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Interactive comment on “Long-term MAX-DOAS network observations of NO₂ in Russia and Asia (MADRAS) during 2007–2012: instrumentation, elucidation of climatology, and comparisons with OMI satellite observations and global model simulations” by Y. Kanaya et al.

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Received and published: 21 June 2014

We appreciate the comments given by the reviewer after careful reading of our manuscript.

Comment 1: Was there an intercomparison with all instruments part of MADRAS at the same site? Even the instruments are quite similar some important parts are not, which

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might lead to systematic differences. For section 2.1 I prefer to have in addition a table illustrating more clearly the differences between the setups instead of a lengthy and difficult to read description. A similar comment to section 2.2: Please add information on site characteristics to Table 1.

Reply 1. We have run two instruments together at the same site. The typical difference in the derived NO₂ DSCDs was <5%, indicating that the instrument-specific difference was small. We will classify the instruments into three types (type A, B, and C), according to the deployed location of the spectrometer and temperature settings (indoors (in a refrigerator, in a temperature-stabilized box) and outdoors in a light-receiving unit) and explain them in text (this part will be moved to section 2.2). The type of each instrument will be shown in Table 1. Table 1 will also include types of the observational sites (e.g., urban, suburban, rural and remote). In section 2.2 of the revised text, only brief information of the observational sites will be included.

Comment 2: Please avoid as much acronyms as possible. Since the text is already quite technical, in particular unusual acronyms like EL (for elevation angle!) and also TropoNO₂VCD (why not just name it “tropospheric NO₂ column”?) do not improve the readability.

Reply 2. Upon suggestion we will use elevation angle instead of EL. After consideration, we retain the acronym TropoNO₂VCD, used more than 50 times in the text and in axis titles in many figures, believing that the acronym leads to conciseness of the manuscript.

Comment 3: Comparison between MAX-DOAS and satellite data: Please refer in your discussion on pp 2901 and 2902 to the study by Leitao et al., (Leitão, J., Richter, A., Vrekoussis, M., Kokhanovsky, A., Zhang, Q. J., Beekmann, M., and Burrows, J. P.: On the improvement of NO₂ satellite retrievals – aerosol impact on the airmass factors, *Atmos. Meas. Tech.*, 3, 475–493, 2010) where in detail the impact of aerosols on the satellite observations has been investigated. P. 2902, l21: Please remove or rephrase

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the statement: “Future satellite observations with smaller footprint sizes will improve the analysis.” This would be the case, if spatial inhomogeneity is the major reason for the underestimation by satellite instruments. But the outcome of this (and other) study (ies) is quite clear, that this is not the case.

Reply 3. Thank you for the important suggestion. In the revised manuscript we will mention that Leitão et al. (2010) theoretically demonstrated that such underestimation by satellite observations could occur when the aerosol layer extends to relatively higher altitudes than NO₂. The sentence regarding future satellite observations will be deleted here. Instead in the summary section, we will mention that future satellite observations with smaller footprint sizes may help distinguishing the causes.

Comment 4: Figure 12: This plot is too busy. Please select either months representative for the different seasons or use seasonal averages (e.g. December to February for winter like in Figure 17).

Reply 4. We will leave data on selected months (Jan, Feb, Apr, Jul, Oct, Nov, and Dec) to improve visibility while highlighting the behavior in winter (mentioned in text).

We thank the reviewer for the comment helping us to improve our manuscript.

Interactive comment on *Atmos. Chem. Phys. Discuss.*, 14, 2883, 2014.

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