

Interactive comment on “Validation of NO₂ and NO from the Atmospheric Chemistry Experiment (ACE)” by T. Kerzenmacher et al.

Anonymous Referee #1

Received and published: 6 April 2008

The article is on the validation of NO₂ and NO profile measurements by the MAESTRO and FTS instruments on the ACE satellite. It is well structured, very well written and also the figures are very good. It would really be overall a very good article, if there were not the issues regarding the measurement error and the agreement with the validation instruments (see comments below). Therefore I can recommend the paper to be published only after these issues have been corrected. Also, I think the following points should be taken into account.

General Comments

1) The article is very long (in my opinion too long), mainly due to the huge amount of comparisons. Nevertheless, the reader is at the end uncertain about the quality of the

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper



ACE data. This is only partly due to the fact that the different validation instruments show different agreements, but in my opinion it is also related to the points mentioned below. Also, I think it would be very helpful to give a proper motivation for the individual comparison studies. You might consider if really all comparisons are necessary, and - if yes - say why, or - if not - skip some of the comparisons.

2) I think the two terms "measurement error" and "measurement variability" should be well defined and also clearly differentiated throughout the text. In many discussions of the agreement with the validation measurements it appears that these two terms are confused (see specific points). Regarding the measurement error, the reader is referred to the papers of Kar et al. (2007) and Boone et al. (2005). I recommend to have a definition and discussion of the error also in this validation paper. I am not absolutely sure (see specific points) but I think in the plots always the standard deviation of all observations is shown (although it is then called fitting error or error bars). Also, I think this standard deviation is not a very useful quantity, because in most cases the considered profiles were measured at different latitudes and seasons.

3) There is very little said about the latitudinal variability of the measured profiles. I strongly suggest giving this point more weight, since it is important for assessing the quality of the profiles. For the user of the data it may be of interest how good the agreement is for a certain latitude and season. Probably, the agreement with the validations is very different for e.g. high latitudes and the tropics. If this was studied by the authors then it should be mentioned in the text. I would prefer to group the comparisons in latitude regions, but this will probably too much work for the resubmission. Also, in the conclusions it may be good to add a table with the latitudes sampled for the respective comparisons.

4) The agreement of the ACE measurements to the validation measurements, given in % in the text, is often not correct, or at least it does not agree with the figures shown. The most striking discrepancies already appear in the abstract (see below), but the authors should take care that all statements given on agreement with the validation

[Full Screen / Esc](#)[Printer-friendly Version](#)[Interactive Discussion](#)[Discussion Paper](#)

measurements actually agree with the figures. The reader would feel annoyed if he finds these disagreements (and this is very easy because of the good quality of the figures).

5) I think there is not enough said regarding the differences (improvements ?) to previous retrieval versions. Also the agreement with previous validation papers should be discussed more.

6) Regarding the validation of the NO₂ profiles I think an agreement of 20% near the peak should not be called "very good" or "excellent". Basically all validation studies for other instruments cited in the article find better agreements.

7) In general and also in a few specific passages I find the text unnecessarily long. On the other hand I appreciate that many details, in particular on the validation instruments, are given. However there are also a few cases where information is repeated or where whole passages seem unnecessary (see below). I think the article would really gain by skipping some of these passages.

Specific Comments:

Abstract:

"The ACE-FTS NO VMRs agree with the Satellite data sets to within about 20% between 25 and 40 km."

In Fig. 38, the MIPAS and the SAGE II sunrise observations differ by more than 20% in the considered altitude range.

"In comparisons with HALOE, ACE-FTS NO VMRs typically agree to $\pm 8\%$ from 22 to 64 km and to $\pm 10\%$ from 93 to 105 km."

For both altitude ranges, the difference to HALOE is larger than stated (see Figs. 35 and 36). This is not changed simply by adding the word "typically". Also, for 93 to 105 km, the typical difference would be around 30% (see Fig. 36).

Introduction

P3031, L14 "Secondary occultation" - better: "... occultation (which is its secondary measurement mode)...";

P3031, L20 "From nadir-viewing" - add: "total columns" (Also, for O3 and NO2 also limited profile information can be obtained from nadir measurements).

P3031, L24 " in limb mode, which is its primary measurement" : Most of SCIAMACHY measurements are performed in nadir mode.

