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Atmospheric Measurement Techniques Discussions

Interactive comment on "First quantitative bias estimates for tropospheric NO_2 columns retrieved from SCIAMACHY, OMI, and GOME-2 using a common standard" *by* H. Irie et al.

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Reply to anonymous referee 2

We thank the reviewer very much for reading our paper carefully and giving us valuable comments. Detailed responses to the comments are given below.

Comment 1: The bias estimate presented in this manuscript implicitly assumes that possible biases in MAX-DOAS and spaceborne tropospheric NO2 column retrievals (i.e. biases with respect to the truth) are constant over time, or have the same temporal dependence. This is not necessarily the case. Both MAX-DOAS and satellite

observations are sensitive to the vertical distribution of NO2 (and aerosols). Boundary layers (which give a reasonable first order profile estimate) tend to be lower in the morning than in the afternoon. In a ground-based versus satellite comparison three NO2 profiles (and three aerosol profiles) play a role: (A) the NO2 profile assumed in the satellite retrieval, (B) the NO2 profile retrieved by MAX-DOAS, (C) the true NO2 profile, which is generally not known. The difference between the tropospheric NO2 column (tVC) retrieved by MAX-DOAS (or satellite) and the real tropospheric NO2 column (tVC true) is related to the difference between B (or A) and C. It is not self-evident that tVC(MAX-DOAS)-tVC(true) and tVC(satellite)-tVC(true) both are constant for all C, or similarly, that tVC(MAX-DOAS)-tVC(satellite) is independent of C. This implies (given the diurnal and seasonal cycle in the boundary layer height and therefore in C) that it is not unrealistic to expect a temporal dependence of the systematic bias between satellite observation and MAX-DOAS. I would like to see this point discussed in the manuscript and see how the reader can be convinced that the possible temporal dependence of the bias between satellite and MAX-DOAS is small compared to the bias between the satellites, as reported in the current version of the manuscript.

Reply: We agree with the reviewer that there is the possibility that biases in MAX-DOAS and space-borne tropospheric NO2 column retrievals are not necessarily constant over time. To address this issue, however, we think that the precise validation for MAX-DOAS retrievals and/or more systematic MAX-DOAS observations would be essential. While these require significant additional efforts (e.g., simultaneous MAX-DOAS observations with well-organized independent NO2 VCD observations), the present work focuses on estimating a representative number for the bias between satellite and MAX-DOAS data. This is now mentioned in the revised manuscript.

Comment 2: In my opinion the article is quite short. This may be considered a strength. However, given the fact that this study is performed with a unique data set which combines long-term ground based observations from multiple stations with spaceborne observations from three different sensors, it is quite remarkable how many potentially

interesting results are not shown in this manuscript. To mention a few possibilities (P1/P2/P3):

Reply: Following the reviewer's comment, we have added some results as mentioned below.

(P1):It would for example be interesting to show more quantitative characteristics of the four data sets compared (NO2 VCD from MAX-DOAS and three satellite instruments), for example a table with, for each measurement site, the 25/50/75 percentiles of the trop. NO2 columns measured by satellite instruments (e.g. for x=0.2 degr.) and MAX-DOAS. Please also add this information with respect to the AOD (only for MAX-DOAS and only for that part of the data which is actually selected for the NO2 comparisons, i.e. no AOD's under cloudy conditions, or in the late afternoon, etc.).

Reply: As suggested by the reviewer, we have added Tables 2-4 to give information about quantitative characteristics of NO2 VCD and AOD for each measurement site.

