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Atmospheric Measurement Techniques Discussions

Interactive comment on "Two-dimensional performance of MIPAS observation modes in the upper-troposphere/lower-stratosphere" *by* M. Carlotti et al.

Anonymous Referee #1

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The manuscript quantifies and inter-compares the performance, when a 2-D tomographic retrieval approach is applied, of the nominal and two other measurement modes dedicated especially for investigation of UTLS by Michelson Interferometer for Passive Atmospheric Sounding (MIPAS) on ENVISAT. For the evaluation, the authors also apply a novel information load approach developed recently by the authors that helps especially to make considerations about appropriate retrieval grid.

The manuscript provides new results, from which not only MIPAS users but also users or developers of other limb satellite instruments considering tomographic retrievals

could benefit. But there are some issues that from my point of view are not discussed or argumented well enough and also may cause misunderstandings. Therefore I would suggest this study to be published in AMT when the following points are clarified in order to improve the understanding and quality of the manuscript:

General points:

In your previous work (Carloti et al., 2001) you show that in 1-D retrieval, where horizontal homogeneity is assumed, systematic errors are introduced in retrieved values when in reality the atmosphere is horizontally inhomogeneous. However it is also shown (at least for scattered light measurements, see Pukite et al., AMT, 3, 1155, 2010) that also for a 2-D tomographic retrieval with a coarse horizontal retrieval grid such systematical errors are not completely eliminated if separation of scans is coarse. I am wondering, why you do not discuss this issue in your manuscript when assessing the retrieval quality of the different MIPAS modes (I think this is important because in Carlotti et al. 2009 (Fig. 7) you showed that the horizontal sensitivity in many cases and especially at altitudes of UT/LS is very unsymmetrical w.r.t. the tangent point). It would be not surprising if such systematic errors become visible for the NOM and perhaps also for UTLS-1 mode. I think it is essential to assure that the result allows to distinguish between random errors and systematical errors because each of them require different methods to fight with them. Standard deviations of the difference between retrieved and true values in Figs 6&9 are not allowing this distinguishing.

Do you calculate information load in Sect. 4 according its definition in Eq. (4) or from the relation with Jacobean matrix in Eq. (5)? Could you add a short description, how do you calculate the information load in practice? I find, in general, the text in Sect. 3.3 below Eq. (4) confusing (see also related specific comments later). There you write e.g. that you assume that the observations are characterized by the same uncertainty. But text does not say why and how well such an assumption is justified? Could you say in the text (perhaps in Sect. 2) if the signal, which MIPAS measures at different modes, has the same quality? If the uncertainty is not the same – is then the quantitative comparison that you provide for the magnitude of information load between different measurement modes, meaningful?

In second row after Eq. (5) you say that "uncertainty on the value of the target quantity q in clove h would be given by $1/\mathbf{\Omega}$". Although the usefulness of the definition of this "uncertainty" is not clear for me (see specific comment later), why do you not just compare it with the uncertainty that you get from simulation study and show in Figs. 6&9?

Is it really always possible to explain the retrieval instability with information load also for scenarios where the distribution of the information load is uniform, e.g. for UTLS-2 mode? I think that it is not always possible and limitations are not discussed at all in the article. As I could understand from Eq. (5) (although notation is unclear, see also specific comments), you analyse only diagonal elements of squared Jacobian matrix ($\mathbf{K}^T\mathbf{K}$), i.e. for a given clove, squared $\mathbf{\Omega}$ is a dot product of $\mathbf{K}$ of different geometries, microwindows and spectral points with the same $\mathbf{K}$ of same geometries, microwindows and spectral points. Thus I would think that using the information load, it is not possible to judge about how well the problem is determined (or constrained) because the off-diagonal elements of this matrix are not considered in the analysis. I imagine that it is still possible to have a uniform distribution of information load and at the same time to have a horizontally (or vertically) underdetermined and thus instable problem like for the UTLS-2 mode with the natural grid. In other words, information load tells if some region is seen by the instrument and provides some quantification for this. But it, especially for a uniform case, does not tell under which geometries observations are performed. But tomography requires that each observation is performed under its own unique geometry. The problem with small horizontal intervals between scans can be that the geometries are not completely unique, thus the problem becomes underdetermined.