Chapter 2 : ACE

P3032, L23 "coverage of the tropics,... " - I recommend including a map with the frequency of the observations (how many % of the observations are performed for which areas/latitudes).

P3033, L2 The passage after "To date ..." might be skipped

P3033, L17 Are really only O3 and NO2 cross-sections for only one temperature used ?

P3033, L22 I suggest adding a typical reference for DOAS like e.g. Platt (1994) or Platt and Stutz (2008).

P3033, L22 The passage from "The NO2 cross section ..." until "... other absorbers." (P3034, L7) can be skipped.

P3034, L12 and Fig. 1 "...simple summary statistics" this term is more confusing than explaining. I suggest simply: "summary statistics" and - very important - to explain the plotted data in detail. Comparing with Fig. 6, I assume the plotted value is σ/\sqrt{N} , with N being the number of occultation measurements and σ the standard deviation of the vmr at the respective altitude. This would then also be in accord with the term "summary statistics". If this is correct, then the formula for calculating this value should be given, and in the text and in the legends of the plots it should be called

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper



statistical error. Otherwise the reader gets the idea that the plot shows the error of the retrieval. Also in this case, the plots show the same value that is later called uncertainty of the mean (Fig. 6). If it is really a fitting error - meaning a value that is determined from the retrieval process and not from the results - then the formula for its calculation should be given. It can not be the estimated uncertainty by Kar et al. (2007), because this would be 5% for altitudes between 20 and 40 km, while the relative error shown in the article is (much) less than 1%. Moreover, if the plot shows the retrieval error (derived from an approach that should be described in the text), then it is strongly underestimated (by at least one order of magnitude) when comparing to the validation results presented in the following. Therefore, I have doubts if these plots are really of any importance to the reader. They certainly give a wrong impression on the quality of the data (at 20 km the presented absolute fitting error is less than 0.01 ppbv). If you like to keep these figures in the article you should 1) describe how you calculated the plotted data, 2) avoid the term fitting error if it is a statistical error and 3) explain what the plots say about the quality of the ACE data.

P3034, L18 Since the temperature effects in the O₃ cross-section are also not accounted for, this should also result in an uncertainty ?

P3034, L26 "... appears to be in the range 1 to 2 km ..." - Please state from where you conclude this.

Figs. 2 and 3 : see comment to Fig. 1 above

Chapter 3 : Validation Instruments

P3037, L5 "...ten satellite products from eight instruments." I suggest adding "for comparison". Also, I think you forgot OMI and GOME which both are also measuring NO₂ columns.

Chapter 4 : Validation Approach

P3047, L23 "... determined on a species to species basis ..." - what do you want to say

[Full Screen / Esc](#)[Printer-friendly Version](#)[Interactive Discussion](#)[Discussion Paper](#)

with this ?

P3047, L26 Why is it checked if the ground-based and balloon measurements are coincident with each other ? Are there comparisons of these measurements in the article ?

P3050, L19 "The ACE instruments only produce fitting errors" - Regarding the term fitting error : You are using it here and above in the sense of statistical error. I find this very misleading. Better write statistical error and say how it is calculated. It may be that in certain branches the word fitting error has the meaning of statistical error. From the meaning of the word itself however, one thinks it is the (total) error of the retrieval. If you want to use the word fitting error (which would be misleading) I think it is necessary to explain what you want the reader to understand with it.

Section 4.2

I suggest to restructure this section. You start with the term diurnal effect, then turn to the local time issue and the scaling factors, and then explain again the diurnal effect. This confuses the reader because one might think that are three effects to be considered. Also it makes the text unnecessarily long because many things are repeated.

Chapter 5 : Results for the NO₂ comparison

P3053, L17 "They agree to within 10% ..." - The differences agree ? This would mean that the differences (FTS-Maestro) agree to the differences found in Kar et al. Probably you want to say that the FTS and Maestro measurements agree ?

P3053, L19 "... reaching values of 50% at 45 km." - The mean value of Maestro for 45 km, shown in Fig. 6 a, is not 50% lower than FTS. Also, in this plot for higher altitudes, FTS is below Maestro and not vice versa.

