

Interactive
Comment

Interactive comment on “Space-based retrieval of NO₂ over biomass burning regions: quantifying and reducing uncertainties” by N. Boussez

Anonymous Referee #1

Received and published: 22 August 2013

This paper has an interesting bottom line as represented in Figure 11, that NO₂ retrieved from space over active fires will have multiplicative errors that linearly increase from unity for small fires to a factor of three for large ones. This and other results in the paper are probably important, however the paper is quite sloppily put together and lacks the context of the recent literature. As a result, I am confused about what the paper's firm conclusions are and the extent to which they are novel.

In its current form the paper is not publishable in AMT. The following are among the issues that should be addressed in a major revision. I do not list every issue as I believe the paper should be dramatically rewritten for clarity, conciseness and to place it in the context of the current literature prior to a more thorough and detailed review.

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper



1) Fires are among many sources that are small compared to the standard a priori used in many retrievals. There have been a variety of recent attempts to account for these issues including papers on improved retrieval strategies from the Berkeley (e.g. Russell et al. ACP 2011), Bremen (Heckel et al AMT 2011), Swiss (e.g. Zhou et al. AMT 2009) and KNMI groups (e.g. Zyrichidou et al. Atmospheric Research 2013), and papers on ad hoc after the fact corrections (Lamsal et al JGR 2011). I am not sure I understand the difference between the beta factor proposed by Lamsal et al and the description of an ad hoc correction described in this paper. A direct discussion of the similarities and differences to approaches in all of these papers is warranted with a discussion that distinguishes between the issues that are generic to study of small scale spatial features and ones that are specific to study of fires (presumably only the aerosol effects?).

2) The references in this paper are very sloppy. The first three references in the paper don't appear in the reference list. I stopped checking after that.

3) I find it surprising that papers discussing satellite observations of fires are not used to motivate this study and place it in the context of recent literature. References that should be discussed include Mebust et al (ACP 2011 and GRL 2013) who use OMI to infer NO₂ emissions from fires, Boersma et al. (JGR 2008) and Herron-Thorpe (ACP 2010). Mebust et al. describe a factor of three bias arrived at in a completely different manner and describe a strategy for correcting for NO₂ conversion to PAN within the OMI pixel that is relevant to this paper's aims.

4) Figures 1, 2, 5, 6, 12 can be omitted and maybe some others as well, to help focus the paper.

5) The abstract indicates the sign of the bias in fire retrievals is not uniform. Some additional clarity on how to move forward given this issue is warranted.

6) The first sentence in the abstract includes a vague descriptor "quality". A more specific definition of this word is needed.

Interactive comment on Atmos. Meas. Tech. Discuss., 6, 6645, 2013.

AMTD

6, C2195–C2197, 2013

Interactive
Comment

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper

C2197