Specific points:

Page 2862, lines 8-9: "in terms of strength, spatial coverage and uniformity" I suggest to skip "and uniformity" because "spatial coverage" is a generalization for uniformity.

Abstract, last line: "validity" -> "worthiness". I think here you wanted to say that the approach can be successfully applied and gives some benefit.

Page 2863, lines 14-15: "to derive the atmospheric field of geophysical parameters" -> "to derive the field of atmospheric parameters".

Same page, line 25: "in terms of strength, spatial coverage and uniformity" I suggest to skip "and uniformity" also here.

Page 2864, lines 1-2: skip "2-D" writing "The algorithm..." because Rodgers introduces AK also for 1-D or any other algorithm that uses his method. Also reference to Rodgers should be put immediately after "provides AK" because Rodgers, 2000 does not discuss the usage for the resolution for multi-dimension cases. Perhaps at the end you might give a reference to your previous work where you introduced the 2-D resolution concept.

Sect. 2, description of measurement modes: I think a table could be helpful summarizing the main features.

Sect 2: How large is field of view? What exactly is the optical path in the "new" configuration? Is the time the optical path is scanned and spectral resolution the same for all modes? In general: is the spectral "quality" of signal the same for all modes?

Page 2865, lines 6, 10 and 16: Please provide the values for the separation also in degrees in terms of OC because later in text you will use them.

Page 2865, line 20: "North Pole" -> "most northern point of orbit" because orbital inclination of ENVISAT is not 90 degrees.

Sect. 3.1. First two lines: “that determines, at the tangent points of each limb scan” Is not the sensitivity of limb 1-D retrievals shifted somewhere towards instrument? Anyway, the values are determined for a horizontal area with extension of many hundreds of km because of low horizontal resolution and might not be the same as at the TP. Therefore I would suggest rephrasing the sentence, e.g. saying something like: “the values are determined for each scan, described by its TP”.

Page 2868, line 21: spectral signal - what is its corresponding physical quantity (spectral radiance?) and unit?

Same line: “at frequency j”: When describing the instrument you used wavenumbers. Could you be consistent?

Page 2869, 1st and 2nd line: "The column vector ... is the Jacobian matrix": I do not understand this sentence. Is the Jacobian matrix a vector? Please clarify.

Page 2869, lines 3-4: “If we assume that the observations are uncorrelated and characterized by constant uncertainty” How well are these assumptions justified for MIPAS measurements? Is it the same uncertainty for all geometries, spectral points and microwindows, and measurement modes as well? Are different microwindows at unique wavelengths?

Eq. (5): Please explain what do you mean with notation you use on the left side regarding brackets and index h: $\mathbf{K}^T\mathbf{K}$ is a square matrix with total number of elements being number of cloves to the square. I assume in my considerations that you select only the diagonal elements of the product? But then the notation is wrong and should be e.g. $(\mathbf{K}^T\mathbf{K})_{h,h}$

Page 2869, lines 7-8: “the uncertainty... would be given by 1/Ω”. Is it true? In Eq. (2) One needs to invert the whole $\mathbf{K}^T\mathbf{K}$ and take the diagonal elements for uncertainty from the resulted matrix. Here you want to say that it will be the same if you invert

only the diagonal elements of the $\mathbf{K}^T\mathbf{K}$. This can be true if all off-diagonal elements of $\mathbf{K}^T\mathbf{K}$ are zero...

Page 2870, lines 5-6: “It can be seen in Fig. 3 that the three observation modes differ in intensity.” Again: It is not said before if the signal quality for different modes is the same. Suppose you have different errors of signal between the modes (e.g. due to different integration times), is the comparison meaningful?

