

Interactive comment on “Determination of an effective trace gas mixing height by differential optical absorption spectroscopy (DOAS)” by B. Zhou et al.

Anonymous Referee #2

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The paper by Zhou et al. describes a method to derive the mixing layer height in the lower atmosphere by combining NO₂ measurements from active and passive DOAS observations. In general, this idea has the potential to be published in AMT. But similar to referee #1 I have serious concerns about the assumptions used for the retrieval and the missing details in the manuscript. When reading the Chen et al. paper in ACP on the same data set I'm sceptical that this manuscript presents sufficient material to merit another publication.

Specific comments:

- Introduction: The paragraph on the estimation of the mixing height (MH) is rather

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weak. Please give information on uncertainties in the estimation of the MH using other data sets. This is in particular needed since the authors compare their results to those data (Yang et al.).

- Experimental: Sorry, but this section is a kind of an impertinence. At least basic parameters for the instruments used in this study are needed. Of course, one can find them in Chen et al., but for the potential reader of this journal it is necessary to have them here.

- Calculation of the effective trace gas mixing height: The instrument is located near the middle circle of Shanghai. One should expect very high local pollution from traffic in particular during rush hours. Therefore I'm more than sceptical that the assumed profile with constant concentrations within the boundary layer is realistic. The authors themselves point out that at least a constant mixing ratio is more supposable. But then they should give a rough estimation on the possible error using the wrong assumption and not just stating, that resulting errors are small.

- Results: How the monthly averages in Figure 4 are calculated? Are all values included? Taking into account the standard variations it is hard to conclude that the highest MH values occur in summer. How the errors for the Yang data are calculated? Why are the error bars missing for some months? High values of MH in January: The lifetime of NO_x is also higher in other winter months like e.g. December. Why one should have these exceptional high values of the calculated mixing height only in January.

- Discussion (Correlation between the ETMH and surface temperature): The whole argumentation is not very conclusive. Which months have been averaged for the values given in tables 2 and 3?

- Discussion (Correlation between the ETMH and surface wind speed): Correlations are always quite small. Not sure that one can draw any conclusions from that. By the way: where the meteorological data come from? How representative are these data

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for the DOAS site?

- Discussion (Comparison with other results) What is the reason that the authors have not compared their results to those obtained from the MESSy model, which has been used in the Chen paper? Error bars in Figure 11?

Further corrections/comments:

- Tables 2 and 3: Please combine tables, indicate which months have been averaged.
- Table 4: Please add this information to Figure 11 and remove table.
- Figure 2: Needless.
- Figure 3 has already published in Chen et al. (Figure 13)! Please indicate this and the date of the measurements.
- Figures 7 and 8: Confusing. What are the different colors/symbols? Information content for both figures quite small.

Interactive comment on Atmos. Meas. Tech. Discuss., 2, 1663, 2009.

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