Atmos. Meas. Tech. Discuss., 4, C555–C558, 2011
www.atmos-meas-tech-discuss.net/4/C555/2011/

Atmospheric Measurement Techniques Discussions

Interactive comment on "Influence of under-sampled a priori data on tropospheric NO_2 satellite retrievals" *by* A. Heckel et al.

Anonymous Referee #1

Received and published: 11 May 2011

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.

- 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.

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

- 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 se-

lected 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.

- 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)”

- 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.

- 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.

- P1912, L9: “such as Western Europe” -> “such as parts of Western Europe”. I

wouldn’t consider Western Europe to be a generally heavily polluted region.

- P1914, L5: “cloud system” -> “cloud systems”

- P1914, L27: “over the area” -> “over an area”

- 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.

- P1915, line 13: a bracket is missing here

Interactive comment on Atmos. Meas. Tech. Discuss., 4, 1893, 2011.