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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Odin/OSIRIS observations of stratospheric NO_3 through sunrise and sunset" *by* C. A. McLinden and C. S. Haley

Anonymous Referee #2

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General comments:

The manuscript presents very interesting scientific material highlighting an unique possibility to observe stratospheric NO_3 by satellite instruments in limb viewing geometry. However, the authors seem to see the main goal of the paper in an evidence that the OSIRIS instrument is capable of measuring at quite large solar zenith angles, rather then in a scientific significance of the new technique to observe the stratospheric NO_3. It's a pity because the first one is rather a technical issue which could be much better demonstrated validating the ozone profiles retrieved at large solar zenith angles. The latter has much larger scientific importance and would conform much better with the title of the paper. So, I think, besides technical details described in my specific comments below, authors need to reconsider the weighting of the paper goals in the abstract and the conclusions focusing more on the scientific importance of NO_3 observations. Furthermore, it should be noted that the fitting of the slant columns is just an intermediate step to the retrieval of NO_3 vertical distributions. Thus, taking into account that the vertical profile retrieval for photochemically active species is not quite straightforward, possible ways how to do the next step, i.e., the profile retrieval, need to be discussed at least schematically. In general, the manuscript is suitable for publishing in ACP after a moderate revision.

Specific comments:

1. Page 5903, Fig 1: It would be useful to show the nighttime profile and the shadow height at each solar zenith angle.

2. Page 5903, Fig 1: “At SZA of 95.5° altitudes above ≈25 km are directly illuminated (neglecting refraction)” - an estimation of the illuminated altitude without refraction is not meaningful.

3. Page 5903, line 6: “At the onset of sunrise (SZA=97.8°)” - how the “onset” of the sunrise is defined? Which altitude is illuminated at specified SZA?

4. Page 5903, line 11: “.. due to the time constant of Reaction (R1)” - please provide an estimation for the time constant.

5. Page 5903, lines 14 -16: “Stellar and lunar occultation ... most recently with GOMOS” - this is correct for the stellar occultation but not for lunar, where SCIAMACHY and SAGE III retrievals should be cited as “most recent”.

6. Page 5903, line 22: “off-axis zenith technique” - conflicting attributes. “zenith” - means that the instrument looks vertically upwards (elevation angle 90°) and everything which is not “zenith” is commonly referenced as “off-axis”.

7. Page 5903, lines 25 - 27: “The fitting window 590-680 nm was selected over the more common 640-680 nm window as the additional pixels increased signal-to-noise

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and reduced correlations with other absorbers, particularly ozone." - the spectral points below 610 nm are quite noisy and contain no additional NO_3 information, so it is not obvious that adding pixels between 590 and 610 nm one really increases the signal to noise ratio. This statement has to be justified in any way. Moreover, looking in page 5906 one sees that this additional spectral range seems to be responsible for the negative bias in the fitted slant columns: "This was determined to be an effect of using the wider fitting window that includes the peak in the ozone absorption at 603 nm. That is, increasing the short wavelength end of the fitting window to, e.g., 610 nm, eliminates this feature.". So, please, give the reasons why you do not want to get rid of the bias skipping "unnecessary" spectral points. The statement about "reduced correlations with other absorbers" would be more convincing when supported by the plots (all other absorbers and pseudo-absorbers).

8. Page 5904, line 27: "The two spectral windows give very similar SCDs" - which two windows? 590 - 680 nm and 640 - 680? If yes, that means that the "correlations with other absorbers" mentioned above do not play any role, right? Furthermore, if the SCDs from these two regions (i.e., including and excluding the ozone peak at 603 nm) are similar, the explanation of the negative bias given at page 5906 (as mentioned above) is wrong.

9. Page 5905, cross sections: why did you select 230 K to scale the NO_3 cross sections to? As follows from Fig. 5 the temperature variation of 20 K is not unusual in the stratosphere. So, why should it be scaled at all and why not to, for example, 220 K (as ozone), 202 K (as NO_2) or 243 K as H_2O? How strong is the dependence of NO_3 cross section on the temperature?

10. Page 5905, line 29: "...SZA bins between 91° and 97°" - are these SZAs at the tangent point?

11. Page 5906, line 1: "...SZA changes by 0.5° to 0.8° over the course of a scan..." - is this a variation of SZA at tangent point? What is the variation of SZA along the line of

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sight?

