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Atmospheric Measurement Techniques Discussions

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

A. E. Bourassa et al.

adam.bourassa@usask.ca

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We thank the referees for the positive comments and the good suggestions for improvement of the paper. The main concern of both referees was the lack of error analysis in the original work. We agree that inclusion of the precision analysis for the new retrieval method is a very important aspect of the work and have provided the error estimates for both the new and standard retrieval methods below. Additionally, at the suggestion of both referees we have included an analysis of the comparison statistics for individual profiles as well as the zonal analysis presented in the original manuscript. We have also made an effort to expand the discussion of the results and provide more detail on the algorithm; however, we appreciate that the nature of the journal is to publish "short

and concise" papers on retrieval techniques and we have striven to maintain this work as a concise demonstration of the applicability of the new technique.

We have prepared a revised manuscript that addresses each of the referees' points as detailed in the red comments below and we thank the editor for consideration of the revision for publication in AMT.

Anonymous Referee #1 Received and published: 13 January 2011

General comments:

In this paper the authors apply a variant of the so-called triplet method and use the MART retrieval scheme to retrieve OSIRIS NO2 profiles using only four wavelengths. I think that this new method is sound and quite interesting addition to the other NO2 retrievals around. This paper is suitable for the AMT special issue and can be published after responding to the concerns mentioned below. I only find the comparison section of the paper insufficient.

1. There is no discussion whatsoever about the error estimates of the new nor the official NO2 product.

The figure below is a revision of Figure 5, which now includes the precision of the new MART retrieval (in the red shading) in addition to the error estimate from the official NO2 product (in gray bars). The error bars shown on the profile are the average uncertainty for each of the average profiles (and not the error in the average profile). Thus these error bars show the typical error for a single retrieved profile in each of the latitude bins. As expected, the uncertainty in the MART retrievals is systematically larger than that in the official product. However, for mid and high latitudes where the signal to noise is high due to the smaller solar zenith angles at the tangent point, the MART error bars are much less than a factor of two larger those of the official product. However, in the tropics where the signal to noise is decreased, the MART uncertainty can be several times larger than the uncertainty in the official product.

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2. Fig 5. Instead (or additionally) of the zonal means, the authors should compare individual profiles and plot the mean/median (and deviation) of the individual relative differences for different latitudes/nodes.

As suggested, we have included a new figure (below) that shows the statistics from the comparison of all individual profiles from the full day of retrievals. The mean of the percent difference from each comparison does not show any bias and is within a few percent of zero from 14 km to 34 km. The standard deviation of the comparison is approximately 20% at all altitudes. This is somewhat high, however, it simply reflects the size of the error bar in the NO2 retrieval as shown in the previous figure.

3. Only one day of measurements is used for the comparisons. I would really like to see more data to be compared before drawing conclusions. I realize that this is more like a proof of the concept, but a little more thorough comparison would improve the paper.

We feel that in the interests of focusing on the presentation of a retrieval technique, as opposed to the development of a new NO2 product, we should keep comparison section succinct. We have already included a new figure to show the statistics of the comparison of individual profiles. Additionally, for the interest of this referee we have modified Figure 4 to included comparisons of both the dawn and dusk sectors of the orbits with the differences as compared to the official product; again these show the good agreement between the MART retrievals and the official product.

4. The processing time of the old and the new products (with a typical modern hardware) should be mentioned somewhere. I am also missing some analysis or at least discussion about the optimal number of wavelengths. The authors use here four wavelengths but would you get better results if you had eight? Would the processing time be twice as long then? Some kind of sensitivity analysis would be required to fully understand this.

This is discussed to some degree in the paper; however we are happy to explain more

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thoroughly in the revision that the retrieval time requirements are essentially proportional to the number of wavelengths required in the forward model. We feel that we have used the smallest number of wavelengths, i.e. the fastest retrieval, possible with the OSIRIS measurements. The additional of more wavelengths will improve the retrieval precision, as shown in the revised Figure 5; however, in the interest of processing time, using only 4 wavelengths as opposed to 36 is roughly 9 times faster.

