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Atmospheric
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Discussions

Interactive comment on "HCl and ClO in activated Arctic air; first retrieved vertical profiles from TELIS submillimetre limb spectra" *by* A. de Lange et al.

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General

We would like to thank the reviewers for their concise and instructive reviews of our paper. Below we address the concerns and queries of both reviewers. Page, line, Figure, and Table numbers refer all to the unrevised manuscript as published on the AMTD website.

Response to Referee #1

Comment 1

I would like to see concentrations expressed as ppbv rather than ppb to avoid confusion between mass or volume mixing ratios.

Authors: Text and Figures 2, 4, 5, 6, 8, 9, and 10 are changed accordingly.

Comment 2

To give an idea of how sensitive the instrument is could you mention the system noise specification (for SMILES which uses similar technology it is 300K SSB).

Authors: Text adapted (p. 6502, l. 5–7) into:

"This 4×4 mm^2 receiver shows a tremendous miniaturisation over traditional electronic circuits, with almost no loss on noise properties. The double sideband system noise temperature of this channel is 250 K, which is indeed very comparable to the cryogenic space-borne ISS/SMILES instrument that has a single sideband noise temperature of 330 K (?), being equivalent to a double sideband noise temperature of 230 K."

Comment 3

On page 6500 line 29 I would say precise rather than accurate.

Authors: Text changed accordingly.

Comment 4

On page 6507 where you discuss how you set the regularisation parameter (gamma). A similar thing is done in a paper by Schimpf and Schreier (JGR, 102, 16055, 1997). You might want to reference that paper? Here they plot the least squares norm against the constraint norm for different values of gamma and as you say choose the value at the corner of the L shape. What you have done seems different in that you plot the norm of the solution (x in eq 3) against the least squares constraint. This might be virtually the same thing, I am not sure, but the left hand side of eq3 should read $||x||^2$ *to show the result is a scalar quantity (as stated in the text).*

Authors: The L-curve approach was proposed by Hansen in 1992. However, Schimpf and Schreier have succesfully applied the technique in atmopsheric retrievals. To add this reference, the text has been adapted (p 6507, after line 24):

"This inversion technique is used frequently for the interpretation of atmospheric limb measurements e.g. **?**, **?**, and **?**. The corresponding minimisation function becomes"

For Tikhonov regularisation in its standard form, the L-curve is the parametric plot of the norm of the regularized solutions versus the least squares norms for all valid regularisation parameters. Eldén has shown that Eq. 3 with an arbitrary $\mathbf{L}$ can always be transformed into its standard form and the the L-curve technique can be applied to the transformed problem, which has been done in the presented study. To clarify this we have added the following sentence at page 6508, after line 3:

"The corresponding cost function in Eq. (3) is transformed to the standard form with $\mathbf{L} = \mathbf{I}$, the unity matrix, as described by **?**". And in Eq. 5 $\mathbf{I}$ is replaced by $\mathbf{L}^\mathsf{T}\mathbf{L}$.

Finally, the left hand side of Eq.3 is not a scalar quantity but a vector and should read $\vec{x}_\gamma$. It is the vector that corresponds to the minimum of the cost function for a given γ.

Comment 5

Page 6510 line 3, I would replace completely with accurately.

Authors: Text changed accordingly.

Comment 6

Page 6511 line 22. What is shot noise?

Authors: Shot noise means the noise of a single measurement which can be described by a Poisson distrubution. However, for the statement in the manuscript, this definition is not essential and we adapted the text to:

"Although the measurement error is not dominated by the random noise on the measurement, systematic errors with pseudo-noise characteristics average out in the mean."

Comment 7

Page 6514 line 3. I would say all errors steeply increase for $H^{37}Cl$*, surpassing those of* $H^{35}Cl$*. As stated it makes it sound like* $H^{37}Cl$ *is more accurate than* $H^{35}Cl$*.*

Authors: Text has been changed accordingly.

Comment 8

A general comment. The MLS vertical resolution for HCl and ClO can never be better than 2.7 km (the retrieval grid). Therefore it really does not make sense to apply the TELIS SIR AK (2 km) to the MLS data. I assume this made no difference in the com-

parisons. If anything you probably should apply the MLS AK to the TELIS data but I think that for this kind of work it is acceptable to just directly compare the raw retrievals.

Authors: Below flight height, the TELIS averaging kernel is generally narrower than the MLS averaging kernel. However above the flight altitude, the TELIS kernel broadens quickly and at these altitudes the TELIS averaging kernel needs to be taken into account. The study is limited to the altitude range below 35 km and so, as suggested by the reviewer, one can use unconvolved profiles for the comparison. This does not affect the results of this study.

In the revised version of the manuscript, we compare TELIS and MLS profiles without using the averaging kernel of the corresponding retrieval. Figures 2, 4, 6, and 9 have been adapted and the following lines have been removed from the text: p6512 l7–9, p6512 l10–11, p6516 l5–6, p6529 last line, and p6533 last line.

