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Interactive comment on "NO_2 Profile Retrieval using airborne multi axis UV-visible skylight absorption measurements over central Europe" *by* M. Bruns et al.

Anonymous Referee #1

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This is an interesting paper showing how NO2 profiles can be derived from skylight measurements at a variety of angles and wavelengths. Its strength is that the method is first evaluated theoretically then applied to a real case, with reasonable results being derived. Thus the paper should, after some revision, be accepted to ACP.

Major comments: 1. Despite the careful theoretical evaluation of the '4-3' method, the reader is left completely in the dark as to why this particular choice of angles and wavelengths was made. We are told that '4-3' is better than other configurations, e.g 4-0, 2-3 or 10-0. However there is no justification of 4-3 per se. Would 5-4 give even better

retrievals? A discussion of why this configuration was chosen should be included, probably in the introduction, and this discussion should be a properly-argued paragraph not a throwaway sentence. 2. The results shown in fig 5 are impressive, but the text in section 5 is repetitive and superficial. The comparison with Sciamachy would be better presented as a Table - of course the two measurements will not be equal because of the footprint so there is no need to dwell on the point. The Table could also include some examples of clean profiles - these are as important as polluted ones for understanding NO2 distributions and may be more useful for identifying systematic errors in the measurements. 3. Surprisingly, no mention is made in the text of the fact that the four profiles in fig 5 are almost identical - certainly within the quoted error bars they are identical. It would be good to show some profiles that are different, if only to convince the reader that the algorithm works. More seriously, are they really suggesting that the NO2 concentration near a main road in an Alpine valley is the same as in the city of Bologna under conditions of light winds and a temperature inversion? Surely emissions in the city are larger than in the valley? Is there some self-limiting mechanism in the chemistry of NO2 that effectively sets a maximum concentration (I am not aware of any)? Are there ground-based measurements that support the retrievals (NO2 is extensively monitored all over Europe).

Minor and typographical comments: a) p.494 l.25 affecting not effecting b) p.495 l.5 omit 'being' c) p.495 l.19-20 'This new setup demonstrates significant improvements even compared to some setups using ten LOS...' d) p.495 l.21. Sentence beginning 'A different method' does not make sense. Suggest 'Wang et al (2004) used a different approach to retrieval of trace gas distributions to that used in this work. They used only the zenith and nadir LOS at three different wavelengths and required significant a priori information about the height of the ...' e) p.496 l.14. comma after spectrometers f) p.496, l.17. quartz fibre bundle g) p.496 l.20 The AMAXDOAS instrument simultaneously measures h) p.497 l.2 well known i) p.497, l.9 'both campaigns were chosen to cover latitudes from the Arctic to the tropics.' - rest of sentence is superfluous. j) p.498 l.15 'sensitivity of ... y to the variation ... in x.' k) p.498 l.19-20 SCITRAN actually calcu-

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lates l) p. 498 l.20. This paragraph uses two different definitions of weighting function and assumes that the reader will understand this. This in turn assumes that the paper will have a very narrow readership of specialists in DOAS retrievals, which hopefully will not be the case. This paragraph needs to be re-drafted and possibly extended to distinguish more clearly between the K matrix and the weighting functions WFI. On l.21 for instance we are told that 'weighting functions give the absolute change in intensity for a relative change of 100% of a parameter'. Intensity of what? In the context of IR transmission, for which Rodgers presented the theory that underpins this paper, the answer is obvious. In the context of a DOAS measurement it is not obvious at all, especially when the K matrix has already been defined as a weighting function. Equation 4 therefore appears from nowhere in the paper, and leads to nowhere. Delta VMR is defined as 'the difference in volume mixing ratio in each layer'. Difference from what? Is there a previous paper where this material is laid out properly? Perhaps then it would be better to refer the reader to that paper for details. In any case, this confusing section of the paper needs SUBSTANTIAL re-drafting. m) p. 500 l.22 correlation length n) fig 2 is much too small. There are far too many lines in 2a in particular (which is referred to in detail in the text). Given the use of colour later in the paper, this panel in particular should also use colour to distinguish the blur of similar lines. o) p.502 l.15 at 9 km p) p. 502 l.19-20 '... demonstrates that the sensitivity of the layer at 9 km of the retrieved profile is largest to changes in the true vertical profile at 9 km'. q) p. 503 l.19 '... measurements increases ...' r) p.504 l. 20 omit still s) p. 504 l.21 'covers part of the Rhine....major highways' (lower case h, also on line 23) t) p. 504 l.27 coincides with u) p. 504 l.29 reference to Pundt et al only needed once v) p. 506 l.26 you have presented a theoretical study not a sensitivity study since you did not consider other possible configurations. w) Fig 1 caption needs clarifying: 'Results are presented in this paper from the section of track between the black dots'

Interactive comment on Atmos. Chem. Phys. Discuss., 6, 493, 2006.