12. Page 5906, Fig 3: error bars need to be shown

13. Page 5906, line 8: "...SZA of 96° during sunrise abundances are near their nighttime levels..." - neither from Fig 3 nor from Fig 4 is clear what the nighttime level of NO_3 SCDs is. I understand that it is impossible to show the retrieved nighttime SCDs but may be you can plot simulated values in Fig 4.

14. Page 5906, lines 10 - 12: "At a SZA of 93° there is only a small amount remaining near 12 km and at a SZA of 92° the SCDs do not differ significantly from zero." - Unfortunately, this is not so nice as described, namely, the SCDs are negative almost in the entire altitude range at 92° and above 20 km at 93°. So, I think it would be much more fair to say that the retrieval results are not meaningful for solar zenith angles lower than 94°.

15. Page 5906, lines 12 - 13: "At sunrise there is insufficient signal to obtain SCDs at a SZA of 97°." - please explain why the signal at the sunrise is weaker than at the sunset. According to Fig 1 there should be a lot of NO_3 seen. Does OSIRIS get less light during the sunrise than during the sunset? Why so?

16. Page 5907, lines 17 - 19: "Note that neither VECTOR nor the photochemical model account for refraction. For a geometric tangent height of 30 km and SZA of 94°, refraction reduces the tangent height by about 2 km (e.g., Uhl and Reddmann, 2004)" - So what? You are talking about the tangent height of the direct solar beam which has absolutely no relation to the discussion in the manuscript. What you really need to know is how the illumination of the atmosphere is changes due to the refraction. Namely, the shadow height is lower than the refraction is accounted for, however, the entire illumination is a little bit weaker due to the longer light path. The first effect which, I guess, is more important for your study can be approximated by an effective solar zenith angle at which then all the model simulation have to be done (instead of the geometrical solar zenith angle).

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17. Page 5907, lines 21 - 22: "Overall, the magnitude and behavior with SZA is very consistent between the modeled and observed SCDs." - comparing Fig 3 and Fig. 4 I can not agree that "he magnitude and behavior with SZA is very consistent". Yes they are similar but not more.

18. Page 5907, lines 22 - 26: "However, there appear to be some differences in the growth of NO_3 after sunset (panel b), with modeled SCDs lagging the observations by about 0-0.5°. Likewise, the model calculations seem to be systematically smaller during sunrise (panel a), and lead the observations by 0.5-1°." - I think the refraction is the main reason for that because the real illumination of the atmosphere does not match the assumed one. I think the results would look much more similar if the simulations were done for the effective solar zenith angles rather than for geometrical ones. A little bit more mysterious is a quite different behavior of the measured and simulated SCDs at sunset below the maximum. Namely, the measured values are significantly larger. Perhaps authors should think a bit more about how to explain this disagreement.

19. Page 5907, Fig 4: I'm a little bit confused by the negative values in simulated SCDs. In absence of errors (either systematic or stochastic) the negative values can only be caused either by a correlation of some fit parameters or by a bug in the fit software. So I would suggest authors to check their retrieval software and reconsider the fitting spectral range to eliminate negative bias at least in the model simulations.

20. Page 5908, line 15: "the temperature profile is consistent with the SCDs." - This is not completely true. Between 38 and 43 km the temperature in scan 1 is higher whereas the SCDs are smaller. Please explain this.

21. Page 5908, lines 26 - 27: "Furthermore, the strong temperature dependence suggests the potential to derive atmospheric temperature information as has been successfully carried out using GOMOS data (Marchand et al., 2007)." - this is only true if NO_3 concentration is known or can be easily modeled like nighttime NO_3 in the steady state assumption in the cited paper. This is, however, not the case for a very complex

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dynamical system during sunrise and sunset. I suggest to remove this sentence from the manuscript.

22. Page 5909, lines 17 - 18: “A logical next step is to test the consistency of OSIRIS NO3 by finding coincidences with SCIAMACHY, GOMOS, and/or SAGE III occultation measurements.” - I do not think that such comparison is possible in terms of SCDs, so the next step should be the retrieval of vertical profiles from SCDs and then the comparison.

Technical corrections:

Page 5903, line 13: “Noxen et al.” - should be “Noxon et al.”

Interactive comment on Atmos. Chem. Phys. Discuss., 8, 5901, 2008.