

Interactive comment on “First quantitative bias estimates for tropospheric NO₂ columns retrieved from SCIAMACHY, OMI, and GOME-2 using a common standard” by H. Irie et al.

Anonymous Referee #2

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This manuscript presents an estimate of the bias between tropospheric NO₂ columns measured by the satellite instruments SCIAMACHY, OMI and GOME-2. For the first time such an estimate is derived using independent ground-based observations (MAX-DOAS). This enables comparison between the different satellite products, despite differences in overpass time (up to several hours). Comparisons such as these are relevant for applications (model validation, data assimilation, emission estimates) making use of tropospheric NO₂ columns measured by multiple spaceborne sensors. This work is also unique because of the large amount of ground based observations available and because of the diversity of the measurement locations (urban/suburban/rural).

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I recommend to publish this work in AMT, after the following comments have been addressed:

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.

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):

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(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.).

(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)

(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).

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?

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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.

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

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