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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.

Anonymous Referee #1

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This is a well-written description of the MADRAS network of MAX-DOAS instruments in Russia and Asia and a thoughtful analysis of the data with respect to the comparison to satellite data. The science component (i.e., what new insight we've gained) is weak, which is common for most validation studies.

I recommend publication after major revisions. My comments follow in no particular order.

Abstract: Overall, the abstract is the worst part of an otherwise great manuscript. I probably wouldn't have read this paper based on the poor abstract. The authors are doing themselves a disservice.

1)(line 41): I doubt many will know what an "aerosol shield effect" is. Really you are just saying that aerosols weren't adequately accounted for in the radiative transfer calculation. Please consider rewording this sentence.

2)I would make it clear that this is the first study with MAX-DOAS in Russia and Asia.

3)(line 45): "The prevailing . . ." This sentence doesn't say anything. You know that the sites are located in urban settings.

4)(line 46): "The presence . . ." This sentence is not clear at all. Why "diesel vehicles" and what "other sources"?

Section 2.3 Retrieval algorithms & Summary

As you discuss later, some of the discrepancies between the data from MAX-DOAS and OMI are associated with different assumptions used in the two OMI and MAX-DOAS retrieval algorithms. Could you describe the two OMI retrieval algorithms, say in a new Section 2.4, and highlight the main differences between the two OMI algorithms and the algorithm used for the MAX-DOAS instruments? You could also comment on the limitations of satellite data, such as spatial coverage, which should be accounted for when comparing to surface observations.

Section 3.1

It is easy to get a great correlation between OMI and MAX-DOAS since they both will reproduce the seasonal cycle in NO2, which is associated with its chemical lifetime. I recommend deseasonalizing the data for a fairer assessment of how well the MAX-DOAS and OMI data agree.

Line 465: This paragraph is very unclear.

Line 473: Describe how "the effect is already taken into account more adequately."

Line 485: Huh? How are the vertical profiles treated differently in the retrieval algorithms?

Figure 3. Why is OMI NO2 much higher over the ocean near Japan than over Japan's cities?

Figure 7. Why are the error bars on the MAX-DOAS data so much larger than those on the OMI data? It seems from your conclusions that the error bars on the OMI data should be much larger.