Page 2870, lines 18-19: “only positive values are represented in the map” Why negative differences are not important in your reasoning? One could plot vice versa the difference between NOM and UTLS-1 and conclude from only positive differences that NOM is better! I think you should say that positive differences outweigh the negative ones and you definitely should show this by including them in the plot. Otherwise it is not possible to judge if this is the case; maybe few very large negative values are larger than many positive?! The grasp can be improved anyway by using different colormap.

Page 2871, line 20: You could skip here the reference to Rodgers, 2000

Page 2871, line 22: You say that you use 1% perturbation of temperature. Is it not too less comparing with the real variation of temperature in the stratosphere? For other targets you used perhaps more reasonable values. Is the magnitude of perturbation affecting the results?

Page 2872, lines 10-13: Eq. 1 contains also Marquardt damping factor λ. Is it also set to zero? “Weak a-priori information was adopted” – Could you specify this a bit more quantitatively or provide a reference?

Page 2872, lines 21-22: “with acceptable precision” How much is acceptable precision, can you give some number?

Page 2872, lines 22: “highest tangent altitude of the observations.” Could you provide a number in brackets mentioning this altitude? I know that one can find it in Fig. 1 but it would be convenient to have it mentioned also here.

Same page, line 24: “information load decreases due to the decreasing N_2O VMR”: Can this decrease be explained by Eqs. (4) or (5)? Explain, how?

Page 2873, line 11: “the precision of UTLS-1 is comparable” -> “the precision of UTLS-1 in terms of standard deviation of the difference between retrieved and true distribution of the target is comparable”

Page 2874, lines 21-22: “trigger a retrieval instability due to an insufficient information load of the observations.” Despite the fact that the distribution is horizontally almost uniform!? I would doubt that the fluctuation is due to insufficient information load, unless you provide some prove. Previously you even showed that UTLS-2 has the best information load from all compared modes (last sentence before Sect. 5). In fact, the information load is also best distributed uniformly. Could it be that the fluctuation is rather caused by underdeterminancy of the retrieval problem because retrieval geometries are not anymore uniquely crossing cloves? But information load approach, which analyses only the diagonal elements of $\mathbf{K}^T\mathbf{K}$, do not provide information how well the problem is determined.

Page 2874, lines 21-22: I would suggest to add here something like: “because the problem becomes better determined” or “because the problem becomes better constrained”.

Last lines in page 2874: You say that 2.25 deg leads to satisfactory stability. But the right panel of Fig. 7 shows something else, i.e. is there no other parameter that would show this more directly that variable vertical resolution? In fact Fig. 7 even shows that the natural grid despite the fluctuations still has better resolution even at the peaks... Perhaps you should demonstrate instead that the benefit of reduced retrieval error outweigh the reduced resolution.

Page 2877, lines 14-15: “up to altitudes that far exceed the UT/LS” Please give some number for these altitudes.

Figs. 2&3. Ω with respect to temperature. Should not also “K” appear as a dimension in the label besides the colorbar?

Figs. 6&9, Right column, x-axis: dimension for vertical resolution “deg” $>$ “km”?

Technical corrections:

Equations: You use “S” both for covariance matrix and for spectral signal. Could you choose different letters for them?

Page 2866, line 14: “In a Geo-fit” -> “In the Geo-fit approach, . . .”

Page 2867, line 25: “In a 2-D analysis” -> “In the 2-D analysis” because you speak about your approach.

Page 2869, line 21: reported -> indicated

First line of page 2870 (and other places in the manuscript): “blow-up” -> “zoom-in”

Page 2874, line 16, add coma after “grids”

Caption of Fig. 2, 2nd line: reported -> indicated

Figs. 6&9 It could be considered to explain the meaning of the lines of different colors within the figure, i.e. adding a legend box where you label them. This is suggested in the manuscript preparation rules. But for me the explanation for these few lines in the caption (as it is now) is also OK.

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