

Interactive comment on “Validation of ACE-FTS N₂O measurements” by K. Strong et al.

K. Strong et al.

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We thank the reviewer for his or her helpful comments. In the following we present the original comments in italics and our responses in plain text.

General comments:

This paper gives a detailed validation of the N₂O measurements from ACE-FTS, a new instrument that was launched onboard the ACE satellite on 12 August 2003. The scope of the paper is therefore of great interest for further use of these satellite data. The authors compare ACE-FTS v2.2 measurements with products from many different platforms (satellite, aircraft, balloon-borne and ground-based stations), in a very well homogenized and comprehensive way. They give a clear overall picture of the data quality of ACE-FTS N₂O measurements. Therefore, I recommend the publication of this paper in ACP.

Specific comments:

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1) Abstract:

Since the authors give the altitude ranges of the profile comparisons with satellite, they could also give the mean values of the partial column limits used in the FTIR comparisons.

These mean values for the partial column limits have been added to the Abstract.

2) Introduction:

Very nice and complete introduction.

3) Section 2:

a) The authors give the precision of ACE-FTS N₂O profiles obtained from the fitting errors but they do not exploit it in the discussion of the comparisons results. It could have been used to calculate a combined random error budget which could have been compared to the obtained standard deviations from the mean (panels c) on the Figs. 1 to 7). Same remark for the systematic error budget and discussions on the biases. Is it because a final complete error budget is not yet available? If it is, maybe this should be said explicitly in the text.

This is correct - unfortunately, a final complete error budget for the ACE-FTS retrievals is not yet available. The only errors available are the statistical fitting errors as described in Section 2. A statement to this effect has been added to the text.

b) Reference is made to Sung et al., 2007 for previous comparisons of ACE-FTS, but nothing is said on their results.

A sentence summarizing the results of Sung et al. has been added to the last paragraph of Section 6, in the context of the discussion of the ACE-FTS vs. FTIR comparisons.

4) Sections 3, 4 and 5:

a) Coincidence criteria : it is not an easy task to find common coincidence criteria for different type of instruments, and this has been done in a reasonable way in the paper. Still, I have 2 questions:

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-MIPAS comparisons concern less months of data, and they apply a tighter coincidence criteria than other satellite measurements. I would expect the opposite, if the number of coincidences is an issue. The impact on the number of coincidence is quite large as seen in Figs. 1 and 3 compared to Fig. 5 and 6. What is the reason for this tighter criteria: do they obtain a worst agreement by taking a more relaxed criteria?

The MIPAS comparisons were limited in time to winter/spring 2004 (21 February to 25/26 March), between the start of ACE science operations and the end of MIPAS full-resolution measurements. This also limited the coincidences to the northern mid- and high latitudes. Tighter criterion were chosen to reduce the likelihood of sampling significantly different air masses during polar spring, as N₂O profiles inside the polar vortex will be subject to subsidence.

-I would also expect a common criteria for MIPAS ESA and MIPAS IMK-IAA data, since the comparisons concern the same measurements, even if the algorithms are different. It is written p.3618 (lines 1-5) that, in case of MIPAS daytime measurements, the time-difference with ACE-FTS measurements are about -6h to -8h. Thus I guess that in the MIPAS ESA comparisons, a lot of coincidences are missed by taking a coincidence criteria of +/-6h (instead of 9h for MIPAS IMK-IAA). And it seems reasonable in case of N2O not to choose such a tight criteria. Could the authors justify such a difference in the criteria? What is the impact on MIPAS ESA comparisons of taking +/-9 h?

The two MIPAS groups worked independently and chose the tighter criteria as they deemed appropriate for the period of overlap. For the MIPAS IMK-IAA comparisons, a maximum PV difference was also applied as a coincidence criterion, reducing the possibility that ACE and MIPAS were sampling different vortex conditions and making it possible to relax the temporal and spatial criteria compared to the MIPAS ESA comparisons. We do not have MIPAS ESA comparisons available for the ± 9 hour window, and the person who performed these comparisons is no longer working in this area.

b) Error budgets:

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-Paragraph d) p. 3609 and panel d) in Figs. 1 to 7: The figures 1 to 7 are very nicely homogenized and clear. But the panels d) only show the variability seen by each instrument, so a mixing between real variability and random errors. Would not it be more interesting to show a combined random error budget in order to compare with the relative standard deviation of panel c) and make some conclusions on the reached precision? Also adding the systematic errors on this plot would help to discuss the obtained biases. These errors are available since you give them in the text (except maybe for ACE-FTS?, see my remark 3.a))

We chose to present only the relative standard deviations in panel (d) of Figures 1 through 7 due to the difficulty of obtaining accurate and consistent random (and systematic) error budgets for all of the data sets compared. Instead, we have provided some discussion of the estimated errors for each of the comparison data sets in the text. In addition, as noted above, only statistical fitting errors are available for ACE-FTS profiles; no systematic error budget exists for the v2.2 data product. However, work is underway to produce an error budget for the next version to be released (v3.0).

-p. 3616, l.17: the authors explain in a paragraph where the MIPAS ESA random error budget come from, but they do not give some values, and they do not use this part anywhere in the paper.

The values were inadvertently omitted from the paper. This has been corrected, and the following sentence has been added: "For the MIPAS ESA profiles used in this work, the random error is less than 20% between 15 and 28 km, increasing to 75% at 6 km and to more than 80% above 36 km."

-Spectroscopy: In some of the comparisons, the spectroscopic database is given, in other ones it is not given. For example: MIPAS IMK-IAA retrievals use HITRAN 2004; what about MIPAS ESA products? In case of MIPAS IMK-IAA (p. 3617), the systematic errors are given. Do they include spectroscopic errors? Since they use the same spectroscopic parameters than ACE-FTS, the effect of the spectroscopic errors on the

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comparisons should be reduced for a large part.