Comment 9

On page 6517 paragraph at line 19. Another factor that might delay the rise of ClO in sunlight may be caused by the time it takes to illuminate the limb path which is several hundred kilometers. This consideration depends on how the instrument line of sight is aligned with the rising sun.

Authors: During the measurements the azimuth of the line of sight was carefully taken parallel to the terminator, or perpendicular to the rising sun. This was indeed not mentioned in the article and the following line is inserted at p 6517, after line 21:

"During the measurements the azimuth of the limb path was taken perpendicular to the rising sun to ensure uniform illumination of the line of sight."

Response to Referee #2

General comment 1

The way you calculated the systematic error due to the detectors' non-linearity was a bit unclear for me. You calculated the systematic errors due to non-linearity by comparing the retrieved profiles between two cases: using non-linearity gain compression factors of 10% and 25%, respectively. Is this correct? Or, is it a difference between the cased of 10% (or 25%) and 17.5% gain compressions? Perhaps, you can add a plain explanation for your calculation.

Authors: It is the difference between 10% (or 25%) and 17.5% gain compression. To state this more clearly in the manuscript, the lines starting from 'In the retrievals ...' (page 6510, lines 19-22), are replace by:

"This range can be rewritten as 17.5% $\pm$ 7.5%. In the retrievals, the central value of 17.5% compression is used. To assess the error propagation on the retrieval products, the retrievals are repeated with the compression set to 10% and 25% respectively, and the error is taken as the difference profile of 17.5% and 10%, which is almost equal to the difference between the profiles of 17.5% and 25%."

General comment 2

For the error analysis on the spectroscopic parameters, I suggest including an additional uncertainty due to the temperature dependence of the broadening coefficient. Also it may be useful for future works to compare the HCl spectroscopic parameters between yours and those in MLS data processing.

Authors: The temperature dependence of the broadening coefficient is implicitly accounted for in the overall temperature error. This error captures all temperature dependencies in the model. To clarify this in the manuscript we have inserted the following line at page 6511, line 1:

"This error covers all temperature dependencies, including atmospheric black-body radiation, partition function of level population, and the temperature dependence of the spectroscopic broadening coefficient."

General comment 3

I recommend adding a bit more detail description about your forward model. For example, do you calculate only targeted HCl and O3 lines with some continuum absorption coefficients? Or do you apply any line selection procedures? What kind of atmospheric refraction model do you use? etc.

Authors: The forward model has been discussed more extensively in ?, which is referred to in the text as well. To include interesting details on the forward model in the current manuscript, we have rewritten the paragraph starting on page 6505, line 21, into:

"The forward model comprises the solution of the radiative transfer equation in a non-scatering atmosphere for thermal equilibrium in spherical geometry, resulting in the intensity as function of frequency, at the gondola altitude in the viewing direction of the instrument. It is a line by line model and only lines from the HITRAN 2008 database (?) are included that have a contribution of more than 0.001 K to the limb spectrum with a tangent height of 25 km in standard atmosphere. To account for broadband continuum emissions, the Liebe 1993 continuum model (?) has been adopted. Refraction of the limb path is accounted for by implementing a non-dispersive refractive index, as is described by ?. The forward model also includes an instrument model to account for the specifics of the TELIS instrument. For more details on the forward model we refer to ?."

Textual corrections

All textual corrections of reviewer 2 are incorporated in the revised version of the manuscript.

Inversion, comment 1

6507, L25. Eq(3) "$\vec{x}_\gamma$" seems to be a typo of "$\|\vec{x}_\gamma\|^2$".

Authors: $\vec{x}_\gamma$ is correct. It is the vector for which the cost function is minimal, for a particular value of γ. Eq.(3) is not changed.

Inversion, comment 2

6508, L13-19. I think that how you select the regularization parameter is one of the most important (and interesting for readers) points of this paper. I would suggest showing one figure of actual L-curve, just for an example to readers, from your HCl or ClO retrievals.

Authors: The suggestion by the reviewer is adopted and an example of an L-curve is included in the manuscript between Fig. 1 and 2. The following line is inserted on page 6508, line 17:

"An example of such a curve is given in Fig. 2."

Retrievals, comment 1

p6510, L1-. Did you try a frequency-dependent offset for the retrieval parameter (I mean, not a constant offset but with slopes, or n-th order polynomial functions with *respect to frequency)? It might be interesting to check whether fitting quality improves or not with introducing a slope-like brightness offset (in particular, for the ClO window).*

Authors: In case of HCl retrievals, a possible slope is effectively accounted for by retrieving ozone simultaneously. In case of ClO, retrieving a 1st order polynomial simultaneously rather than an offset, does not affect the retrieved ClO profile. A higher order polynomial may interfere with the line fitting, and is therefore not considered. We propose to leave the manuscript as is.

