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Atmospheric Chemistry and Physics Discussions

Interactive comment on “Intercomparison of ground-based ozone and NO_2 measurements during the MANTRA 2004 campaign” *by* A. Fraser et al.

A. Fraser et al.

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We thank the reviewer for their helpful comments on our paper. In the following, the reviewer’s comments are repeated in italics, followed by our responses.

Page 3, Section 3.1, end of paragraph 1: It is said that the NO2 DSCDs calculated using WinDOAS for MAESTRO and SPS are unreliable. Could you give possible explanation(s) on this and include them in the paper ? Is it related to WinDOAS or to the MAESTRO and SPS observations ? If it is a WinDOAS-related problem, contacting M. Van Roozendael and C. Fayt could certainly help, as suggested by Referee H. K. Roscoe.

We believe the issue to be with the MAESTRO and SPS observations. The MAESTRO and SPS both have uncooled detectors, whose signal-to-noise ratios increase with a colder temperature. Both instruments were operated inside a modified commercial freezer in an effort to reduce the temperature and increase the quality of the recorded spectra. This deployment was not ideal, and the viewing windows of both instruments often frosted over, decreasing the signal to the detectors. This decrease in signal would affect NO2 retrievals more than ozone retrievals due to the fact that the signal from NO2 is much weaker than that from ozone. For MAESTRO, another possible issue is the direction of the polariser, installed in the foreoptics. This polariser was found to be in the wrong position at the start of the next measurement campaign that MAESTRO participated in. If the polariser was in the wrong position, only the weak polarisation would have been recorded during the MANTRA campaign, which could explain why the NO2 signal is so weak in the MAESTRO spectra. We have added the above discussion of these two problems in Section 3.1.

Page 3, Section 3.1, paragraph 2: One single NO2 profile is used to calculate NO2 AMFs. So the variation of the NO2 concentration along the different light paths is not taken into account in the AMF calculation. At SZA around 90, this can have a significant impact on the NO2 AMF and therefore on the NO2 VCD. I think you should include this source of error in the estimation of the error on NO2 VCDs given at paragraph 3 in Section 3.1.

The radiative transfer model allows NO2 to vary along the path of the light, using a climatological diurnal variation. We have added a sentence to this section clarifying this fact. The error in the VCD includes a term to account for possible errors in the AMF.

Page 4, last paragraph of Section 3.1: Mention here that total columns of SO2 are also retrieved using Brewer observations is a bit confusing because the Brewer SO2 columns are not shown and discussed in the paper. I would mention only in the instrumental section (Section 2) that the Brewer also measures SO2.

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We have removed reference to SO2 in this section of the paper.

Page 5, Section 4.2, paragraph 1: SPS O3 DSCDs display large scatter especially at SZA higher than 90deg. Could you comment on this in the paper.

This is most likely a result of the ice build up discussed above, which was worse during the period the instruments were left by themselves overnight. We have added a sentence discussing this:

"SPS DSCDs become scattered above 91o, most likely due to the build up of ice on the viewing window during unattended operation, which was worse for the SPS than for MAESTRO."

Also at the end of the same paragraph, it is said that both SPS and MAESTRO datasets are not always in agreement at small SZAs. From Fig 1a, it is also the case at high SZAs (higher than 90deg). Again, could you comment on this.

By this we mean that the DSCDs of the two instruments are sometimes shifted with respect to one another (just as the UT-GBS DSCDs are 250 DU higher than the SPS and MAESTRO DSCDs in this figure). We have clarified this sentence to read:

"The two datasets are not always in agreement, on some days the DSCDs are separated by a roughly constant value of up to 200 DU over all SZAs."

Page 5, Section 4.2, paragraph 2: Due to large errors at small SZAs in the UT-GBS data, only SZAs in the 85-91deg range are used in the regression analysis. Is it permitted by the NDACC to reduce the SZA range for Type 1 analysis ? If not, could this comparison still be labeled 'NDACC' ?

The instrument comparison protocol document (Johnston et al.) states:"some groups may have instruments that produce results that are close to these figures over a more limited SZA range and these should be considered in their merits." Considering the low signal-to-noise ratio of the UT-GBS at small SZAs, due to the error in the data acquisition code, we feel the limited SZA range represents a fair comparison of the

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four instruments.

Page 5, Section 4.2, paragraph 3: The residual of the type 1 analysis for ozone are large and variable. Except for the UT-GBS, this may be due to shifts in the calibration of the spectra (due to temperature change). It is mentioned that this effect is not accounted for in the WinDOAS analysis. Why ? According to my knowledge, this effect can be accounted for in WinDOAS.

We have removed this section. The shifts in the calibration due to temperature changes are accounted for in WinDOAS, by calibrating each spectra individually.

Page 8, paragraph 2: The SPS VCD in average 15 DU lower than those from the other instruments. For some days (e.g., 236 or 253), the difference reaches about 35 DU. Do you have possible explanation(s) for that ?

The difference between the SPS and the UT-GBS ranges between -5 and 25 DU, between the SPS and SAOZ it is between 1 and 25 DU, and between the SPS and MAESTRO it is between 4 and 23 DU. There is no obvious correlation between the difference and cloud cover or SZA range of DSCDs for either instrument, nor is there any correlation in the differences between the instruments (e.g. the SPS-SAOZ difference is not correlated to the SPS-UT-GBS difference). We believe the large (and small) differences to be random, and to be expected from any comparison of two data sets.

Page 8, Section 5.2: Both instruments show a general decrease in NO2 during the campaign period. It would be useful to mention in the paper the percentage of decrease in NO2 during the campaign for both morning and afternoon and for both instruments. According to Fig. 8, it seems that for the morning, the decrease seen by the SAOZ is smaller than with the UT-GBS.

In the new analysis, the decrease seen by SAOZ in both the morning and the afternoon is larger than that seen by the UT-GBS. We have added discussion on this decrease in Section 5.2, and have added the observed percentage differences.

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Page 9, Conclusions: The fact that the O3 and NO2 DSCDs and VCDs from UT-GBS only partially meet the types 1 and 2 NDACC standards can be, according to the authors, related to a problem with the detector and an error in the data acquisition. Since these problems have been solved, do you have first indications (e.g., from comparisons of Eureka campaign data sets) that the UT-GBS instrument will now meet NDACC standards ? If not, a "non-optimal" use of WinDOAS should be considered and again, contacting M. Van Roozendael and C. Fayt would help.

The results of the Eureka comparisons are an improvement over the results discussed here.

Interactive comment on Atmos. Chem. Phys. Discuss., 7, 10205, 2007.

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