MIPAS ESA uses the MIPAS dedicated spectroscopic database that was built for the mission. This has been noted in the text and a reference has been provided.

The given MIPAS IMK-IAA systematic errors excluded the spectroscopic contribution, as both MIPAS IMK-IAA and ACE-FTS retrievals use HITRAN 2004. However, while both MIPAS IMK-IAA and ACE-FTS retrieve N₂O using spectral microwindows near 1200 cm⁻¹, ACE-FTS also uses a series of microwindows between 1860 and 2600 cm⁻¹. It is possible that those bands will have different spectroscopic errors, which will not cancel totally even when using the same version of HITRAN. Therefore the systematic errors given in the text have been increased to include the spectroscopic contribution, and the above clarification has been added.

c) Reference -p. 3616: Vigouroux et al. 2007 should be replaced by an appropriate one (Fischer et al., ACPD, 2007 or an older reference).

This reference has been changed to Fischer et al., ACP, 2008.

5) Section 6:

a) References: Since many references are given in Table 2 on the retrievals techniques of N₂O profiles at the different stations, I do not see the point of adding the 2 references Barbe and Marche (1985) and Sussmann and Schaffer (1997) in the text: they deal with stations and retrievals strategies that are not used in the paper, and the FTIR retrieval techniques are explained in a more complete way in the references given in Table 2.

These references were added after the technical review of the manuscript, at the request of one of the reviewers, as “two early publications showing first investigations on how to derive information on the vertical N₂O distribution from ground-based infrared spectra”.

b) Spectroscopy and biases:

p. 3624, l. 27: It is nice to have the information on the bias due to the use of HITRAN

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2000 compared to HITRAN 2004 (1.3%). Maybe it would be interesting to stress that the results of FTIR are even better if one considers this bias: -5.3% for Kiruna, instead of the 6.6% value, which is given also in the abstract and the conclusion. Remark: I guess from the text that the official updates of HITRAN 2000 do not concern the N₂O lines?

Yes, HITRAN 2000 + official updates is equivalent to HITRAN 2000 for N₂O. This has now been noted explicitly in the text.

Yes, better comparisons result if the Harestua, Kiruna, and Izaña mean differences are adjusted by the 1.3% high bias in the N₂O columns (and hence low bias in the ACE-FTS – FTIR differences). The adjusted numbers have been explained and added to Table 3 in brackets, and it is these numbers that are now referenced in the summary discussions in the Abstract and Conclusions for consistency.

c) Coincidence criteria:

p. 3625, l.12: Since the authors have chosen to take only the closest ACE profile in coincidence with FTIR measurements (which is not the case of satellite comparisons), it could be worth to give the mean value of time and spatial difference of both measurements (as has been done in Sect. 4.3.2 for MIPAS IMK-IAA comparisons). It will give a better idea of what are finally the mean applied criteria.

We have calculated the mean (and standard deviation) for the distance and time differences for the ACE occultations used at each FTIR station, and the overall means for all stations. These values have been added to Table 3, with a note in the text.

d) Partial columns calculation: p. 3625: why the pressure and temperature from ACEFTS were not used instead of the FTIR ones in the partial columns calculation of ACEFTS?

We chose to use the same pressure and temperature profiles for the calculation of densities, and hence partial columns, of ACE-FTS and the corresponding FTIR mea-

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surements, in order to minimize possible sources of difference. This way, only the ACE-FTS VMR profiles were assessed in the comparisons, rather than a combination of ACE VMR, temperature, and pressure information. In order to keep the FTIR measurements independent of ACE, we used the FTIR pressure and temperature profiles, rather than those from ACE.

e) *Errors:*

-I have the same remark than for satellite discussion: the standard deviation in Table 2 should be compared to the combined random errors from both instruments.

As with the satellite comparisons, we do not have consistently defined random error budgets for all of the FTIR comparisons. Instead, we have added a sentence comparing the standard deviations to the combination of the ACE-FTS statistical fitting errors over the partial column altitude range and the typical FTIR partial column error (revised to 3%, see next comment).

The authors give 10% for FTIR errors: it is sensibly higher than the error budget given in Vigouroux et al. 2007. Do you have an explanation? For seven of the twelve stations (or of the nine stations if we remove the high-latitude stations that have problems of viewing different airmasses during the polar vortex season), this error seems conservative, if one considers the standard deviations of the comparisons.

We revisited the partial column errors for the mean altitude range for the FTIR N₂O partial columns (14–27 km) using Toronto data and found that the retrieval noise error was 1%, the state space interference error was <0.25%, the temperature error was 3%, and the smoothing error was 3.5%. Combining these in quadrature yields a total error of approximately 3% without, and approximately 5% with, smoothing error, not 10% as stated in the manuscript. This has been corrected in the text. Since the ACE profiles are smoothed by the FTIR averaging kernels, the smoothing error should not be included in the total error budget. We also checked that these values were consistent with available errors provided for other FTIR sites. For example, Jungfrauoch N₂O

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partial columns (18–28 km) have a total error of 4.07%, of which 3.83% is smoothing error, and Wollongong total errors are 4–10%.

6) Conclusions

Very nice summary, particularly in Fig.12 and Table 4. A combined random error budget could also be added (see remarks above).

Please see replies above.

Technical corrections:

-List of authors: I guess it is “Ridolfi”;, not “Ridolfii”.

-p. 3623, l. 3: “on the measured N2O”, instead of “on the the measured N2O”

-p. 3624, l. 26: “HITRAN 2004”, instead of “HITRAN2004”;

These three corrections have been made.

Interactive comment on Atmos. Chem. Phys. Discuss., 8, 3597, 2008.

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