Atmos. Meas. Tech. Discuss., 5, C3204–C3209, 2012
www.atmos-meas-tech-discuss.net/5/C3204/2012/

Atmospheric Measurement Techniques Discussions

Interactive comment on "Position error in profiles retrieved from MIPAS observations with a 1-D algorithm" *by* M. Carlotti et al.

M. Carlotti et al.

massimo.carlotti@unibo.it

Received and published: 11 December 2012

The reviewer' s objections are reported in italic before our replies.

The concept of 'position error' itself (section 3), has been discussed previously e.g. in von Clarman, 2009, and although this paper applies a different technique it does follow many of the same processes. This is acknowledged and addressed somewhat in Section 5.3; however it would be clearer if this discussion were to be included within Section 3 itself.

In the revised text we have added a statement (after introducing the 'position error' at the end of line 8 of page 6528 in the discussion paper) that acknowledges the previous

discussion in von Clarmann et al. (2009). We refer to Sect. 5.3 for a discussion that, included in Sect. 3.2 would have broken the flow of our reasoning.

As the most novel results of this paper concern the impact of the temperature position error on trace gas retrievals I would suggest changing the title of the paper to reflect this.

Actually our paper is centered on the identification of the position error and its implications. We show that one (and probably the most relevant) implication is the propagation of the T position error on VMR retrievals. In von Clarmann et al. (2009) the emphasis is centered on the horizontal resolution of 1-D retrievals. Therefore we prefer to maintain the original title.

the effect of temperature position errors in an inhomogeneous atmosphere was discussed and compared to a 'total' error due to horizontal inhomogeneity. However, it was confusing, and somewhat contradictory at times as to what this 'total' error included; is it the effect of horizontal inhomogeneity in both temperature and trace gases, or is it solely inhomogeneity in temperature that is considered (i.e. position error compared to other sources of error such as the spread of information). For example, while the last sentence in section 4.1 talks about 'the total error expected as a consequence of not modelling the horizontal variability of T in 1-D retrievals', in Section 4.2 the description states that it is horizontal inhomogeneity in both T and VMR's.

The reviewer is right. The blue line in Fig.s 6, 8 and 9 represent (according to the procedure correctly described in Sect. 4.2) the total error expected as a consequence of not modelling (all) the horizontal variabilities in 1-D retrievals. The last sentence of Sect. 4.1 has been modified in the revised text to correct our mistake.

However, if both temperature and vmr inhomogeneities have been considered, it would be instructive to also show how much of the remaining error in figures 8 and 9 are due to other errors due to temperature and how much due to vmr inhomogeneity errors.

We agree. Actually we are analyzing the relative contributions of T and vmr inhomogeneities but they do not combine linearly and the results differ depending on the target vmr. This kind of discussion would be lengthy and beyond the aim of this paper. We plan a dedicated paper to this subject.

In both the abstract and conclusions it is stated that the authors have shown that the information load analysis provides a tool for the selection of observations that minimize the position error of the retrieved profile. This did not seem to be discussed anywhere in the actual text though. These claims should either be removed, or actually proved in the main paper.

In our view the statement in the abstract and conclusions was legitimated by the five panels of Fig. 1 showing the contribution of the individual microwindows to the asymmetry of the whole set. The meaning of the five panels and the conclusions are discussed in the last paragraph of Sect. 3.2 (lines 1-6 of page 6529 in the discussion text).

The first part of the paper defines position error to be compared to the average position of the tangent points of all the scans in a set of measurements. However, in the latter portion of the paper, the tangent point of each scan separately is the more relevant quantity. Why is this not considered instead in the definition of position error?