P3055, L12-18 You write that ACE-FTS and ACE-Maestro both show differences to HALOE within $\pm 15\%$ from 22 to 42 km. This is quite surprising because in section 5.1.1 you find that FTS is around 10% lower than Maestro. If this is due to the fact

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper



that HALOE observations are performed for northern high latitudes mainly, I think this should be pointed out here.

P3056, L8 "... 25 to 40 km." - add the reference to Fig 8a of Kar et al. already here.

P3056, L12 "... below 25 km." - add the reference to Fig 14 already here (not one line later which refers to Fig. 9a of Kar et al.)

P3056, L22 Please say why there are fewer coincidences for FTS than for Maestro.

P3058, L23 and Fig. 17 I suggest to add ticks at the upper axis and the 0% line. Also the yellow shading for the 20% region would be nice.

P3060, L16 Why is this polar vortex criterion only applied for the OSIRIS comparisons ?

P3060, L21 and Fig . 19 Why is the altitude grid of FTS and OSIRIS 2 km for this comparison ?

P3061, L11 Again, FTS and Maestro show a similar (about 1ppbv) difference to OSIRIS at the peak, although FTS was found to be lower than Maestro in section 5.1.1. Please explain why this is so.

P3062, L12 Please explain why the total columns are expected to be larger than the partial columns, i.e. mention tropospheric part (above or here).

P3062, L20 How do you conclude that there are no seasonal or latitudinal biases ?

P3064, L12 Is there an explanation why FTS and Maestro do not observe this gradient above 24 km ?

P3064, L15 In Fig. 23c the difference of FTS to SAOZ is larger than 5% at 24.5 km.

P3066, L25 Here it would be nice to discuss the differences with respect to the location of the stations, i.e. why is the correlation worst for Kiruna ? Also, the possible effect of clouds might be discussed here.

[Full Screen / Esc](#)[Printer-friendly Version](#)[Interactive Discussion](#)[Discussion Paper](#)

P3067, L10 Did you check if the differences for the high-latitude stations can be explained by gradients due to denoxification ?

P3068, L9 Since the plotted profile is the mean profile of observations in May and September (or March and July), the variability of the profiles is (mainly) determined by seasonal effects. Therefore I do not think it makes sense to argue that the observed differences are within this variability. That is similar to: The mean value of all profiles measured by FTS in March and July differs to those measured from GB by 25%, but it agrees with the profiles measured in March.

P3068, L29 Again, please point out that the errors shown in Figs. 1-3 are statistical errors and no systematic error is available.

P3069, L3 In how far are these comparison results consistent with each other ?

Chapter 6 : Results for the NO and NO_x comparisons

P3071, L3 The considerable increase of the difference starts already below the peak at 45 km.

P3071, L9 The difference is (much) larger than 20% also for altitudes above 50 km.

P3072, L21-24 I think this argumentation is not possible. If the precision of both instruments is larger than the atmospheric variability, they can also be similar.

Chapter 7 : Summary and conclusions

P3075, L22 Since the differences to MIPAS are up to 70 % or larger than 1.5 ppbv, I think "slightly more negative" is not the correct term.

P3075, L22-24 Yes ! Since this is the correct description of the agreement, it should be given also in the abstract !

P3075, L26 - P3076, L11 The conclusion is not the right place for this. The reader would need this information, when the profiles are compared (section 5).

[Full Screen / Esc](#)[Printer-friendly Version](#)[Interactive Discussion](#)[Discussion Paper](#)

P3076, L13 For altitudes close to the NO₂ peak a difference of 20% or 1ppbv can not be called "very good".

P3078, L22 Again, for altitudes around the NO₂ peak, a bias of 10% is not "slightly".

P3079, L22 Again: the difference to MIPAS and SAGE data is larger.

P3079, L23 See comment to abstract.

Technical corrections

P3054, L24 : comparisons

P3066, L19 : the agreement

P3068, L22,26 : should be 10e15 molec/cm²

Table 2 SAOZ (balloon), coincidence criteria : there is one km too much, or one value is missing.

Fig. 7 : In the equation for the linear fit you probably mean ACE-FTS on the left side.

Fig. 21 : Also here, it should be ACE-FTS in the equation.

Interactive comment on Atmos. Chem. Phys. Discuss., 8, 3027, 2008.

[Full Screen / Esc](#)[Printer-friendly Version](#)[Interactive Discussion](#)[Discussion Paper](#)