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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Retrieval of nitrogen dioxide stratospheric profiles from ground-based zenith-sky UV-visible observations: validation of the technique through correlative comparisons" *by* F. Hendrick et al.

Anonymous Referee #2

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The paper deals with the very interesting issue of NO2 profiling from ground-based UV-Vis observations. The authors go one step ahead of previous papers by comparing the retrieved profiles with satellite and balloons direct observations, establishing the limits of resolution and errors.

The manuscript, particularly the theoretical description, is written carefully in a concise way.

General comments

From the title I was expecting that profiles retrieved from GB slant columns were to be

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validated by balloons and satellites measurements but when reading the manuscript, it is unclear who is validating who. When discrepancies are found between satellite and GB, the authors attribute the differences to limitation in the solar occultation technique below 25 km but they donŠt provide any explanation on the causes. Even though that could be the case, a description of the possible satellite limitations would help to support the statement.

Specific comments

Figures 6 to 8 and comparison sections. There must be a problem with smoothing (SAOZ, DOAS, POAM III) since smoothed profiles do not match the unsmoothed ones. The discrepancy is particularly obvious in Fig 6 Harestua 13.8.98 and the top of Andoya 27.3.03 but it can be seen in all plots. This has an obvious implication in the degree of agreement (tables 1 and 2) that should be modified when corrected.

Page 2886, Lines 1 to 20. It is mentioned in the text that according to Gordley et al 1996, orbital instruments yield lower columns than GB since there is a layer below 80 hPa not seen by the occultation technique spectrometers. The authors claim this is in agreement with their results, but according to figure 12, HALOE smoothed profiles yield larger, not lower, values than GB ones. The magnitude of the differences are about the same but of opposite sign.

Technical corrections

Page 2872. Noxon, 1975

There is something wrong with the section numbering: Page 2872 line 25. Section 3.2 does not exist. Page 2876, line 9 Sec. 5.4 does not exist (is section 11 in the manuscript), etc.

Page 2882. line 22. ŞA good agreement is observed since both instrumentsŤ is somewhat misunderstanding. It would be better to say Ş... between balloon and GB profile inversionŤ, since the profile obtained from slant GB are not direct measurements and

ACPD

4, S981–S983, 2004

a model and the a priori play a role.

Page 2883. line 8. <decreases> instead of <increases>

Page 2883. lines 21-23. The sentence introducing the figure 9, is not enough clear. The authors intend to show that the differences are seasonal dependent but they donŠt mention it until later in the manuscript. Explanation of the reason why the fig. 9 is introduced would clarify.

Page 2884, lines 10-14 and figure 10. From figure 10 it cannot be concluded that there is a seasonal dependence. In 1998 there are no data in spring. In 1999 relative differences are of the same order in spring and summer.

It is hard to see (at least in my printing from the pdf file) all light gray lines: Fig. 1. Curve NO2 natural variability Fig. 2. Curve 5 km. Fig. 9. Right plot. Summer curve. Fig. 11. right plots. Sunset curves.

Interactive comment on Atmos. Chem. Phys. Discuss., 4, 2867, 2004.