In the definition of position error we compare to the location where the retrieved profile is represented in practical retrievals (the average position of the tangent points) with the position where the information actually comes from (the median of the information load). In Sect. 4 we study the propagation of the T position error into the retrieved values of the VMR profile (independent of where the VMR profile will be geo-located). For this exercise we need to leave the approximation of average position and we want to compare the position where the information is believed to come from with the position where the information actually comes from (the median of the information load). The simulated observations are generated from a set of reference profiles that describe the

horizontal structures of the atmosphere (as stated at point 1 of Sect. 4.2). For the purpose we have used profiles retrieved from a previous analysis of the considered orbit and we have located these (slant) profiles at the position of the tangent points (Fig. 7 left panel) where the information is believed to comes from. We have specified this choice at point 1 of Sect. 4.2 in the revised text.

The position error is defined with respect to the median of the IL analysis. However, as is acknowledged, this definition breaks down at some altitudes. It would be useful to have indications on the plots or in the texts as to which altitude ranges we can use 'position error' as a sensible concept.

The altitude where the median criterion (from which depends the assessment of a position error) fails depends on the target and on the considered atmospheric scenario. At line 25 of page 6527 (of the discussion text) we provide the value of 40 km as average of the highest altitudes where the median criterion is valid. In the revised text we better specify (at the end of the period) that the threshold depends on target and atmosphere.

The simulations and conclusions have only been drawn on the basis of the operational MIPAS microwindows. However, I believe that these microwindows have already been selected to minimise the impact of horizontal gradients? If other spectral regions were to be used would the conclusions drawn in the paper be affected?

These MWs have been selected to minimise the impact of horizontal gradients but not the impact of the position error (that was not considered at all) and the propagation of the T position error on VMR targets due to horizontal gradients. We have tested sets of MWs used for the 2-D retrieval of T, water and ozone (that were selected without the requirement to minimize the impact of horizontal gradients): the results are quite similar to those shown in this paper.

In Section 5.3, it is stated that the advantage of the IL analysis over 2D averaging kernels is that it is a property of the observations rather than the retrieval. However, given

the variability of position error around the orbit and the dependence on the temperature gradient, is there a practical way of using the IL analysis without first retrieving the temperature distribution?

We plan to carry out a statistical analysis in order to establish whether it is possible to characterize the position error as a function of season, latitudinal band, day/night. . ... If some reproducibility is verified we could think to a recipe for corrections.

Figures 1, 3 and 11 are somewhat confusing, particularly the fact that the atmosphere is plotted upside down with the surface at the top. It would be clearer plotted the other way up, but at the very least there should be clearer labelling of altitudes on the plots. They would also benefit from having the position of the satellite indicated.

We have chosen to show a blow-up around the south pole because the temperature field of the Antarctic summer atmosphere allows to better appreciate details that are discussed in the paper. Probably it would have been more confusing if the south pole (marked in the figures) and the Earth's curvature were upside down. In the revised paper we have included altitude labelling in Fig.s 1, 3 and 11. The position of the satellite is indicated in the text at lines 16, 17 of page 6528. We found it difficult to introduce the satellite in the above mentioned figures but we have specified its position in the caption of Fig. 1 in the revised text.

section2: Since ENVISAT is no longer operating, this paragraph could be updated slightly

In the revised text we have specified the end of ENVISAT operations (at line 18, page 6523 of the discussion text).

p6537, line 25-26. 'e.g. the cross-section of the analysed transitions' – this is not mentioned in the text only in the conclusion. It would be helpful to add this part to section 5.2

We have included this consideration at the end of Sect. 5.2 of the revised text.

p.6521, References in lines 12 + 13. As there are lots of different retrieval techniques/ papers it would be better to have these as examples

In this introductory section we want to remark the major steps of the dimensional evolution going from "onion peeling"(0-D) to "global fit"(1-D) to "2-D" methods. In this respect we believe to have cited the basic papers. However, if the reviewer is aware, and wish to suggest, other basic papers of the dimensional evolution that we have missed we will include them in the next steps of the publication process.

The English in the paper would benefit from further checking. While generally ok and always understandable, there are a lot of minor errors.

In the revised text we have implemented several English corrections (mostly suggested by the other reviewer). We leave the editor to decide whether copy editing is needed.

Interactive comment on Atmos. Meas. Tech. Discuss., 5, 6519, 2012.