Response to anonymous referee #3

We would like to acknowledge the referee for their helpful and thorough review. We believe that their comments improved the quality of this work.

Some of the statistics presented in the study have changed as a result of the error weighted linear fitting applied to the collocation data sets.

Our responses (in blue) follow the reviewer's comments (in black italics).

Specific comments:

Page 4, line11. I can not agree with the author that "the MAX-DOAS observations of tropospheric NO_2 were quite sufficient to be used for the satellite data validation". The total coincident measurements were only 22 for OMI, 13 for GOME-2A, and 28 for GOME-2B (found in Table 2). I suggest this paper should not be presented as a satellite validation work, which will need some relative long-term measurements and more work (at least more than one year, without large data gap). I suggest author rephrase relevant parts in the paper, and be concerned with the use of the term -- "satellite validation".

The manuscript has been revised accordingly.

Page 4, line 12. Here it is mentioned the lowest measurement was made with 2°, but why it is not used? On page 6, line 16, it is mentioned that NO_2 was retrieved from 15° and 30° measurements.

Since the tropospheric columns of NO_2 were not derived from profile retrievals, we chose to use the elevation angles of 15° and 30° which are not significantly affected from aerosol. When AMF look-up tables are used for the MAX-DOAS retrievals, uncertainties can be introduced in the VCDs calculated from the observations performed at lower elevation angles, due to the presence of aerosol.

Page 6, line 18. Please specify which residual is referred to here. Spectral fitting residual, or maybe NO_2 dSCDs residual, or something else?

The spectral fitting residual is mentioned in that sentence. The text has been revised.

Page 7, line 13-14. Can you find some evidence to support this idea? If we use simple geometry to estimate a coarse horizontal sampling distance of MAX-DOAS instrument (e.g., 15° elevation, 1 km NO_2 layer height, as mentioned in your paper), you may find the MAX-DOAS is sampling air mass very close to the site. This makes me feel puzzled, because why GOME-2B (with the largest footprint) could "luckily" measured the days have NO_2 smoothly distributed in a larger area. Using the reference criteria group (Table 2), you have 23 coincident measurements with GOME-2B, which doubled the numbers for GOME-2A and OMI. So, maybe this good agreement is not simply due to "luck".

No evidence could be found to support this statement. The better agreement can be partly attributed to the lower NO_2 observed by MAX-DOAS in combination with the larger collocation data set compared to GOME-2A, which improves the metrics of the comparison. The manuscript has been revised.

Page 7, line 19. Since the error bars from both MAX-DOAS and satellite data are not small, can you include error weighted fitting? I am wondering if this could improve the results (or show more insightful detials).

Only the error bars of OMI were representing the measurement error. The GOME-2A, GOME-2B and MAX-DOAS error bars were representing the standard deviation of the average value. In the revised manuscript the error bars represent the measurement error in all cases. Thus, the MAX-DOAS errors are quite smaller now. Error weighted fitting has been applied to the collocation data sets and the corresponding figures and statistics have been changed.

Page 8, line 23, Table 2. I found by tightening CF criteria, you have improved r for both OMI and GOME-2A, but (slightly) decreasing trend for GOME-2B. I do not think this is due to over strengthening the criteria, since with CF ≤ 0.2, you still have 15 coincident measurements for GOME-2B, which is about twice the number of coincident measurements from OMI and GOME- 2A with CF ≤ 0.2. Do you have any comments on this?

This is probably due to the relatively high variability of the data pairs, which leads to quite wide 95% confidence interval (0.22-0.87) in case of GOME-2B when more stringent CF limit is used. Revisions have been made in the manuscript and the statistical significance of the comparisons is discussed. Also, more statistics are presented in Table 2.

Table 2. Another point is the CF tightening lowered the bias for the OMI and GOME-2B, but (slightly) increased the bias for GOME-2A. Any comments? With small sample size, the changing of bias and/or r should be carefully quantified, before any meaningful conclusion can be made. So, I suggest, for example, maybe calculate the confidence interval for r and bias.