(P2): It would be interesting to put more effort into analyzing the results over Japan (which are essentially ignored in this study), for example in one or more case studies. Is the comparison between MAX-DOAS and satellite more in agreement with the Chinese case if the temporal fluctuation of the NO2 VCD is low, and if in addition wind speeds are not too low? (the latter could give an indication of an NO2 field that is homogeneous over a larger spatial domain)

Reply: We also hoped that we could make use of data from the Tokyo case. We made an additional analysis and found that for the Tokyo case the representative spatial scale, on which we could regard the NO2 distribution as being homogeneous, is smaller than our smallest latitude/longitude grid size ($\sim$0.05°) or the OMI pixel size (see Figure 6 of the revised manuscript). We agree with the reviewer that it would be worthwhile to do case studies with a consideration of the temporal fluctuation of the NO2 VCD. However, independent observations supporting this within the latitude/longitude grid we are investigating are unavailable. We think that this should be addressed not by

wind data but by independent NO2 observations, because factors other than the wind can play significant roles in determining the NO2 distribution. We realize, however, that completely ignoring the Tokyo case was not a good idea, as pointed out by the reviewer. So, the revised manuscript now includes the results from the Tokyo case in the discussion of the bias estimates, where the Tokyo case is used to support the results for the China case.

(P3): It is well known that aerosols have a big impact on MAX-DOAS observations (for this reason an aerosol retrieval is performed prior to the NO2 retrieval) and also that they affect NO2 observations from space. It is not unthinkable that aerosols introduce systematic errors, because the variety of possible aerosol conditions is large and because random fluctuations may introduce systematic effects. For this reason it would be interesting to report the bias not only as in the current version of the manuscript (namely, based on the entire Chinese data set) but also split up between conditions with low aerosol optical depth (e.g. below 0.4) and conditions with high AOD (e.g. above 0.4).

Reply: The aerosol impact is of interest to us too. We made additional analysis for low (AOD<0.8) and high (AOD>0.8) aerosol conditions, where the AOD threshold of 0.8 was taken from the statistics for the Chinese data. We found that differences between slopes (and therefore biases) for both conditions were similar. Since significantly more effort would be needed to extract aerosol effects and to understand how such aerosols effects could appear in MAX-DOAS and satellite products, we decided not to discuss it in this paper.

Comment 3: P.3959,l.20-22: "Considering this ... various conditions." The bias reported in this study is essentially the slope resulting from the regression analysis, where the intercept is forced to zero. Please explain why not to report both a slope and an intercept? Please also explain the implications of this approach. Do the reported biases equally apply to high and to low NO2 VCD values? If not: to which range of NO2 VCD values do the biases apply?

Reply: According to the reviewer's comment, we have added a quantitative discussion about the intercept in the revised manuscript. We thought that as a first approximation the reported biases were valid over the entire NO2 VCD range analyzed in the present study. Upon this reviewer's comment, however, we now realize that the validity of the reported biases should consider the error quoted for the satellite retrievals. As stated in the text, the error in the satellite tropospheric NO2 VCD retrieval can be expressed as $\sim$1x10^{15} molecules cm^{-2} + 30%. Thus, at least for a NO2 VCD less than $\sim$1x10^{15} molecules cm^{-2}, a significant systematic error, in addition to our estimated bias (less than 10%), is anticipated. So, our estimated biases would be invalid at VCDs less than $\sim$1x10^{15} molecules cm^{-2}. This is now stated in the revised manuscript.

Comment 4: P.3957,l.11-12 "Comparisons are ... than 20%" and P.3960, l.2-3 "We test ... and 1.00": Please explain in more detail how these two constraints (cloud fraction and spatial region) are combined: -Is a pair of simultaneous MAX-DOAS/satellite observations included in the comparison if the cloud fraction 'according to the satellite cloud product' is below 0.2, or is cloud filtering applied to MAX-DOAS as well? -For a given coincidence criterion x, which pixels within the spatial domain are included: only those with cloud fraction below 0.2 (even if, for instance, this applies to only one pixel in the entire domain)? or is a minimum fraction of pixels within the spatial domain defined which should have cloud fraction below 0.2? Please specify and also explain if different choices are made for the three different satellite instruments.

Reply: We first use the cloud fraction according to the satellite cloud product to select cloud-free satellite NO2 VCD data. For this, we use a cloud fraction threshold of 20%. For the selected data, a given coincidence criterion is applied to further select the satellite NO2 VCD data. Thus, comparisons are made only when both criteria are satisfied simultaneously. The same procedure has been taken for all three satellite instruments.

Interactive comment on Atmos. Meas. Tech. Discuss., 5, 3953, 2012.