Reply to comments of reviewer #2 on the manuscript "Operational total and tropospheric NO_2 column retrieval for GOME-2" by P. Valks et al.

We would like to thank the reviewer for his helpful comments and suggestions. In the following, we will reply to them point by point, including the reviewer's text in italic.

Page 1620, line 26: the authors state that GOME provides global NO2 on a daily basis, while in fact GOME observes NO2 with a global coverage every 3 days and GOME delivers the data to the ground every orbit. Please clarify this sentence.
We have improved the description of the spatial/temporal coverage of GOME/ERS-2.

Page 1622, line 19: I suggest to put "level 0" in brackets behind "MetOp" in this line, to clarify the process.
Done.

Page 1624, line 1: Why is a cubic polynomial chosen and not a higher-order polynomial ?
Thanks for pointing this out. We noticed that the description on the polynomial is incorrect: a third-order polynomial is used in the GDP 4.4. We have corrected this mistake in the paper. Sensitivity studies show that NO_2 is not very sensitive to the degree of the polynomial. Changes in the degree of the polynomial can introduce changes of a few percent at maximum in the NO_2 background level. However, we did found better fit residuals using a third-order polynomial over some regions (e.g. deserts).

Page 1627, line 23: The use of the cosine of the latitude implies that the surface area is important for the relative value given to NO2 in this climatology. The NO2 derived at the poles in the climatology is not counting at all. Please explain this.
The cosine of the latitude is indeed used to take the surface area into account when interpolating between the centre latitudes of the 16 bands. However, the most poleward latitude zones of this climatology are 65-85ºN / 65-85ºS, so the poles itself are not covered. In the GDP, the NO_2 profiles for these latitude zones are used for GOME-2 measurements with latitudes larger than 85º as well.

Page 1634, line 18-20: Please add a discussion why NO2 profiles would differ for cloudy and adjacent cloud-free scenes.
There are several reasons why the NO_2 profile could be different in cloudy and clear scenes:
- photolysis rates will be different within and below clouds, leading to changes in NO / NO_2 ratio as well as OH which determines NO_2 removal. Both effects tend to increase NO_2 below a cloud relative to a similar pixel without cloud.
- convective clouds are linked to vertical movement of air with updrafts in the centre of convection and downward movement of air at the sides of convective clouds. Over polluted regions, this can lead to significant changes of the vertical distribution of NO_2.
- in some clouds there is lightning which changes the vertical profile of NO_2 by adding additional NO_2 in the middle and upper part of the cloud.

It is difficult to judge the quantitative effects of all this, in particular as current global models do not resolve these processes and even regional models often use parameterisations of clouds, convection and lightning. However, it is fair to say that the vertical NO_2 profile in cloudy pixels is not necessarily the same as in adjacent clear pixels and therefore the uncertainties are also higher. We have added this discussion to the manuscript.

Page 1638, line 4: The authors mention a seasonal variations in Figure 7. However, for GOME the variations are varying from daily to annual. I suggest to simply describe them as temporal variations.
We agree. The spectral structures vary from daily to annual. We have changed the sentence in the paper and describe them as temporal variations with a seasonal component.

Page 1641, line 16-23 and Figure 9: Striking in Figure 9 are also the enhanced errors along the ship tracks in the Indian Ocean and high errors along coast lines. These are not mentioned in the text, but seem worth some discussion.
We agree that the discussion on Figure 9 is rather short, and this is extended in the updated manuscript. The effect of the cloud fraction uncertainty is generally larger for areas with enhanced tropospheric NO_2 at low altitudes and small surface albedo (i.e. areas with small AMFs). This is the case for the ship tracks and the coastal areas, where the AMF is relatively small compared to the cleaner oceans. There is strong (negative) correlation between the uncertainty in the AMF (Fig. 9 in the paper) and the AMF itself (Fig. 2 in the paper). This is clearly visible in the figure below, in which the colorscale of Fig. 2 has been adjusted to highlight local AMF differences at the low and mid-latitude region.

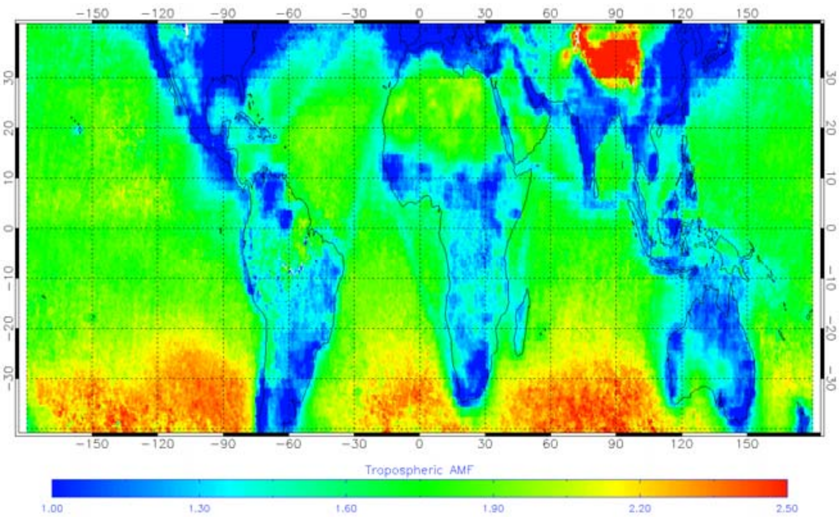

Fig. 1 Monthly averaged tropospheric AMF for March 2008.

Page 1644, line 18: The reference of Boersma et al., 2007 mentioned here for the TEMIS NO2 product from SCIAMACHY is actually a paper about NO2 retrieval from OMI. Please, either add OMI to the list of other satellite data sets or change the reference. Citation instructions are mentioned on the TEMIS website.
The reference for SCIAMACHY TEMIS product has been corrected with Boersma et al., 2004, following citation instruction on the TEMIS website.

Page 1645, line 18: Please mention the version number of the SCIAMACHY data product.
The SCIAMACHY product version number (v 1.10) has been added.

Page 1645, line 20: Are the different angular dependencies solely caused by the time differences of the 3 instruments ?
There are dependencies related to the solar zenith angle (and thus related to the time differences) but also differences in the viewing angles of each instrument (differences in swath and nominal scanning angle).