

Interactive comment on “Technical Note: Feasibility of CO₂ profile retrieval from limb viewing solar occultation made by the ACE-FTS instrument” by P. Y. Foucher et al.

Anonymous Referee #1

Received and published: 30 January 2009

The technical note “Feasibility of CO₂ profile retrieval from limb viewing solar occultation made by the ACE-FTS instrument” presents a concept to obtain altitude-resolved information of CO₂ abundances from space-borne infrared measurements. My major concern is, if ACP is the appropriate journal for this paper. Although I consider the quality of this paper as good, I realize that the reader does not learn anything from this paper about atmospheric chemistry or physics, but about measurement and retrieval techniques. While these topics were covered by ACP(D) prior to the launch of the AMT(D) journal, now such papers to my opinion clearly belong into AMT(D) rather than ACP(D), particularly as an act of fairness facing that other authors submit to the thematically best-fitting journal regardless of the current impact factor. Nevertheless, I am

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper



not going to discuss this issue any further, and will try to review the paper independent of the journal chosen.

General comment:

The paper is generally well written, well organized, with adequate references to prior work. Particularly the approach to use the N_2 signal for tangent altitude retrieval in order to utilize the CO_2 absorption for CO_2 abundance retrieval is original and well justified. I think that the first who discussed the possibility to retrieve pointing information from the IR N_2 signal might have been G. Toon (JPL) in the early 1990s but I doubt that there exists a written reference.

Some parts of the paper are quite lengthy and perhaps over-detailed. This applies particularly to the discussion of the optimization of microwindows (performance vs. number of spectral gridpoints used). I doubt that all this information needs to be included in the paper. Perhaps it would be sufficient just to present the characteristics of the finally chosen optimal setup. Lengthiness can also be criticized at other parts of the paper, e.g. the discussion of the vertical resolution.

I miss the presentation and discussion of the application of this method to real measurements, particularly since such tests are mentioned in the paper. Since this paper is not a pre-flight study, and since tests with real data seem to already have been performed, it is not clear why these tests have not been included in the paper. Only these tests will show if the method is robust and if all error sources have been considered appropriately. E.g., CO_2 results out of the expected range may hint at neglect of an important error source.

I recommend publication in the journal considered appropriate by the editor after revision.

Specific comments:

Eq. 2 and text above: The current text reads as if Eq. 2 has been suggested by

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper



Levenberg and Marquardt, but these authors did not apply their method to a regularized retrieval. A less misleading wording would be appropriate. To my knowledge, the first application of the Levenberg-Marquardt method to a regularized retrieval has been published by C. J. Marks and C. D. Rodgers, A Retrieval Method for Atmospheric Composition from Limb Emission Measurements, J. Geophys. Res.,98, D8, 14,939-14,953, 1993.

p 418 l 25: The term ‘resolution’ is misleading here. I would prefer ‘spectral grid’.

p 420 l 12: Here the authors use the term ‘geometric altitude’ as opposed to ‘true tangent altitude’. However, it is not clear to me what the difference is. Is it with/without refraction? Or is it the tangent altitude of the central path of the field of view as opposed to the lower boundary? A clear definition of the terminology is necessary here.

p 421 l 15: The meaning of the sentence “Finally, spectral regions for which radiative transfer model is not relevant are rejected.” is not clear to me. Do you mean: “Spectral regions where the signal is insensitive to CO₂ mixing ratios”?

p 424 l 17 and l 24: The error due to CO₂ and N₂O uncertainties is reported twice.

p 427 l 19 “This constraint is quite strong” is no precise wording because the noise does not change the strength of the regularization but only its impact. I suggest “This constraint has quite a strong effect” or “The regularization effect of this constraint is quite strong”.

First I was confused because here it is stated that the inverse a priori covariance matrix is used to constrain the retrieval, while in Section 3.2 a Tikhonov method is mentioned. After multiple reading I have now understood that different regularization schemes were used in the microwindow optimization and in the retrieval. An explicit statement on this might provide clarity.

p 428 l 1: The resolution is NOT limited by the field of view. In case of overlapping fields of view easily a better resolution can be obtained. This is stated in the following

[Full Screen / Esc](#)[Printer-friendly Version](#)[Interactive Discussion](#)[Discussion Paper](#)

lines by the authors themselves but the subsequent discussion does not turn the first incorrect statement correct. I suggest just to delete the first sentence.

p 428 I 6: The term ‘exceed’ is misleading, because it suggests that the resolution becomes larger (i.e. coarser). The authors obviously mean that the resolution becomes better. In this case, however, not the resolution but the resolving power becomes larger. I suggest “...vertical resolution of ACE-FTS can be better than...”

p 429 top: There is a lot of technical information about the curves and labels in the plot. I suggest to move this to the figure caption and restrict the text in the body of the paper to the scientific discussion of the results.

p 429 I 15: ‘high’ resolution usually means better resolution, not coarser resolution. To avoid any ambiguity, I suggest ‘worse’ or ‘not as fine’ or something similar, as long it is unambiguous.

p 429 I 20: The conclusion from the peak values of the averaging kernels on the source of information rely on the choice of the retrieval grid, which here probably is equal to the measurement grid. As soon as the grid is much finer, these numbers can be significantly smaller without implying that the information is dominated by the a priori. An explicit statement on the choice of the retrieval grid and its possible equivalence with the measurement grid would help.

p 429 last word: The information does not come from the a priori matrix but from the a priori profile. The term ‘a priori matrix’ is not defined. The a priori covariance matrix does not contain the information but rules the weight of the a priori profile.

p 430 I 11 and I 12: “error” is a very generic word. Instead of “error” and “dispersion” one could use “accuracy” and “precision”.

p 430 I 19: Here real occultations are mentioned. Real occultations are indeed the ultimate test-bench for a new retrieval scheme, because its robustness can be demonstrated. The discussion of the application to real applications would make the paper

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper



much more interesting, and it is not clear to me, why the whole study is based on synthetic observations. To assess a method on the basis of synthetic measurements is of course important, because contrary to the case of real occultations, the “real” state (i.e. the expected result of the retrieval) is known. However, since this paper is not a pre-flight study but real occultations are available, I miss the demonstration of the applicability of this study to real data. One can learn a lot from a potential discrepancy between real and expected results, and I do not see a justification for exclusion of real results, which, according to the text, are already available.

p 431 | 20/21: Why can, in the context of synthetic measurements, improvement on the spectroscopy affect the fit performance? Aren't synthetic measurements and $y(x)$ calculated with the same set of spectroscopic data? Don't spectroscopic errors cancel out because they are the same in y_{obs} and $y(x)$? Or doesn't this statement refer to any of the calculations actually performed?

p 433 | 14: “The major difficulty is the correlation...” This statement can only be understood after reading the following sentence. I suggest: “ The major difficulty, when applying a conventional method where the tangent altitude pointing information is retrieved from CO₂ transitions is the correlation ...”

p 434 | 16: It is ok to mention the funding sources of this paper but inclusion of the logos in the acknowledgement text is not appropriate. Such kind of advertisement does not belong in a scientific journal.

Technical comments:

p 417 | 17 and | 22 ‘non-linear’ instead of ‘non linear’; same for ‘non-linearity’;

p 425 | 1: ‘micro-windows’ instead of ‘micro windows’. Please check for consistence throughout the paper.

p 431 | 7: Fig 9b shows

[Full Screen / Esc](#)[Printer-friendly Version](#)[Interactive Discussion](#)[Discussion Paper](#)

Interactive
Comment

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper

