Reply to the interactive comment of reviewer #2 on "Influence of under-sampled a priori data on tropospheric NO 2 satellite retrievals" by A. Heckel et al.

We would like to thank the reviewer for the important and helpful comments. We will address the reviewer's suggestions in detail below.

This manuscript addresses the important problem of low-resolution a priori data used in most NO2 satellite retrievals today. Using such low-resolution data for a priori NO2 profiles, surface reflectance and other parameters relevant for the computation of air mass factors leads to an under-sampling of the true variability of these parameters at the comparatively high resolution of satellite pixels from instruments such as SCIAMACHY or OMI and, as a consequence, to systematic retrieval errors.

The issue is addressed here based on a case study for a sunny day in August 2005 over a coastal area with variable topography and emission levels (San Francisco area in California, US) providing the necessary challenge for such an investigation. High-resolution input data sets are obtained from MODIS observations (for surface reflectance and aerosol distribution) and using the WRF-Chem chemistry and transport model for a priori NO2 profiles.

The study is an important contribution to the topic and although it focuses on a single domain and a single day it makes a convincing case. The restriction to this setup reduces the representativeness of the results but on the other hand allows the authors to demonstrate the separate effects of limited resolution of surface reflectance, a priori NO2 and aerosols very clearly. Furthermore, the authors argue convincingly that the results of the selected case study are typical and relevant since a substantial fraction of the global population is living in coastal areas similar to the chosen domain.

The paper is generally very well written and structured, previous literature is adequately referenced, and the data analysis is scientifically robust. I found the joint presentation of spatial maps of air mass factors (AMFs) together with AMF frequency distributions in comparison with the low-resolution results particularly useful and original. A somewhat weak point of the study, however, is the generalization of the results in the discussion section 4 which remains rather speculative, and that's where some of my minor comments will be directed to.

I thus support publication after minor revisions as outlined below:

Minor comments: - page 1900, line 4: There is no radiative transfer model with an "exact" treatment of multiple scattering and aerosols. Every model has to make simplifying assumptions for example about the (radiative) properties of aerosols.

We agree. The sentence has been changed to:
"AMFs are computed here by the radiative transfer model SCIATRAN (Rozanov et al., 2005), which includes full treatment of multiple scattering and aerosols."

- P1902, L6: Does the average NO2 profile represent a daily mean or is it a profile at the typical overpass time of the satellites (i.e. around noon?). For several reasons it would make little sense to use daily mean profiles. Please make clear, e.g. by mentioning in Sect. 2.2 that here only model output at xx UTC is used.

The NO2 profile shown is taken from the model results at the time of satellite overpass, in this case for an instrument in morning orbit. In Section 2.2 we added:

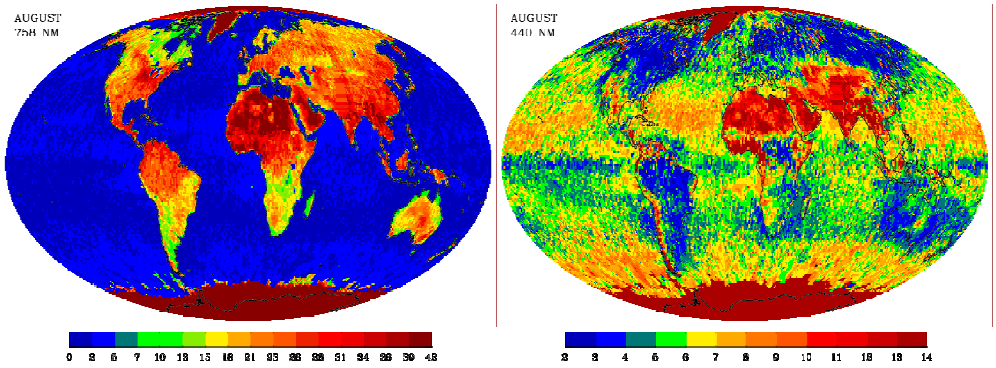
"The WRF-Chem model fields are sampled according to the typical satellite overpass time 19UTC."

