

Interactive comment on “Springtime Arctic ground-based spectroscopy of O₃ and related trace gases at Eureka, Canada – Part 1: Evaluation of the analysis method and comparison with infrared measurements” by E. Farahani et al.

Anonymous Referee #1

Received and published: 6 April 2009

This is a review of the paper “Springtime Arctic ground-based spectroscopy of O₃ and related trace gases at Eureka, Canada - Part 1: Evaluation of the analysis method and comparison with infrared measurements” by E. Farahani et al.

General Comments

My overall impression of the paper is that there is not a significant amount of new information presented here that could not have been included in the second paper that is mentioned. The improvements provided by the use of WinDOAS are not entirely

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unexpected and, in any case, are only briefly discussed. The comparisons between the UT-GBS, FTS and ozonesonde data, although new results, are discussed in a fairly qualitative fashion.

Specific Comments

1. One instrument parameter that can be important in the retrieval of trace gases is the pixel sampling with respect to the spectral resolution. This information for the UT-GBS should be added to section 2.

2. In section 2 you mention that in 2001 you observed that the bias was no longer constant and from that point onwards determine a daily bias value. However, you do not comment on whether the bias was indeed observed to be constant in the previous period or just assumed to be, and the impact on the retrievals if the bias had indeed been variable prior to 2001.

3. You do not specifically comment on what the primary improvement in switching to WinDOAS. It apparently provides (a) a better wavelength calibration, (b) a wavelength-dependant slit function, (c) and a slit function that is varied with time, as compared with your original DOAS analysis. Have you assessed which of these improvements has provided the most benefit? At least some information on the change/improvement in the RMS residual and calculated DSCD using the two methods should be mentioned in regards to Figures 2 and 3.

4. On page 348, line 16, you state that you used WinDOAS with “exactly the same retrieval parameters”. You should be more specific in what you mean by “retrieval parameters”. Are you referring to the absorption cross section, pseudo-absorbers, etc., as well as the spectral window?

5. You should clearly state in section 3 that the improved spectral fitting provided by WinDOAS makes retrievals of additional trace gases such as OCIO possible, to provide a better connection to the OCIO retrievals that you discuss later. E.g. OCIO retrievals

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were not possible with your original DOAS analysis?

6. In your DOAS/WinDOAS comparison plots (Figures 4 and 5), I think that the horizontal lines that you show are a bit misleading. It would be better to just state in the text the difference in the average VCD values from the two methods.

7. In the first sentence of section 4.1, I am not sure what you mean by “corrected twilight spectra”. You discuss calibrating (and presumably normalizing) the spectra with a common reference spectrum for each year in the following sentences, so what do you mean by “corrected”?

8. You state that you used averaged profiles from 11 ozonesondes for the a priori temperature and absorber (O_3) number density, but it is not clear exactly what this means. Were these ozonesondes from one year, or all years, etc. How representative is the O_3 profile that you use, and what error is introduced from any initial profile errors?

9. What is used for the a priori NO_2 profile and again, or are the retrievals affected by initial profile errors?

10. It appears that the UT-GBS errors that you present in this paper (Table 3) are largely from the use of the original DOAS analysis (from Bassford et al., 2001). Although some of these errors might also directly apply to the WinDOAS analysis, you should specifically comment on this.

11. HITRAN 1992 seems a fairly dated version of line parameters to be using in your analysis, in particular since later you state that line parameter uncertainties can be a significant source of error in the MSC FTS retrievals.

12. You state the “total uncertainty” for the MSC FTS measurements and a reference to the method used to determine this uncertainty. However, I feel that more information is required, at the very least a breakdown of the error sources that were included in the analysis and their magnitudes.

13. In the ozone total column discussion, you need to be more specific about the

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“correction” that you apply to the ozonesonde data if you are going to include it in the comparisons.

14. The ECMWF plots (figures 7-10) are very difficult to read.

15. Is the localized event that you mentioned in line 18 of page 354 seen in any other data (e.g. ECMWF O₃)?

16. It appears that the UT-GBS and MSC FTS differences vary quite a bit, both day-to-day and year-to-year, but this is only vaguely discussed as perhaps being caused by spectroscopy errors or viewing different air masses. More discussion of this is required if such comparisons are being used to quasi-validate the UT-GBS measurements.

17. In the NO₂ total column discussion, presumably some effort could be made to correct the MSC FTS measurements to the UT-GBS local time (or vice-versa) to provide a better comparison rather than just broadly stating that the differences are attributed to diurnal cycle differences.

18. In the OCIO DSCD discussion, I am not sure that the measurement is “novel”, rather it is a “new” measurement at ASTRO. Also, although it might be apparent for some readers, you should discuss why you do not attempt to calculate OCIO VCDs.

19. In the conclusion you state that the original DOAS and WinDOAS results were found to agree to 4% and 5%, but you do not conclude that the WinDOAS results were better, although it would seem that it does produce better results.

20. In the conclusion you mention that the total columns from the UT GBS agree to “better than 5%” with the FTS measurements and “generally agreed” with the ozonesonde measurements, but this is not really presented (in particular the 5% value) in the previous discussions.

Technical Corrections

1. On page 348, line 23, RMS has not been defined.

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2. On page 348, line 28, SZA has not been defined.

3. In Table 1, FWHM has not been defined.

Interactive comment on Atmos. Meas. Tech. Discuss., 2, 343, 2009.

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