Atmos. Meas. Tech. Discuss., 3, C2856–C2860, 2011
www.atmos-meas-tech-discuss.net/3/C2856/2011/

Atmospheric Measurement Techniques Discussions

Interactive comment on "Fast NO_2 retrievals from Odin-OSIRIS limb scatter measurements" *by* A. E. Bourassa et al.

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Received and published: 27 February 2011

Please read and refer to the Author Comment posted to Referee #1 as many of the points below are addressed in that response.

Anonymous Referee #2 Received and published: 28 January 2011

The manuscript introduces a new method for NO2 profile retrieval from ODIN measurements. The measurement vector is constrained by a modified triplet technique involving 4 spectral pixels around the NO2 absorption band at 448 nm, and the MART technique is used to iteratively find the profile. The algorithm seems feasible and the manuscript provides new interesting results. I generally support its publication in AMT. However,

in my opinion, there are some deficiencies which should be addressed beforehand: 1. It is not possible to fully appreciate the new method due to lacking error analysis and corresponding discussion. The reader does not get information with what decrease in retrieval precision one should pay for the increase in the calculation speed.

We have addressed these concerns in the response to Referee #1.

2. I see that the daily average determines the zonal variation rather well, is it the case also for individual orbits? The analysed data set (only one day) is very small. What would be standard deviation for profiles retrieved for a longer time period? How it compares with retrieval precision, i.e. can a substantial part of this variability be explained by natural variation? Also results for different seasons could be investigated. Is the discrepancy at high latitudes appearing then also?

The statistics of comparisons with individual profiles and the scope of the comparisons are addressed in the response to Referee #1.

3. In introduction, abstract and conclusions, the authors state that the much faster approach (comparing with the standard retrieval) could be used for tomographic retrievals. This is especially enhanced in the conclusions where one of the only two sentences states this, although such an application is not presented and discussed in the main text of the manuscript at all. Although it is meant as an outlook, reading only the abstract and/or conclusions, one can get impression that a major part of the paper is about a new tomographic approach. I think that these statements in their current form and proportion give wrong impression about the work actually performed.

Although this is an important potential application of the technique, we did not mean to imply that we have performed any work towards this end in the paper and we will be clear of the scope of the work in the revision.

4. Also the statement (which is even meant as a main conclusion of the study) about the reducing for "the computational cost by almost an order of magnitude" is mentioned

only in the abstract and conclusions as a conclusion of the study. It is not discussed and proved in the main text, in which steps of the retrieval and how the improvement is get, and how time consuming are each of these steps comparing with the nominal retrieval.

This is addressed in the response to Referee #1 and is made clear in the revision.

5. The algorithm description is not complete. Standard method uses a number of spectral corrections. Are there corrections for error sources necessary to worry about, e.g. Ring effect, straylight...? Is it necessary to account for them in the new retrieval? If yes, mentioning this and a short description would be nice. What cross-sections are used, at which temperature? Dou you use the same slit function for your retrieval as for the standard method to convolute the cross-sections to actual resolution? Cross-sections generally vary with temperature; how this is accounted for in the new approach comparing with the standard retrieval?

All of the corrections and error budget investigated by Haley et al. apply equally to this retrieval. We will clarify this in the revision. Following Haley et al., we have used the same slit function to convolve the cross sections and we handle the temperature dependence using a linear interpolation of the cross section measured at 4 temperatures.

6. The discussion of the results is much too short. What are the strengths/weaknesses of the retrieval without that it is now much faster? What is the impact of different retrieval settings (see also the comment before)?

See comment to Referee #1.

7. With respect to the conclusions section: Two very general and partly speculative sentences is too less for conclusions. Please summarize some key facts from the results and discussion. Which is the essential retrieval step where you get the improvement? Besides the statement about the calculation time, it would be nice to see some measures about the differences in accuracy and precision between both compared

retrievals.

Again, see comment to Referee #1.

Specific comments: Abstract: Second last sentence, second part (also similar statement in the introduction and conclusions): I think these statements are too speculative and I am sceptic about them. Arguments and discussion for them are needed in the main text of the manuscript. Two-step algorithms suffer not much from increase in the computational burden (comparing with direct retrievals) when moving from 1D to 2D because the spectral part of the retrieval stays the same and RTM should be done for one or two spectral points per one geometry only. Also total number of geometries per orbit is not very large, so inversion is not a problem for a modern PC. Is it then reasonable to degrade the retrieval precision few times as a cost for higher speed? Do you need now to do RTM for each of 4 points to get modelled?

We believe that the referee is referring to the typical trade-off between spatial and spectral accuracies. The number of geometries for an instrument designed to make tomographic measurements with high horizontal resolution can be very large and, as demonstrated here, the benefit of an order of magnitude less time per spectral calculation with a decrease of say a factor of 2 in precision might be an attractive alternative. We will expand this discussion in the revision.

Abstract: last sentence: Do you have some estimate for optimal spectral resolution requirements for your retrieval? Although one could gain on signal to noise ratio, reducing spectral resolution will also smooth out the narrow absorption bands, so the measurement vector will reduce. Can you provide arguments for that in the main text of the manuscript? What instrument would be capable to satisfy at best your requirements?

This study could be done; however, it seems that a first order estimate of a slit function that is a few times narrower that the absorption features is all that is required to implement this technique.

Page 5502, line 2: I would suggest adding SCIAMACHY, to read: “current instruments, such as OSIRIS or SCIAMACHY”.

We add SCIAMACHY to the revision.

Interactive comment on Atmos. Meas. Tech. Discuss., 3, 5499, 2010.