- P1902, L23: "obtaine" -> "obtain"

Done.

- P1903, L17: The good correspondence of the domain average reflectance of 0.04 with the GOME LER value is probably just a coincident. Quite a large part of the selected domain is covered by ocean for which a constant value of 0.04 has been chosen which happens to be the same value as given by the LER database. The Koelemeijer data is generally problematic along coastlines (Popp et al., Atmos. Meas. Tech., 4, 463-483, 2011), although this problem is likely less pronounced at the wavelengths where NO2 is retrieved than in the oxygen A-band region mostly considered in that study.

We agree with the comments of the reviewer. The agreement between the Koelemeijer and the domain averaged reflectance is a coincidence, low resolution data such as the Koelemeijer reflectance is not appropriate in heterogeneous areas such as coastal areas and the problem is much smaller at 440 nm than at 700 nm (see figure). Our choice of the Koelemeijer data base was motivated by the fact that it was widely used in satellite data analysis including high resolution OMI data until very recently.



- P1905, L16: Here and at many other places the format of the references is not correct. E.g. "Martin et al., 2003" should be "Martin et al. (2003)"

Corrected.

- P1907, L25: Why not call this section "Overall error" rather than "Vertical column uncertainties", or maybe "Overall error in AMFs and vertical NO2 columns"? The previous sections were addressing the different contributions individually, while this section presents a synthesis and the term "overall" would reflect this. Accordingly, the first few sentences could be adapted to make clear that this section addresses the overall uncertainty for AMFs and for NO2 columns both with respect to relative errors (which are identically for AMF and NO2) and with respect to absolute errors in NO2 columns. What is mostly called "uncertainties" in Section 3.5 are in fact rather systematic errors if we assume that the high (15 km) resolution results provide the truth. I therefore suggest giving more weight to the term "error" as opposed to "uncertainty" in this section, e.g. by changing the titles of equations 3 and 4 to "Absolute Error" and "Relative Error", respectively.

We have changed the title of section 3.5 to "Overall uncertainties in AMFs and NO2 columns" as suggested.

With respect to the difference between "error" and "uncertainty" we feel that uncertainty is the more appropriate word to use here. If we look at the many different AMFs calculated at high resolution for one single low resolution AMF, this reflects the uncertainty in the AMF values. Applying these uncertain AMFs to real measurements will result in NO2 errors (deviations between the true NO2 column and the retrieved NO2 column). From this point of view, we discuss retrieval errors due to uncertainties in the *a priori* estimation.

- Section 4.2: This section should be called "Sensitivity to solar zenith angle" rather than Application to other seasons" since by neglecting the effects of NO2 profile changes, emissions and (not even mentioned) seasonal changes in surface reflectance, the analysis is too limited for a realistic

representation of seasonal effects. The point can still be made that the effect of solar zenith angle was studied as it is one of the key retrieval parameters changing with season.

Agreed. We have changed the title of this section as suggested and rewritten the section to make it clear that this covers only one aspect of seasonal changes.

- P1912, L9: "such as Western Europe" -> "such as parts of Western Europe" wouldn't consider Western Europe to be a generally heavily polluted region.

We agree and changed.

- P1914, L5: "cloud system" -> "cloud systems"

Corrected.

- P1914, L27: "over the area" -> "over an area"

Corrected.

- Section 4.4 on Cloud Effects. There is another important effect of spatial undersampling in connection with clouds not mentioned here: Errors in surface reflectance (e.g. due to a low-resolution data set) lead to errors in cloud parameters such as cloud fraction which in turn affects the NO2 retrieval for partly cloudy pixels. This issue was recently addressed in Popp et al. (AMT 2011) which I suggest to reference here.

We agree and added the additional reference.

- P1915, line 13: a bracket is missing here

Corrected.