Atmos. Chem. Phys. Discuss., 8, S1288–S1293, 2008
www.atmos-chem-phys-discuss.net/8/S1288/2008/

Atmospheric Chemistry and Physics Discussions

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

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Received and published: 4 April 2008

General comments

This paper deals with the validation of a new atmospheric chemistry satellite mission (ACE) for an important atmospheric species (N2O). This theme is of high significance for the atmospheric chemistry and physics science community and fits well into the ACP journal scope. The adopted validation concepts are of excellent scientific quality, and the study is based upon a full variety of high quality correlative data sets. The paper is presented well and should definitely be published.

On going carefully through this paper, I encountered a few minor issues which are listed as <Specific comments> thereafter. While consideration of these issues might help to further improve science quality and readability of the paper, I would like to render the decision on whether and how to deal with which of these minor points to the responsibility of the authors, because of the already high quality of the discussion paper at hand.

Specific comments

<Abstract>

>The abstract summarizes validation results/numbers mainly in terms of <mean absolute differences>. However, the validation results shown in the discussion, contain in addition some nice information on natural variability of N2O as a function of altitude as well as some information on precision of the individually measured profiles or profile differences (sigma, sigma over sqrt N, of profiles and differences, see panels a-d of the validation figures). Why is this valuable information not exploited, leading to some final statement within the abstract? I would be personally interested in questions like

i) in which altitude domain is data quality sufficient to detect the natural variability of N2O from individual measurements?

ii) up to which altitude is data quality found to be sufficient to measure the absolute VMR from an individual ACE profile?

3599/13-14 <Overall, the quality of the ACE-FTS version 2.2 N2O VMR profiles is good over the entire altitude range from 5 to 60 km.>

>qualitative statement within an Abstract - maybe you skip this sentence?

3559/19-20 <..., again excluding the aircraft and balloon and aircraft comparisons.>

>i) I do not see to what exactly <again> is referring; and ii) why are balloon and aircraft validation results excluded?

<Introduction>

>It gives mainly a very nice science overview on the role of N2O, and on the satellite measurements/missions that dealt with it before. But at the end of the third paragraph

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I would expect a geophysical science paper to follow, and not a validation paper. I.e., paragraph 4 shows up then rather surprisingly.

To solve this the thirst 3 paragraphs might be shortened, and it would be interesting to get in addition here in the introduction some information on the state of the art (achievements/problems) of previous satellite validation studies that dealt with N2O: Paragraph 3605/8-13 might be shifted from Section 2 to the Introduction for this purpose.

Furthermore, I would be interested in a brief discussion of the natural variability of the N2O profile as a function of altitude and the mechanisms behind that. (E.g., we constructed a covariance of the N2O profile limited to 3-9 km a.s.l., see Fig. 9 of Sussmann and Borsdorff, Atmos. Chem. Phys., 7, 3537-3557, 2007). This would be a basis then to the later question whether ACE is able retrieve the natural variability of N2O.

Finally, I feel that the paragraph on the ACE mission and science goals given in Section 2 (3603/9-13 plus 20-27) would better fit to the Introduction than to the <retrievals> Section.

<3 Validation approach>

>Very good, especially the type of plots with panels a-d. But, as said before under <Abstract> (see above): These validation plots contain some nice information on natural variability of N2O as a function of altitude as well some information on precision of the individually measured profiles or profile differences. Why is this valuable information not further discussed/exploited, leading to some final statement within the Abstract, in addition to the numbers on the <mean absolute differences>? My overall interest would be to get a picture on all what is known a priori on the true N2O profile and its variability, and, then, in which altitude domains ACE is able to measure this profile and its variability with sufficient quality.

3607/13 <..., along with the standard deviations on each of these two profiles>

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>I would more easily understand <..., along with the standard deviations calculated from the individual profiles for each altitude>.

3607/20-21 <..., and the standard deviation of the distribution of this mean difference.>

> I would more easily understand <..., and the standard deviation of the individual differences of all coincident pairs as a function of altitude.>

<eq. (2) versus eq. (3)>

>It is certainly good to use eq. (3) and not eq. (2). But nevertheless, it is a rather trivial point, in a sense that it can indeed be easily understood by a scientist from 1 short explanatory statement like <use averages in case of small denominators>. To avoid overstatement and to improve readability of the paper, I would therefore recommend to cancel eq. (2) cancel all cyan solid lines in all Figures cancel 3611/9-24 cancel 3614/13-18 (<Dividing ... behaviour>)

<Table 1>; <100-1 hPa>

>change for consistency to altitude units: <x-y km> (same with 3613/10, 3613/18, 3613/21, and 3613/23)

<Table 1>; <Coincidence criteria>

>Different coincidence criteria are chosen for every correlative technique, and even different ones for MIPAS-ESA versus MIPAS-IMK.

Why are they different and what is the reason behind that? Are this ad hoc assumptions of the different validation groups? Or are all the different coincidence criteria the result of one common strategy to find them, e.g., some kind of tradeoff?

I know this is a difficult question, and it is not answered in many validation papers, but it is an important question of general interest. One possible way around is to discuss the effect on the validation results from changing the selection criteria by, e.g., a factor of 2 or 0.5: you may find an example for this approach via Table 1 of Sussmann et al.,

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Atmos. Chem. Phys., 5, 2419-2429, 2005.

3612/1-3 <These larger values are consistent with the noisier data, particularly from SMR, above 40 km, as seen in the relative standard deviations on the mean profiles plotted in Fig. 1d.>

>I do not understand this sentence.

3612/23 <... relative standard deviations on the mean profiles>

>I would more easily understand <... relative standard deviations of the individual profiles>

3616/7 versus 3617/23 <25 March> versus <26 March>?

>Typo or intended difference?

3616/8-9 <... horizontal resolution is 300-500 km along-track (Vigouroux et al., 2007).>

> Is it really appropriate to make reference to a validation paper to document the horizontal resolution of MIPAS?

<Section 5.2 and Section 5.3>

>Individual profile time mismatch is 13 h (SPIRALE) and 26 h (FIRS-2), respectively.

I would be interested in some discussion on the magnitude of the differences seen in the profile comparisons relative to the expected natural variability of the N_2O profile for the given time mismatch. Is it significant errors of the measurement systems or might the differences seen just reflect natural variability?

3625/24-25 <... determined by the sensitivity of the FTIR measurements, which 25 was required to be 0.5 or greater, ...>

>I can only guess what you really mean by <sensitivity>. Could you give a hard explanation in terms of retrieval theory: Is it the peak-height of averaging kernels, in

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which unit (VMR or normed), or is it the area of the kernels, or maybe the peak of the partial column averaging kernels ...?

3626/4-5 <... the state space interference error (due to unphysical correlations between different parameters in the state space), ...>

Finally, here I become personally very interested and curious:

- which interfering species do you consider?

- how big are the interference errors you get in real numbers?

- did you use the simple initial 1-parameter approach by Rodgers and Connor (J. Geophys. Res., 2003, eq. 8 therein), or did you use the corrected and extended approach by Sussmann and Borsdorff, Atmos. Chem. Phys., 7, 3537-3557, 2007, which gives more realistic values and thus shows much higher interference errors for ground-based FTIR, using scaling retrieval for the interfering species?

- did you try to minimize the interference errors as we suggested?

I enjoyed reading this paper.

Ralf Sussmann

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

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