from v1.4 to v2? How does it compare to MIPAS?

ANSWER: We have separated the ozone data into outside ozone hole conditions (May to September) and inside ozone hole conditions (October); in the case of HNO_3 we have calculated the differences without the July data which were disturbed by a high-altitude enhancement of NO_x. While certainly interesting, we have decided not to compare N_2O-CH_4 correlations in this paper. The reasons are as follows:

- The ratios N_2O/CH_4 are particularly sensitive to error correlations between these species, which are retrieved in the same spectral interval and considerably interfere with each other's signal. These correlations are not regularly stored and thus cannot be recovered for the data used in the intercomparison.
- N_2O/CH_4 correlations are very sensitive to different altitude resolutions of the two species (c.f. von Clarmann and Grabowski, 2006, Elimination of hidden a priori information from remotely sensed profile data, ACP 7 397-408, 2007). The information needed to remove related artefacts in the profiles cannot be restored.

For these reasons we think that inclusion of these correlations in a paper aiming at validation of data raises more questions than it answers and we leave this certainly interesting issue for a dedicated scientific study.

- *In the SH, version 1.4 provides a better agreement against MIPAS than version 2. Why? If I correctly deduced the differences between v1.4 and v2.0 (see above) I would conclude that one of the changes (or both) degrades*

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the results, either the new spectral parameters or the new tangent height registration, or both. This must be discussed. If necessary, a new version of ILAS should be built and based on that one, add the new transmittance correction that was implemented in version 2.1.

ANSWER: A better agreement between ILAS-II V1.4 and MIPAS has happened to seen in SH than between V2 and MIPAS, but as already mentioned above, a more comprehensive work should be needed to conclude it. For example, Ejiri et al. (2006) reported that the V1.4 CH_4 data are somewhat (0.1–0.2 ppmv) larger than HALOE. Thus, V2 CH_4 gets a better agreement with HALOE than V1.4. Further, MIPAS CH_4 mixing ratios may be biased high by $\approx$ 10 % below 40 km (personal communication, Markus Engelhardt, 2007).

Yes, we agree with a necessity of a new algorithm, especially for a more refined trend correction for 100 % level part. Such a discussion is now given in the revised paper.

- *The agreement between ILAS and MIPAS derived by the plots of differences are not explained. Why these differences? Are they significant with respect to the instrumental errors and the intercomparison method? Since MIPAS species are already validated, is the agreement between ILAS and MIPAS of the same order than the agreement between MIPAS and other correlative data?*

  ANSWER: The biases are significant if they exceed two times their standard errors of the mean differences, as stated above. Additionally it is now mentioned, if they were of the same order than the agreement between MIPAS and other correlative data.

- *The chosen coincidence criterion is discussed very quickly. Can you develop on this? Above 25-30 km, the volume mixing ratio of ClONO2 presents a diurnal cycle. Is the intercomparison method still valid? Could other intercom-*

parison methods help, using a photochemical box model or data assimilation for example?

ANSWER: The coincidence criteria were approximately those used for MIPAS validation studies or comparisons with other measurements. Additionally we had a look for different coincidence criteria before we chose the criteria used in the paper and decided to use the criteria finally chosen, because with larger distance in space and time the atmospheric conditions changed too much and the differences got worse. The diurnal cycle of $ClONO_2$ was already shown in the context of MIPAS intercomparison with the ACE-FTS occultation instrument by application of a chemical transport model. However, in that paper application of the CTM lead to an overcorrection of the diurnal effect, such that a definite conclusion about the agreement between the two instruments in that altitude range remained open since it is not clear whether the remaining difference is due to CTM-model errors or the instruments. Thus, we have decided not to use such a correction method for the this paper.

4. *In the introduction, the authors remind one the role of O3, H2O, CH4, N2O, HNO3 and ClONO2 in the global warming, the greenhouse effect and the ozone depletion. I find this is written in a very naive way. I suggest to the authors to be more pragmatic and address the paper on the validation issue. The questions that the introduction could address are: why to improve the ILAS retrieval algorithm? which study can benefit from these improvements? About the conclusion, I think the authors fail to answer the question they are asking (and which interests the user community): which version of ILAS do you recommend to use? I think the paper should be presented in order to answer this question.*

   ANSWER: Introduction has been re-written to emphasize why an improvement of V1.4 was necessary. In Conclusions, the recommendation of ILAS-II V2 is added.

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