Atmos. Meas. Tech. Discuss.,
doi:10.5194/amt-2016-366-RC1, 2017

Atmospheric Measurement Techniques
Discussions
Open Access

Interactive comment on "Intra-pixel variability in satellite tropospheric NO_2 column densities derived from simultaneous spaceborne and airborne observations" *by* Stephen Broccardo et al.

Anonymous Referee #1

Received and published: 6 March 2017

This paper presents results from an airborne field campaign in 2007 over the Highveld region of South Africa. The authors compare NO2 measurements from the iDOAS airborne instrument with NO2 columns from the OMI and SCIAMACHY satellite instruments taken from the www.temis.nl data archive. On the whole, the paper is very well written and organized, and easy to read. The results and conclusions are not very surprising (OMI and SCIAMACHY, with lower spatial resolution observations than iDOAS, are unable to resolve the high values in a plume), but the paper is a nice description of several case studies and a record of the campaign, and provides some interesting context for those looking at satellite measurements over the Highveld region. I would

Printer-friendly version

Discussion paper

recommend it be published in AMT after addressing several minor comments below.

The title could be the title of many papers, both previous and upcoming, but this is a fairly narrow study of one field campaign in one location involving one airborne instrument and 4 flights. The data shown is even averaged across-track, so the true variability is not really easy to assess in the paper. There aren't really any very general conclusions to draw from the paper, or general recommendations on how to deal with these descrepancies, that really warrant such a general title. Maybe add something like: "Results from the X campaign over the South African Highveld", or mention iDOAS, SCIA, OMI etc for a more descriptive title. I apologize – this is something I should have pointed out in my initial quick review of the paper.

I see there are some figures in the Heue et al. instrument paper showing a sample swath of NO2 observations below the instrument, but I think some kind of figure showing the original NO2 measurements and their spatial extent and resolution could help put these observations in context in this paper. Also, it is very hard to tell from the figures of OMI/SCIA and iDOAS measurements (Fig 6 etc) that this is a 2-D type of observation. Are these plots only showing the average value every 1.2 km or is there 2-D resolved NO2 in the plots? It's hard to tell and I don't remember seeing it in the text (maybe add to figure captions). Also, it's quite hard to see the colors in some of the plots along the flight track. I'm worried when the figures become the size of a single column it may be even more difficult to see – just ensure at the smaller size that the NO2 colours are indeed visible. You mention the swath width but it is quite late in the paper. I think this could help describe observations if it were mentioned earlier in the paper with the discussion of spatial resolution.

Page 2, Line 32: The slant density is the integral of the path length times the number density of that absorber, not the concentration (which describes the number density absorber as a fraction of the total air density).

Page 3, Line 4: "to a first approximation, is slanted" is a bit confusing. Do you mean because of geometry?

Page 3, Line 12: The analysis of the NO2 slant column is skimmed over without really any detail. I realize there is another paper describing this process, but could you say a few words about what other absorbers and parameters are fit, as well as individual fitting uncertainties from noise or systematic uncertainties? Also, not much info on instrument. What is SNR, are these from spectra that have been co-added spatially, what is the size of the CCD array (pixels), spectral resolution, spectral sampling etc?What is used for a reference spectrum?

Page 4, Line 2: Can you expand just briefly on why a photolytic converter is desirable? Also, why do you present NOy and not NO2?

Section 2: Subheadings would increase the readability of this section. For example: “iDOAS NO2”, “In situ measurements”, “Satellite observations” etc.

Page 4, Line 16: This only the best case at nadir. The sides of the OMI swath are much larger.

Page 4, Line 17: Can you give uncertainties in satellite VCD’s? These can be quite large.

Page 4, Line 20: I got confused here as on initial reading it sounded like the TM4NO2A was OMI data but with SCIA stratospheric slant columns as strat columns had just been mentioned.

Page 4, Line 26: Suggest mentioning swath width here and how many across track pixels there are here.

Page 5, Line 12: Why do you average to 1.2 km? If purpose is to examine intra-pixel variability, how much cross-track and along-track information are you losing? Is this done to reduce error from noise?

Page 6, Line 2: Are there only 8 profiles total and what are locations? Maybe mention here to put in context. This intro to the section is a bit confusing as it presents the conclusion all of a sudden without referencing the data/figures. Maybe add an introductory sentence to ease into the analysis.

Page 6: Is the representation of some profiles as exponential valid in this region? Do you have any surface observations, or model profiles to check against? I realize this is done for contraining the error more than anything, so probably doesn't make a big difference, but I'm just curious.

Figure 2 and 3: Can you specify surface altitude here or show on figure? Is it the bottom of the y-axis?

Figure 4 and relevant text: What do you do for NO2 profile in stratosphere in the model? Does NO2 in the stratosphere contribute to the AMF or do you assume it cancels perfectly with the reference spectrum?

Section 5: Could increase readability with subsection headings here.

Page 12, Line 11: Clearly the AMF changes drastically with the surface albedo. You are using your calculated AMF values to set bounds on the AMF error. How is the uncertainty in the AMF from uncertainty in the albedo determined (it's going to be high, with such a low resolution OMLER product)? Why not use MODIS albedo, or MODIS BRDF for an even better represention of the surface for high resolution observations from the aircraft?

Page 15, Line 15: I found this confusing. What is your reference? Do you have remote ocean measurements from the aircraft?

Page 14, Line 14: Could the same effect be achieved by putting the iDOAS on an aircraft that flies higher?

Page 14, 19: "Underestimates" the peak only. What it's actually probably doing is just averaging out everything in the field of view (so you could equally say "overestimates" the background).

Page 13, Line 5: Not sure why you have to use two fitting schemes. Does that tell us anything?

Page 15: You make a few comments about plume age, source etc. I noticed you used HYSPLIT earlier in the paper. Can it tell you anything about these specific cases?

Page 16, Line 19: Can you remind us of SCIA overpass time here? Also Figure 13 caption reads a bit like the times are for the satellite observations (I'm guessing it didn't take 50 minutes to fly over the region!)

Page 17, Line 6: Not sure you can draw any conclusions about SCIAMACHY vs OMI at all here. There is a very limited amount of SCIA data at high NO2 values. Obviously your slopes are very different on different days with OMI as well.

Fig 6 and similar: Can you specify that these are 1.2 km averages in caption or in text (which I'm assuming they must be?)

Figure 6: I can't tell which is sub-aircraft pixel as it looks like flight was right down the border of two cross-track positions. Can you clarify this in text?

Figure 7 and similar, and relevant text: The average isn't technically over the "area" of the OMI pixel, which might have a 13x24 km2 size. Clarify.

Figure 7 and similar: Specify colors for elevation/albedo subplot.

Figure 8: Specify which are OMI and which are SCIA observations (maybe in legend?)

Interactive comment on Atmos. Meas. Tech. Discuss., doi:10.5194/amt-2016-366, 2017.