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Atmospheric Measurement Techniques Discussions

Interactive comment on "Space-based retrieval of NO_2 over biomass burning regions: quantifying and reducing uncertainties" *by* N. Bousserez

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Response to anonymous Referee #1:

I would like to thank the anonymous referee for his useful comments and suggestions about the manucript. Please find my responses to each of them below, as well as a revised manuscript attached (pdf file).

General comments: 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

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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.

Response: The references have been updated in the introduction of the paper. The introduction has been entirely rewritten in a effort to clarify the context and goal of the study. The conclusion has been revised according to the changes made in the paper.

Specific comments: 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 theaerosol effects?).

Response: The references proposed have been included in the introduction. The different approaches to retrieval improvement are now clarified. In particular, improvements in the studies mentioned rely mostly on using high-resolution data and model simulations used in the retrievals, while here we propose to use information from the measurements themselves to correct for shape factor errors in the AMF. The beta factor defined in Lamsal et al. (2011) describes the sensitivity of the retrieved NO2 column to the underlying NOx emissions. In our study we establish a relation between the shape factor correction and the associated slant column variation. Now we also clearly sepa-

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rate the shape factor representiveness issue (common to all sources with high spatial variability) from the biomass burning aerosol issue in the introduction.

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. Response: We updated the reference list.

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 it 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 papers aims. Response: The introduction now includes and discusses the abovementioned references. However the methods proposed in Mebust et al. (2011) are designed to improve the estimation of NOx Emission Coefficients (ECs) based on satellite observations of NO2 and fire radiative power. A different problem is addressed in this study, which is quantifying the errors in the NO2 retrieval itself.

4) Figures 1,2,5,6,12 can be omitted and maybe some others as well, to help focus the paper. Response: Figures 6 and 12 have been removed. Figures 1 and 2 have been combined, since they provide useful information on the location of the aircraft measurements, such as their vertical and horizontal spatial extent. Figure 5 shows the spatial distribution of the AMF perturbation related to biomass burning emissions for boreal and savanna fires. Since quantifying the AMF errors over biomass burning regions is one of the main goals of this work, this figure has been kept in the revised manuscript.

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. Response: The

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difference in the sign of the bias in the retrieval was discussed for the aerosol impact over savanna and boreal fires. In response to another reviewer's comment about the fact that clouds may implicitly account for aerosol effects, only the map of the AMF shape factor correction over the two regions is now shown and discussed. This shape factor correction has same sign for both regions.

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

Please also note the supplement to this comment:
http://www.atmos-meas-tech-discuss.net/6/C4275/2014/amtd-6-C4275-2014-supplement.pdf

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