

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

Anonymous Referee #1

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Comments on “Determination of an effective trace gas mixing height by differential absorption spectroscopy (DOAS), by B. Zhou et al.

The idea promoted by this paper is that of a derivation of the mean height of the planetary boundary layer from a combination of DOAS passive and active NO₂ measurements. The capacity of the method is illustrated from a comparison of height derived from DOAS measurements in the city of Shanghai and meteorological information.

General comments The reviewer appreciates the innovative idea, which might be potentially useful for air quality modelling and thus could deserve publication in AMT. But the manuscript is not really convincing because of the complete lack of information on

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the measurements themselves and the relatively poor analysis of the data. Here are comments which might hopefully help the authors to improve the manuscript.

Missing information a) The relevance of the assumption that the NO₂ concentration (or mixing ratio) is constant within the PBL and equal to that measured at the surface strongly depends on where and how the measurements are made. This assumption will be totally wrong if the active measurements were made next to a local source, e.g. at street level. However, there is no information at all on where and how these measurements are made: instruments? location in the city?, height above street level? etc.. b) The same for PBL height by meteorology, where? By which method?

Specific comments

Experimental. Description of measurements totally missing. Fig 1 and 2 very little informative. Could be easily removed.

Calculation of effective trace gas missing height Fig.3. On which day / season these measurements have been made? How can you say from these that the analysis confirms your basic assumptions? 0.3-0.8 km seems to be very small for a mixing layer. In the text you are saying 1-2 km and in the conclusion 0.1 to 2.8 km. Discussion of error: this is just an affirmation. There is nothing to support this.

Results The data are said selected for cloud cover using O₄. I have no access to the paper of Chen et al. What is the meaning of “mainly” cloud free? It could be good to show some statistics on cloud information for selected days. Description of Figures missing in the text. Fig 4. Comparison of DOAS and met derived mixing layer height. Since DOAS measurements can be performed during daytime only why not using Met height at 8:00 and 14 h only? Would be useful to show also the monthly mean NO₂ passive column and surface concentration to see how much the ETMH could be sensitive to pollution. Fig 5. What is the time ? Solar? Standard? How is the NO₂ diurnal photochemical cycle taken into account? It might be different at surface level (close to emissions) than higher in the PBL. The photochemistry should be far less

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active in January. The only data you have in the summer are the 7 days of September. The height decrease after 14h seems surprising compared to other month. Is there a reason for that? How it compares with met information, including cloud cover. Would it be possible to show the MH height diurnal variation in the winter in a similar format?

Discussion

Correlation with surface temperature Fig 6. Date missing in the caption. This is a case of very large amplitude of temperature diurnal cycle. Would be interesting to see the same for the smallest T amplitude during the same month (may be cloudy) and during a colder period in December. Fig 7: meaning of various symbols and colours? The mean R in May is about 0.60, meaning that the 0.922 of Fig 6 is an extremely good example. It is said that there are too little data in March (4), June (3) and October (8) for making statistics. In that case what about the 7 days of September?

Correlation with wind speed Fig 7: same comments as for Fig.6 What is the meaning of anti-correlation? Large height in the absence of wind? small height in strong wind maritime air? Other? Last paragraph: discussion. Cloud cover has a strong impact on convection as well as on NO_x photochemistry. You said that you are using O₄ for deriving it. But which threshold is used? Are all the data used here really cloud free? It would be helpful to see the plots of the periods quoted in text, together with met info: temperature, wind speed and cloud cover, + passive and active DOAS measurements.

Influence of atmospheric lifetime. What are the polluted regions? Beijing-Nanjing? Reference? Not only long distance but also local pollution could play a significant role in the MH derivation. Is there an indication of strong local pollution? Fig 9. It would be very informative to plot also the passive and active DOAS NO₂ measurements.

Comparison with Met MH The comparison between the 15 yr mean MH diurnal variation and those derived from a few days each month in 2007 (excluding summer) is not very convincing. Better show this for the same days, or at least the same months in Fig 5.

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Conclusions Since the ETMH is said to vary between 0.1 and 2.8 km, it could be good to show an example of measurements (including passive and active DOAS NO₂ column and concentration) presumably in the summer, to see how the DOAS derived ETMH compares with meteorological information. A yearly average is not very demonstrative. Since the winter season is shown to be uncertain because of the little actinic activity and the long distance transport of pollution from other areas, why not late spring/summer/ early fall observations only. A correlation of 0.37 with wind speed has very little meaning. Before speaking about other species, I could recommend to demonstrate better how the idea applies to NO₂.

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