Specific comments: 1. Abstract "..boosting signal to noise by reducing spectral resolution requirements." Are you saying that the spectral resolution is not so important for future instruments? Are you sure you don't mean the spectral range of the instrument?

Indeed you are correct. This technique shows only that a reduced spectral range is sufficient; we will clarify this in the revision.

2. Sect. 1. "Radiance were measured at two wavelengths..or two wavelengths.." Sounds like you don't know which wavelength pair they used. Say "..depending on the orbit", or so.

You are correct. The phrase "depending on the orbit" clarifies this sentence.

3. Sect. 1. "The large number of wavelengths measured by current instruments..." I'm not sure if the number of wavelengths is really the crucial issue here. I would think that more important is the spectral resolution and the signal to noise ratio of the instrument. These are determined by the instrument design (slit, integration time etc.). Of course it makes a difference if you measure at 50 wavelengths instead of 2, but if you have noisy spectra with poor spectral resolution, it won't help if you have 500.

Again, you are correct. We will clarify this and use "spectral resolution" and "signal to noise" instead of "number of wavelengths".

4. Sect. 1. "The addition of a..." This sentence is slack.

We will change it to 2 sentences: "The addition of a few discrete channels in the NO2 absorption region is an attractive improvement for instruments designed for high-

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resolution in other spectral regions, for example, an O3-BrO-OClO instrument. Alternatively, it can provide an efficient correction for interference due to NO2 in an instrument dedicated to another species, such as the Ozone Mapping and Profiling Suite (OMPS) scheduled for launch on the NPP satellite (Flynn et al., 2009).

5. Sect. 2. "The retrieval technique we have employed..." I find this paragraph a little confusing. You first say that your retrieval is based on the triplet technique explained in the references. Then you say that you generalize this technique. What do you mean by that? The only difference you mention here is that you use 4 wavelengths instead of "small number of wavelengths". This is hardly a generalization. However, in the end you say that you use Multiplicative Algebraic Reconstruction Technique? Do you use triplet or MART or both together?

We use both together: the triplet technique is use to construct the measurement vector and the MART is the non-linear inversion. We will clarify this in the revision.

6. Sect. 2.1. "In this work, we have strategically chosen 4 OSIRIS.." You should give some reasons why you selected these 4 specific wavelengths. In Fig. 1. the two wavelengths at 450nm are almost the same. I guess you have some good reason for this?

Page 5505, line 10, specifically addresses the inclusion of the 4th wavelength.

7. Sect. 2.1. "..the effective depth of the absorption feature is decreased through the averaging." What is the advantage of striving for the maximum effective depth? I mean are your results really worse if you cover the whole absorption peak with pixels? Or doesn't it make any difference?

Maximum NO2 optical depth means maximum sensitivity to NO2. The point is that with OSIRIS resolution, additional pixels always fall away from the peak, resulting in an average value that does not maximize the optical depth.

8. Sect. 2.1. "...we can add an additional reference wavelength without decreasing the

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effective depth." Did you try to add a second wavelength to the short wavelength side too? And maybe on the top of the peak, too? I don't see much difference (in the cross section level) in these cases either, so the effective depth wouldn't decrease.

It may be possible to add an additional short wavelength reference, however, if you inspect the cross section closely, even one more pixel near the peak will decrease the effective optical depth. In any case, we believe the proof of concept of this technique is still clear using the selected wavelengths.

9. Sect. 2.1.1. "...we calculate the average value of the measurement vector over a range.." Say the range (it looks like 45-55km in Fig 1.?). OSIRIS scans go much higher, did you try some different altitude ranges? And do you know how much there is deviation (or noise) in the radiance at these tangent heights and wavelengths?

You are correct that the scans go much higher, however, measurements above 55 km at these wavelengths have significantly reduced signal to noise and using them provides only marginal improvement in the averaging for the reference.

10. Sect. 3. "..with vastly different techniques." "Vastly" is an exaggeration.

We will remove "vastly".

11. Sect. 3. "difference between our results minus the official version divided by the average..". You are comparing your results with the validated official product. Then your relative difference should be (new-official)/official*100%.

Agreed. We have modified Fig 4.