Retrievals, comment 2

p6510, L25-29. Temperature/Pressure profiles are one of the most important factors in the retrieval analysis. I would suggest comparing your temperature a priori (i.e., MIPAS product) to the ones from MLS measurements, or to the reanalysis model data such as ECMWF or GEOS5, in order to confirm if the assumption of 1K systematic error is appropriate. And, I think it is also possible to retrieve the temperature profile from the O3 line measured by TELIS. This can be another interesting future work.

Authors: In previous studies the MIPAS-B temperature profiles have been validated against ECMWF and/or radiosondes (**?**) and against MIPAS Envisat and (indirectly) other satellites (**??**). In most cases MIPAS-B temperature data are within 1 K (1σ) with respect to ECMWF. Sometimes larger deviations can be explained by the fact that MIPAS measures 'integrated' temperature (cf. 2D averaging kernel) whereas ECMWF gives gridded assimilated data. In the manuscript a reference is already included to the study of **?** and the cited error of 1 K has been used in the current study. We propose therefore to leave the text as is.

Retrievals, comment 3

p6511, L1. The error of 1 K is assumed for the temperature profile. Is this a systematic error? Or including the random errors coming from the measurement noise of MIPAS instrument?

Authors: This error includes both systematic and random errors.

Retrievals, comment 4

6511, L6. Comment: If I remember correctly, the broadening parameter of ClO 501 GHz on the HITRAN 2008 is based on the laboratory measurements by Bauer et al. (1998, within a frame work of an ESTEC study for the MASTER database). As described in the MASTER introduction paper by Perrin et al. (2005, J. Atmos. Chem. 51, p 161-205), their measured broadening parameter was significantly larger than the gammas for other ClO transitions measured by different groups. For this paper, I think the currently assumed 5% uncertainty on ClO gamma is a reasonable value, but just I would like to point out that gammas are still a kind of highly caution-needed parameters.

Authors: We fully agree with the reviewer's comment.

Retrievals, comment 5

6512, L7-11. Considering the closeness of the vertical resolutions of MLS and TELIS, I think you can directly compare both profiles without applying vertical smoothing. This comment is also applied to the ClO comparison.

Authors: The averaging kernel is mostly relevant above flight altitude where the TELIS kernel broadens and the kernel by MLS is still narrow. In the current study, however, the profiles are only investigated up to an altitude of 35 km and we agree that profiles can be compared directly without using an averaging kernel. Text and figures are adapted accordingly (see also Comment 8 of reviewer # 1).

Retrievals, comment 6

6512, L29. If I see the Table 2 of Froidevaux et al. (2008), I would select 0.2 ppbv rather than 0.1 ppbv.

Authors: In the footnote of that Table, it is stated that the values pertain to 2σ, and therefore we opted for half the value.

Retrievals, comment 7

6517, L20 (Fig. 10). I would like to see the corresponding solar zenith angles within this plot (for example, putting ticks of solar zenith angles at the upper horizontal axis). And I would prefer to use different symbols, not only different colors, for the better visibility.

Authors: Both suggestions are now included in the figure.

Future

Do you have any plan to perform further comparison studies with other satellite measurements, such as SMILES, ACE/FTS or Odin/SMR? In particular, I expect interesting/useful works can be done with SMILES and Odin/SMR since they observe exactly the same HCl and O3 625 GHz transitions (SMILES), and ClO 501 GHz (SMR), if these instruments were luckily observing at close locations with TELIS at this day/time...

Authors: We fully agree with the reviewer and we have actually concrete plans into that direction. We have added the following in the future outlook of the manuscript at page

6518, after line 28:

"In addition, cross-validations will be performed with the ISS/SMILES, Odin/SMR, and ACE/FTS instruments. In particular the SMILES and SMR instruments are interesting since they observe exactly the same transitions of HCl (SMILES) and ClO (SMR)."

1 Other adaptions

We took the liberty to implement two minor adaptions.

1.1 Adaption 1

Going from Eq. 2 to Eqs. 3 and 5 it is implicitedly assumed that the measurement noise covariance matrix is accounted for in $\mathbf{K}$, which involves a transformation. This transformation is not mentioned in the text and to make all equations consistent we have adapted Eq.3 and Eq.5 to:

$$\vec{x}_\gamma = \min_{\vec{x}} \left(\|\mathbf{S}_{\vec{y}}^{-\frac{1}{2}}(\mathbf{K}\vec{x} - \vec{y})\|^2 + \gamma^2 \|\mathbf{L}\vec{x}\|^2 \right), \quad (1)$$

and

$$\mathbf{D} = \left(\mathbf{K}^\mathsf{T} \mathbf{S}_{\vec{y}}^{-1} \mathbf{K} + \gamma^2 \mathbf{L}^\mathsf{T} \mathbf{L} \right)^{-1} \mathbf{K}^\mathsf{T} \mathbf{S}_{\vec{y}}^{-1}, \quad (2)$$

respectively.

1.2 Adaption 2

We have changed the time axis in Fig. 10 from local time to universal time and have adapted the text accordingly.