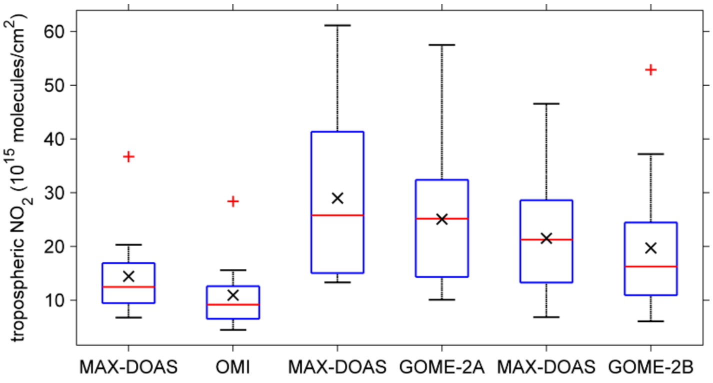
95% confidence intervals have been calculated for both r and bias and for each scenario of the coincidence criteria under investigation. All the 95% CI are reported in Table 2. The confidence range for GOME-2A bias is quite wider compared to those calculated for the other two satellite sensors.

Table 2. The CF criteria part is nicely visualised in Figure 9. Maybe you could perform the same visualisation for the radius and averaging time criteria. Few important information might be overlooked in such large and busy table. For example, why tightening radius criteria led a large jump in GOME-2B mean bias (from -1.8e15 to 4.6e15 molec/cm2)? Any comments?

This large jump cannot be easily explained. Considering the high r value and the quite larger than unity slope value (1.18), GOME-2B seems to overestimate NO_2 columns for high ground-based NO_2 observations. However, the 95% confidence range is quite wider compared to those estimated for the OMI and GOME-2A. Bar plots for all collocation criteria have been included in Figure 9.

Page 11, line 5-8. Please provide more evidence (number) to support this idea. Maybe box-and- whisker plots of satellite and ground-based NO_2 can show/support that GOME-2B really sampled low NO_2 loading condition. It is hard to conclude this by only looking at Figure 6.

The text has been revised so that it is clear that lower NO_2 levels are observed by MAX-DOAS during the GOME-2B overpass time on GOME-2B collocation days compared to those measured around the same time on GOME-2A and OMI collocation days. Figure 6 can support this statement, because both MAX-DOAS hourly averages and standard deviations around GOME-2B overpass time are lower on GOME-2B days compared to those corresponding to GOME-2A and OMI overpass days. In the following figure box-and-whisker plots are presented. Each satellite box-and-whisker follows the corresponding MAX-DOAS derived. Both GOME-2B and MAX-DOAS observations are lower on GOME-2B collocation days compared to GOME-2A and MAX-DOAS measurements on GOME-2A collocation days. However, this figure is not included in the revised manuscript.



General comments:

The sample size in this study might be not good enough for satellite validation; maybe it is to sparse to draw a solid conclusion on the effects of the criteria studied here. However, even if you found the sample size is too small to provide critical results by the end, the process of drawing this conclusion is still valuable. I suggest author spend more time in tightening the conclusions made in the manuscript, and I think this could be a valuable paper.

The manuscript has been revised accordingly and more statistics have been included and discussed.

Technical corrections:

Page 4, line 23. The I0 effect. The subscript should be zero, not "o". Please also correct the ones used in Table 1.

The subscript has been corrected.

Figure 4. dAMFs is not defined in anywhere. Give unit for the x-axis.

Revisions have been made.

Figure 5. Apparently, Guangzhou is not the only NO_2 hotspot on the figures. So, maybe adding location symbol for Guangzhou and a map scale will help the reader to find the city. Please do not use letter x for a multiplication sign.

Changes have been made on the figure and its caption.

Figure 9. Give unit (percentage sign) for the top panel y-axis.

The absolute differences are presented now on the figure instead of the % differences.

Page 12, line 15. Check all your references. Use consistent abbreviations for journals.

References have been checked.

Page 15, line 2. A comma is missing after "Atmospheric Research". And please check if the format of page numbers is correct.

Corrections have been made.

Page 16, line 29. This paper has been published on AMT; please do not cite the AMTD version.

The AMTD version has been replaced with the final version.