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Comment

Interactive comment on “Retrieval of carbon dioxide vertical profiles from solar occultation observations and associated error budgets for ACE-FTS and CASS-FTS” by C. E. Sioris et al.

Anonymous Referee #2

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The authors perform a detailed analysis of the error budget on the determination of the tangential heights and CO₂ levels from ACE-FTS solar occultation observations. They make also comparison with in situ observations to validate their results.

The work, whilst not always easy to read and follow, should be published after major revisions.

Comments

1. Could the authors provide a first figure with a typical observation of ACE, maybe two spectra corresponding to 2 tangent heights would be valuable to see the changes

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in the absorption features and clearly understand the choice of microwindows. These microwindows and some color code could also easily indicate which line belongs to which species; an indication of the 4 microwindows used for the aerosol retrieval would also enlighten the reader. This is a general remark for the complete paper: the author obviously describe knowingly their data, but it is very hard for a reader who is not involved in the mission and instrument to follow. Some introductory words to start each section would make the reading more easy.

2. The inclusion of the CASS-FTS is not well justified nor described : one sentence at the end of the introduction and a very short (15 lines) description on the error budget. No description of the instrument or mission, of the differences relative to ACE, or of any improvements, no simulated data, analysis , etc. If this stays at this level, I would recommend to remove the reference to this instrument completely from the paper.

3. Could the authors add page number (and line number) it facilitates the commenting of the paper

4. section 1, 2§: the statement ‘well-known atmospheric pathlengths’ : is this not in contradiction to the point demonstrated in the paper ? Error on the tangent heights would impact highly on the pathlength so if THs are calculated only on the s/c information it will reflect in wrong pathlengths.

5.section 1, §starting with “The Lafferty et al . . . N2 CIA ” - determination of $\delta^{13}C$: $\delta^{13}C$ is determined using spectra taken from HITRAN : what are those spectra? It is very confusing and not easy to follow which data sets are used and in which way. Figure 1 contains data which are not cited or discussed in the text. The figure is just mentioned but not presented nor discussed. It shows differences and is not a direct illustration of the method to obtain the $\delta^{13}C$, which is what is discussed where the reference to the figure is inserted in the text. By the way in the text, it is mentioned that the spectral interval goes from 2130 to 2600 cm^{-1} , and the figure does not cover this interval. Figure 2 does not bring any information, I would suppress it. Why not replace it with

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the spectra that were used for this analysis. This will moreover show to the reader the shape of the N₂ CIA spectra, and have more feeling about the observed data. Maybe this discussion about CIA spectra should be introduced by a sub-section title ?

6. Similarly, sub-sections could be set up in section 2.1: cloud detection, Tangent heights, CO₂, and aerosols. It would render the text more readable.

7. section 2.1 - Description of clouds detection : Here again a plot of a spectrum would help following the discussion Some numbers are specified (0.0689, 0.076) without justification: either they are absolutely needed and then a more detailed explanation for the values is necessary; or they are not necessary for the understanding of the method, and maybe reference to a published paper in which more details can be found should be indicated. These numbers are indicating changes between successive tangent height spectra: is the vertical sampling always about the same that such a rough law can be applied on all occultations The authors state that only 8% of the spectra pass the selection (+15% half pass): what happens to the rest, are these data completely rejected and never analysed; I hope not, then in that case how are the tangent heights (and CO₂ profiles) determined

8. section 2.1 – description of tangent height The authors mention that some lines have been chosen to ‘increase the temperature-insensitive CO₂ signal’ : this might be difficult to understand for non spectroscopists and the distinction between sensitive and insensitive lines has not been introduced before (it is somewhat in the next §). Maybe add a column in the Table to clearly indicate which lines are or are not sensitive to temperature.

9. section 2.1 last sentence of the first page : Is the comment inside brackets ‘(which does not have a Q branch)’ pertinent to the discussion ?

10. section 2.1 – discussion on CO₂ retrieval: There is again a discussion of a comparison of data, this time spectroscopic parameters, which is not easy to follow because not well introduced. The authors should stress that the discussion concern spectro-

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scopic parameters, and laboratory measurements. The misleading sentence is ‘we have compared the line intensities to measured ones’. Even knowing that Toth and the other cited authors are laboratory related, the sentence hints about comparing with atmospheric measurements.

11 section 2.2 – The paragraph starting with ‘One source of error ...’ is incomprehensible and in contradiction with the list of all error sources just stated above. For example ‘The uncertainty due to spectroscopic parameters of the interferers ... should be considered in future work’ tells exactly the contrary to uncertainty nb 8 in the list. I suppose that this paragraph has been copied from a previous work, and not adapted consequently with the new approach.

12 section 2.2 is very heavy, not always well structured and should be rewritten. One typical example is that the different uncertainty types addressed are not following the order presented in the list. Maybe a clear separation (sub-sections) between the different topics will help. Maybe also clearly separate the ones that affect CO₂ via biases in TH and the others.

13. section 2.2 §starting with ‘Biases in TH...’ : Fig 4 and 5 are not discussed in enough details. Fig 5 contains 2 curves derived from 2 methods which are not described nor discussed in the text

14 section 2.2 §starting with ‘Perturbing the N₂’: How was the value of 0.9% chosen?

15 section 2.2 §starting with ‘We also considered the impact’ : the authors introduce a ‘scaling factor’ to take into account the spectral dependence of the aerosols, is this factor fitted on spectra, if not could it be; is this factor the same for all spectra from one occultation series, whatever the tangent height values ?

16 section 2.2 §starting with ‘In summary’: the authors list the 7 sources of error which MOSTLY affect CO₂. The 7th one, sub Lorentzian line shape, was just described in the previous paragraph and its effect was deemed ‘trivial’. Moreover this topic is not even

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further discussed in the following paragraph. Probably it should be removed from the list 17 Maybe add a Table summarizing the numbers ?

18 section 2.2 §starting with 'With respect to the overall ' : the enrichment factor is given with far too many decimal, restrict to the first decimal

19 section 2.3 : either remove or complete the description From figure 7 the total uncertainty seems to be of the same order of magnitude, I would not state that CASS error are lower.

20 section 3.2: the authors apply different versions of the software on two parts of the datasets. Would it not be more consistent to apply the same one on all data ? Since it seems that the older version cannot do it, why not apply the V3.5 on all?

Other comments

section 1, 1st §: CO₂ (volume mixing ratio) VMR profiles -> CO₂ volume mixing ratio (VMR) profiles

Section 1, 1§: Michelson . . . Spectrometer (MIPAS, Fisher et al. . .

Section 2.2, §starting with 'Error in temperature' : 'translates into an air nb density errorS since' removes S

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