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Atmospheric Measurement Techniques Discussions

Interactive comment on "Retrieval of tropospheric NO_2 columns over Berlin from high-resolution airborne observations with the spectrolite breadboard instrument" *by* Tim Vlemmix et al.

Anonymous Referee #2

Received and published: 8 April 2018

The manuscript by Vlemmix et al. presents observations from an airborne imaging spectrometer to measure NO_2 vertical column densities over Berlin during the AROMAPEX campaign. The scientific focus is on studying the effect of surface reflectivity on the conversions of differential slant column densities (DSCD) to tropospheric vertical column densities (TVCD). The manuscript also discusses some more general aspects of the SBI instrument, which was developed in the authors institution. Overall, this is an interesting manuscript that convincingly presents the main point, i.e. the importance of detailed surface reflectivities for NO_2 TVCD retrievals. The manuscript clearly fits in the scope of AMT. I found some of the discussion of the instrument, such as Section 5.1, to be somewhat distracting, and I would like to encourage the authors

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to focus on their main point when revising the manuscript. I will also point out a few parts of the manuscript that are difficult to read/understand for someone who is not intimately familiar with the concepts used in the UV-vis remote sensing community. I recommend this paper for publication after addressing some minor issues listed in the following:

Page 2 last paragraph: One of the issues I found initially confusing is the definition of surface reflectance, in particular in relation to general terms of reflectivity and bidirectional reflectance distribution function (BRDF). I think it would help the manuscript to define the terms early on, and briefly review the concepts

Page 2, line 31: Figure 1 needs much more explanation. Only absolute specialist will understand this figure. (see also comment on AMF's below)

Page 3, line 10: Define "BRDF"

Page 4, line 14: Why only average along track direction? Would averaging into more square-like pixel not make the interpretation easier?

Line 4, lines 28-31: This is not a sentence. Please reformulate to remove the two colons following each other.

Page 5, Section 3.1: The aerosol extinction profile and its link to AOT is not clear. Is the entire AOT present in the boundary layer, or is some of it above the BL as a background aerosol? How about stratospheric aerosol?

Page 5, lines 32-33: After reading the entire manuscript I understand the motivations for scaling. However, when I first read the manuscript I was thoroughly confused at this point. I think it would help to add a sentence here to explain why it makes sense to scale the radiances. Also, it is not clearly stated which radiances are scaled. I am assuming it is the observed 440nm radiances from the aircraft?

Page 6, lines 9-12: Why were other trace gases that absorb in this wavelength window not included? Most DOAS fits in this window now include H_2O, O_4, glyoxal, and

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sometimes even IO.

Page 6, lines 14-15: How do you know it was a clean region?

Page 7: The concept of the airmass factor needs to be introduced here. AMF's are not universally used in the remote sensing community, and therefore need to be defined. This also applies to height resolved AMF's such as shown in Figure 1. The AMF terms in equation 1 need to be better explained, in particular with respect to the altitudes to which they apply. For example, I am assuming that M_{trop}^{ref} is for downward viewing geometry from the aircraft (once through the troposphere + the lowest 3km from the ground to the AC?). At this point this section can only be understood by an absolute specialist. It would benefit the manuscript greatly to provide a more general explanation. A more general question: When performing the radiative transfer calculation you essentially assume that the surface reflectivity of one pixel applies to the entire atmosphere. However,y our observations are essentially a 3D RT problem. Do you think the reflectance of the surrounding ground, i.e. under the entire slanted light path, has an influence?

Page 7, line 31-32: Can you please explain which of these profiles was ultimately chosen for the TVCD's reported later in the manuscript? How did you select it?

Page 8, Table 2: One would expect that NO_2 and aerosol have a very similar box shape, as the profile is determined by vertical mixing, i.e. height of the boundary layer, which influences both equally. Why investigate profiles with different top height?

Page 8 line 22: Something is wrong with this sentence. Please fix.

Page 9, last two paragraphs: Can you provide a more quantitative argument for the approach using adjacent pixel? Is there a statistical basis for defining the detection threshold at DSCD's for which relative differences are smaller than 50

Page 10, line 12: This statement is not supported by Figure 2d,e. Please modify the figure to support your argument (an arrow from east to west, or something similar would

probably be sufficient).

Page 12, Section 5.1: I think this section is not necessary for the manuscript. The fore-optics used for the study has no impact on the main conclusions. The discussion of the choice of binning could be move to Section 2.

Page 13, Section 5.2: It would be interesting to know how well one needs to know AOT and its spatial distribution to improve the observations.

Page 14, line 23-24: I do not remember that processing only every second across track viewing direction was mentioned previously. Either add a section on workflow in section 2, or leave this discussion out.

Figure2: Please increase the fonts on the panels b-e. The gray grid in the panels is not at all readable, even in the PDF version of the manuscript.

Figure 14. I could not find a discussion of VCD_{geo} in the manuscript.

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