12. Sect. 4. This is the shortest conclusions I have ever seen.. Don't you have anything to speculate? Future plans or work? Are you planning to compile the whole OSIRIS dataset with this new method?

Yes, we can expand the conclusion to better represent the work in the paper; however, we do not wish to advertise this as new OSIRIS NO2 product. This work is the

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presentation of the alternative and efficient retrieval technique.

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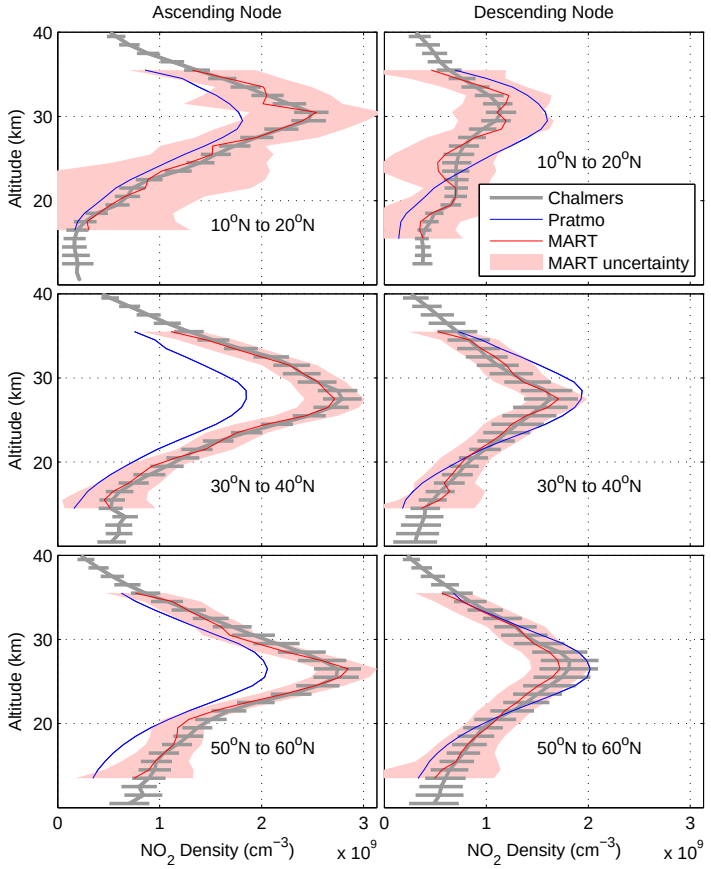

Fig. 1. Revised Figure 5

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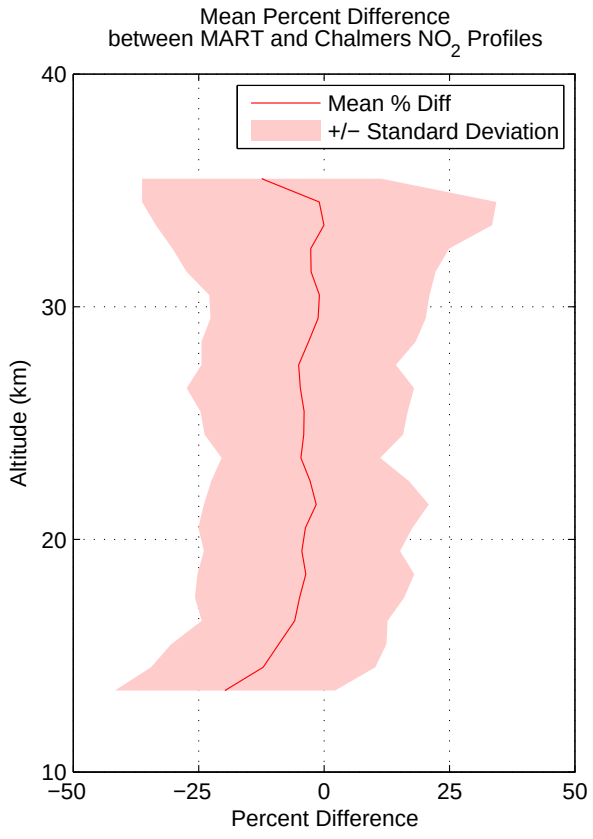

Fig. 2. To become Figure 6

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