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Atmospheric Chemistry and Physics Discussions

Interactive comment on "First multi-year occultation observations of CO_2 in the MLT by ACE satellite: observations and analysis using the extended CMAM" *by* S. R. Beagley et al.

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This paper presents a new dataset of CO2 volume mixing ratio (vmr) in the MLT region as measured by the ACE-FTS satellite instrument. The knowledge of CO2 is important to understand the energy budget of the MLT region and to verify our understanding of transport and mixing processes in this atmospheric region. The altitude course of the CO2 vmr shows a fall-off at about 80∼km from the constant value, which is similar to what was obtained by several infrared emission measurements. The authors try to explain this fall-off by means of the extended Canadian Middle Atmosphere Model (CMAM) by varying various model parameters. All of these scenarios are not able to

explain ACE-FTS CO2 and CO measurements simultaneously and the authors speculate about the role of meteoric dust for the destruction of CO2 without increasing CO at the same time.

In summary, the paper is worth to be published in ACP. Modifications to the 'measurement part' are necessary or this part should be published in a separate paper.

The title of the paper is a bid misleading, because the emphases of the paper (in its current version) are the CMAM simulations and not the presentation of the measurements. This would require a more detailed error analysis of the data. I suggest to strengthen the 'measurement' part of the paper (see my comments below) and keep the title; otherwise it should be changed.

I have several suggestions and questions related to the measurements, but no specific comments to the 'modeling part'. The latter is done very well and supports the results of other modeling studies (such as Lopez Puertas et al., 2000; Chabrillat et al., 2002). They agree in the fact, that tuning model parameters such as CO2 photolysis rates or diffusion coefficients are not able to explain the altitude shape of CO2 mixing ratios in the MLT region. CO2 destruction on meteoric dust might be an option to reduce CO2, but I have no expertise to evaluate this suggestion.

Another reason for the model/measurements discrepancy might be on the measurement or retrieval side. With respect to the measurement principle (solar occultation in the infrared), ACE-FTS CO2 vmr is probably the most reliable global dataset of this species we have so far. In my point of view, the first and most important step in a presentation of this data should be a thorough analysis of the data quality and its uncertainties. This is done very briefly, only, and should be extended significantly. This is of particular importance, since there is (to my knowledge) no other up-to-date publication about the characteristics and uncertainties of a CO2 vmr retrieval from solar occultation measurements. I suggest to add the following points according to this topic:

* accuracy of temperatures and their effect on the retrieval and the transformation of CO2 abundance to CO2 vmr. A summary (reference?) of ACE-FTS temperature uncertainties and validation should be given. And what is the uncertainty of the relative altitude spacing (delta-z)?

* there is a statement on page 6 about the constant CO2 vmr > 125 km in the p/T retrieval. This should be quantified.

* a detailed list of microwindows used in the retrieval should be given. A statement about the optical thickness of the atmosphere at these wavelengths would be good.

* The vertical representation of CO2 vmr and p/T in the retrieval (forward model, inversion module) should be addressed and possible uncertainties given (a reference to Boone et al. is not sufficient)

* what is the radiometric accuracy of ACE-FTS at 4.3 um? In which way is the ACE-FTS ILS considered? (Like in the Boon et al. paper?) Any uncertainties here?

* CO2 spectroscopy uncertainties should be considered in the error budget as well.

Beside this, I have some other questions/comments:

Lopez-Puertas et al., 2000 (AGU Monograph), which is an important work in this context, should be mentioned with respect to the compilation of CO2 and CO measurements and their modeling results using TIME-GCM.

ATMOS Measurements: I do not see the point, why ATMOS measurements are not considered/plotted. A signal-to-noise ratio of 74:1 (for Spacelab 3) is not so bad.

The CRISTA profile in Figure 5 should be referred as 'CRISTA-1' - CRISTA-2 profiles are available > 80km, only. Please add a statement, that the deviation of the CRISTA-1 CO2 vmr from its lower-atmosphere mixing ratios is significant above 80 km, only (with respect to the accuracy of the data).

Accuracy of ACE-temperatures (at the end of section 5): What quantitative conclusion

(with respect to the comparison between ACE and CMAM CO2) can be drawn from Fig. 11?

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