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Interactive Comment

Interactive comment on "New temperature and pressure retrieval algorithm for high-resolution infrared solar occultation spectroscopy: analysis and validation against ACE-FTS and COSMIC" *by* K. S. Olsen et al.

Anonymous Referee #2

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GENERAL COMMENTS

The paper describes a new retrieval scheme for temperature and pressure originally destined to be used for MATMOS, a discontinued Mars orbiting instrument.

The paper is very well written and its scope fits well into AMT. However, the paper has some shortcomings listed below, which must be addressed before publication. These relate mostly to the mathematical description of the new method in Section 2.3, which should be straightforward to address.

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SPECIFIC COMMENTS

page 10828, lines 6f:
What characteristic of MATMOS is decisive for this? spectral resolution, SNR, or both? I.e. will the TIRVIM replacement not be able to fulfill this?

page 10835, Eq. (1):
This equation, while also present in the Stiller paper, has exchanged the order of the sum and the exponential function. As written, more radiation than emitted by the sun would usually measured.

page 10835, Eqs. (2) and (3):
Eq. (2) is highly confusing. Following the argument in the text, it is reasonable to arrive at xi' = S(T) xi P T' / (S(T') P' T), which is consistent with Stiller et al. I do not see how the VSF enters the picture unless something along the lines of xi' = VSF xi => VSF = S(T) P T' / (S(T') P' T), was intended.

The usage of VSF in Eq. (3) is also not helpful as it is not clear what "Column" is or what the integration range is. Does this refer to the full column from ground to atmosphere? The full path of one spectral measurement through the atmosphere or just through one layer?

Does this text and the following paragraph relate to the fact that the radiative transport model likely does not assume each atmospheric layer to be homogeneous?

Incidentally, if my line of reasoning were correct, the quotient of xi/xi' in Eq. (5) is incorrect. While this would not affect the derived temperatures, it would affect the pressure computation by means of the intercept.

As this paper is about describing this new method, all those question must be clarified.

page 10839, line 4f:
Most of the reasons given for the method of integration could be easily achieved with simple numerical integration schemes (e.g trapezoidal rule), even exactly taking varia-

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tions in g into account. The major aspect seems to be the implicit smoothing of the T profile, which indeed might benefit the computed P. On the other hand polynomials of higher degrees have the tendency to overshoot. How do you prevent this?

page 10840, line 1ff:
This section deals with the sensitivity of the proposed retrieval scheme to employed a priori information. While it certainly argues that the available a priori information is likely sufficient (with some exceptions), it fails to quantify the reliance and thus the claim to be "minimal" with respect to a priori knowledge. For example, what effect does the a priori CO2 VMR profile has on the retrieved temperature profile? This could be quantified as a Jacobian dT_i / d $CO2_i$ profile (neglecting the potential impact of different layers) or as an relative or absolute error for a prescribed perturbation. The same is true for temperature and pressure information.

Further, the text notes that a temperature profile deviation of more than 10K would seriously degrade the quality of the final temperature product. For altitudes from the upper stratosphere upwards, this would be worrying on Earth (a worrying that seems to be justified as for some profiles, where the a priori profile differed by 30K from the ACE-FTS result, which consequently could not be reproduced by the new scheme). How does this relate to expected temperature uncertainties on Mars, which most readers (as am I) are probably not an expert in?

As the method is in effect a kind of linearisation around the a priori CO2/temperature/pressure profile, could it not be employed iteratively; that is use the derived T/P profile as "a priori" and repeat the process? In fact, Stiller et al. used this to reduce the impact of a priori and applied some damping factor for the iterative updates to compensate for over-correction. Did you follow such experiments, especially with the aspect of reducing the impact of required a priori information? One might combine this with a trust region-like approach that'd limit the amount of temperature correction to the 10K where the linearisation seems to hold up quite well.

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Lastly, many of the given arguments for the increasing quality of Martian a priori information could also be brought forward to justify the use of the ACE-FTS retrieval scheme. At which point is the proposed scheme more robust?

MINOR REMARKS

page 10828, line 28f:
Fixing parameters makes the retrieval simpler, obviously. But I do not understand the notion that fixing parameters is "advantageous since gas absorption coefficients depend on T and P". I assume that some elaboration would be helpful in respect to what is going to be fixed and what the added advantages are beyond making the originally ill-posed problem well-posed.

page 10830, line 3:
I assume that 129 profiles of spectra were analysed?

page 10834, line 19:
What kind of a priori information is entered here for the retrieval of CO2 with respect to CO2? Is optimal estimation used or some kind of Tikhonov-regularisation?

page 10834, line 21:
VSF looks like an acronym, but was not introduced.

page 10835, line 14:
It would be clearer to introduce I' here as [$I'(v)/I_0'(v)$].

page 10835, line 16:
I and I' as defined in (1) are not defined on a layer, however, part the sum contained in (1) are, which is probably meant here, but not stated. The easiest remedy would be the definition of some variable with an underbrace or similar for the term in between [and] in (1).

page 10835, line 18f:
Dropping the subscripts may enhance the brevity, but not the clarity. While xi' was a

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discrete variable previously, it is unclear in what fashion it is used in Eq. (3) where a continuous function in s is suggested.

page 10836, Eq. (5):
The physical constants h and c are missing. Or simply c_2 could be used.

page 10836, lines 14ff:
The description now mingles the planetary-body agnostic algorithm and the implementation and data used for the tests using ACE-FTS data. It might become more readable with a separation of method and application.

page 10836, line 17:
It is rather confusing to reuse xi here for the true CO_2 profile.

page 10837, line 1:
"and a the" -> "and the"

page 10837, line 10:
What kind of uncertainties are used here? Noise error, or also systematic errors due to background gases?

page 10837, line 12:
What is T and what is delta T? You state that you derive one temperature T_j for each line j. Is delta T_j = T_j - T' ?

page 10838, line 25:
I do not see how keeping M and g constant keeps this more adaptable to other planets than allowing a variation in latitude and altitude for either variable. Obviously it is the simplest solution. To keep it adaptable but more correct, it could be simply implemented as a table to be read in, while an analytical formula would naturally be preferable.

page 10840, line 20:
Why should the VSF of CO2 not be unity? Isn't it mostly in the vicinity of unity, especially, if the a priori temperature and pressure are good?

page 10845, line 15:
Why was this interpolation scheme chosen, especially in contrast to the perhaps more common four-point cubic spline? Is this consistent with the interpolation scheme that underlies the radiative transfer model?

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