

***Interactive comment on “Tropospheric NO<sub>2</sub> column densities deduced from zenith-sky DOAS measurements in Shanghai, China, and their application to satellite validation” by D. Chen et al.***

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A.

General comment:

This manuscript is mostly thorough and the subject matter important, though one could debate its degree of originality in scientific as opposed to technical content. Although it is clearly deserving of publication after revision, the authors should consider whether it might be better placed with the new EGU online atmospheric journal for work of a technical nature.

B.

Specific comments:

1. The error estimates in Section 3.1.4 contain assertions of small errors at the end of each subsection, but there are few details to show how they are derived. In subsection 3, there are no details at all. Such errors are fundamental to the main thrust of the paper, and for a manuscript otherwise so full of technical detail they are strange omissions. We are also not told the typical error in PBL height in Section 3.2, incongruous because the effect of an error in PBL height is later explored, but without the reader knowing whether the values used are representative.

2. Surely, the positive intercepts in Figs7 & 9 are unphysical? When there is zero NO<sub>2</sub> at the surface, we should expect the amount in the free troposphere to give an amount in the zenith-sky view, resulting in a negative intercept as these graphs are cast. Or have I misunderstood something? In any case, the fact of intercepts at all is worthy of comment in the text.

3. The manuscript is long and sometimes repetitive. Most sections have an introduction which adds little to the sub-section headings, and could easily be cut. Elsewhere, detailed cuts can easily be made, two examples picked from p167373 are:

(a) line16: cut <over the urban site> from the sentence <Since our experimental site suffers from heavy traffic pollution, . . . >

(b) line26: shorten <The light data are . . . (Nighttime lights are for the year 2003).> to <The light data are . . . for 2003.>

4. p16714 line26 seems to assert that tropospheric NO<sub>2</sub> contributes to radiative forcing. This would be astounding if true and should be backed up with references. If the authors mean it contributes locally via its effect on ozone, then it cannot be true locally in the boundary layer, because changes in composition there occur at almost the same temperature as the surface, so there is little change in net upwelling radia-

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tion. To achieve significant radiative forcing, changes in composition must occur at a temperature significantly different to the surface, e.g. in the upper troposphere.

5. The assertion in Section 4.1 that high thin clouds can decrease tropospheric absorption is counter-intuitive, the authors should provide some explanation and a reference.

6. Section 4.1.2 ignores changes in PBL height during the course of the day. Although sunrise and sunset can often be similar, their difference from early pm can easily be 100%.

7. What is meant by orthogonal regression in Fig13 caption? Is this some special form of regression?

C.

Technical corrections:

p16715 line21: replace NDSC by NDACC

p16728 line6: replace and by to

p16731 line15: replace AMF by AMFs

Fig6 caption: they are comparisons, not groups of comparisons

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