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Interactive comment on "Comparison of GOME tropospheric NO_2 columns with NO_2 profiles deduced from ground-based in situ measurements" *by* D. Schaub et al.

Anonymous Referee #1

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This is an excellent paper that fully warrants publication in ACP. The authors have taken on the very challenging task of comparing in-situ surface measurements with space-based measurements of NO2. Both the in-situ measurements and the space-based measurements are very well described. The authors have carefully addressed the major issues involved in the comparison including the NO2 vertical profile, spatial variability over the range of surface measurements, and contamination of the in-situ surface measurements with HNO3 and PAN. The manuscript further examines the role of clouds in the retrieval, and quantifies the measurement differences. This paper should make a strong contribution toward understanding the quality of space-based

measurements of tropospheric NO2 columns.

I do have a suggestion regarding a potential source of confusion in the discussion of the averaging kernel and measured profile information. For example in the abstract the second comparison is described as including averaging kernel information, which implies that the first comparison did not use averaging kernel information. However, the first comparison does include averaging kernel information such as the vertically resolved instrument sensitivity. It would seem clearer and more accurate to say that the second comparison replaced the NO2 profile assumed in the retrieval with a NO2 profile determined from in situ measurements. Similarly the last phrase of the abstract would be more clearly stated as "Ĕ and demonstrates the importance of using accurate NO2 profiles for (partially) clouded scenes." A clearer distinction between the "using averaging kernel information" and "using a measured profile" would improve a frequent source of confusion throughout the paper.

Line 10 of page 2197, is the NO2 in the upper troposphere really neglected? Line 25 of page 2207 implies that 45% of the signal measured by the space-borne instrument originates from above the PBL, implying possible errors in such an approach. Or do you simply use the TM4 model profile in the absence of in situ measurements? If the TM4 model profile is used, then the second comparison described in the abstract is not really independent of the a priori NO2 profile used in the GOME retrieval as described. Please clarify.

How is snow treated in the retrieval? Could transient snow cover be interpreted as a cloud by FRESCO? Some discussion on this issue would be useful given the mountainous terrain of this work. Section 4.3.2 might be a good place for this.

SPECIFIC COMMENTS: Line 25 of page 2191, the C-shape profile is for NOx, not NO2. NOx is mostly NO in the free troposphere.

Line 16 of page 2209, isn't the regional spring maxima found by Moxim et al. NOx rather than NO2? In fact, their analysis is for such a broad region that it doesn't seem

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very relevant for explaining the local observations enhanced NO2. Does TM4 show a local spring max in NO2?

Page 2216, NO2 columns $>$ 300 x 1015 molec cm-2. This seems to be too large for the AK approach to be useful. An earlier section states tau$<$0.005 is required.

Interactive comment on Atmos. Chem. Phys. Discuss., 6, 2189, 2006.