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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Validation of ACE-FTS N_2O measurements" *by* K. Strong et al.

Anonymous Referee #1

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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:

1) Abstract:

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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.

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.

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

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:

- 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?

- I would also expect a common criteria for MIPAS ESA and MIPAS IMK-IAA data, since

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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?

b) Error budgets:

- 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))

- 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.

- 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 comparisons should be reduced for a large part.

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

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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.

b) Spectroscopy and biases:

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

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.

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

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.

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- 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.

6) Conclusions

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

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