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

Anonymous Referee #3

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General Comments

This paper deals with an important topic for the scientific community dealing with trace gases retrievals from satellite observations. It is a relevant subject and the paper can be a valuable contribution to the understanding of aerosol impact in the satellite retrievals. To my knowledge, a study focused on satellite retrieval of NO₂ from biomass burning events making use of measurements from specialized campaigns has not been presented before. The idea is interesting and has been discussed in previous publications so it is certainly a relevant subject and a novel achievement (if well conducted). There are many issues with the current form of the manuscript and as it is the paper cannot be published. Major revisions are necessary and my main concerns are raised below. The general structure of the paper is reasonable but the text becomes confus-

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ing at parts and often the author states facts not fully explained to the reader. A more complete exploration of scientific concepts with further depth sending across a clear message with ground-breaking conclusions is certainly missing from the manuscript. As a full revision is needed the conclusions should also later be made clearer and focused on what was actually found with this study and how can that be used in the future.

The text needs some major revision on grammar and spelling. Some suggestions are made below but there is room for additional improvement regarding grammar and clear writing to help the reader follow the paper. For example, section 3.1 repeats too much information in different sentences and this could be more concise.

The whole referenced literature needs a profound revision, not only regarding the reference list that does not include all those mentioned in the manuscript, as well as, addition of some more references on the topics explored such as: characteristics and emissions of biomass burning events (see below further comments); previous sensitivity studies done (several mentioned in Leitao et al. 2010 that is the only mentioned for the conclusions in P6655). Please provide references to facts stated such as: the campaigns mentioned in P6648; P6657 L19 the typical values of surface reflectances. There are many declared facts in the paper that are not supported with suitable literature.

Missing references in the reference list are at least (but this was obviously not a comprehensive control): Logan 1983, Finlayson-Pitts and Pitts 1986, Leue et al 2001, Eskes et al 2003, Andreae and Merlet 2001.

Verify as well the references in the text as sometimes errors are made in year Jacob et al. 2009 should in fact be 2010.

Specific comments

1) In the second paragraph of the introduction the author says the NO₂ satellite retrieval

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is a two-step process. This would be the case if one considers the fit and the slant to vertical column conversion the two broad steps. However, in the latter there are different steps to be performed and it should be at least mentioned that the slant column determined from the fit is total and to obtain a tropospheric part several corrections are needed. Considering that the paper is mainly focused on the retrieval of NO₂ and its uncertainties a proper description of retrieval process is essential.

2) Also, the paper fails to provide a comprehensive introduction of the problematic of satellite retrievals in the case of fires. What has been done in previous studies and how is the aerosol correction addressed by the different groups working on this? How is this correction useful and important for the scientific community? Furthermore it is important to make clear distinction from what is already done and what can be changed with suggested methodology of this study.

3) It is not mentioned anywhere in the manuscript that fire events are different in many characteristics (e.g., intensity and fuel and resulting type of emissions). Temporal and spatial variability are high but the different fire types also represent a challenge to accurately reproduce these events in models or apply standard correction to satellite retrievals. Again at this occasion a proper literature review support is missing. This should also be mentioned in the beginning of section 5.

4) Also, in section 4 the measurements made are not put into perspective with previous findings and published studies that would support similarity or differences within events. For the AMF retrieval the retrieval of correct and typical optical properties and aerosol load is important and if the data from the campaigns is used as base of the study it is important to understand if the values are or not expected according to previous observations.

5) In P6652 it is concluded that the higher values of the aerosol extinction coefficients are because of more intense fire emissions. It is not clear to me how the author came to this conclusion. Higher extinction does not necessarily and straightforward means

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higher emissions, as this factor depends on size, mass concentration and chemical composition of aerosol. From Fig. 6 with the different scales used for Canada and African events this conclusion cannot be easily perceived. In this study two very different fire types are analysed: boreal and savannah. It is true that the magnitude aerosol emission of each event might be quite different simply because of different burning phase and combustion efficiency and fuel burned. But all these factors also influence the type of aerosol (mixture) found in each of the events, an issue not mentioned anywhere. This might be an important information for the study performed as, depending on the aerosol load, the different composition might lead to different impact on the retrieval and this issue is not addressed in the manuscript.

6) Still in section 4, the AERONET data is compared to the DABEX campaign results. Why isn't this done for the other campaign as well? Why also in Fig. 6 the CALIPSO observations are only presented for DABEX? In fact, both campaigns are mentioned but then only one is fully explored.

7) In section 6 it is not clear what model is used for the sensitivity tests. The LIDORT model is mentioned in the abstract and section 2 but not in this section. Also it's not clearly explained what were the input parameters for the model and what info is taken from model and from measurements.

8) In P6656, L16 the sentence "Aerosol may impact the retrieval of cloud parameters." On its own and short like that it is a confusing sentence as the opposite happens as well, it all depends on what is to be retrieved from which instrument measurements. So please explain better what the idea is behind this sentence. Essentially, the relation and influence of aerosol on clouds and vice-versa is not clearly explained and becomes confusing to understand how the author managed to separate one from another. For example, in section 7 the concept of "pre-existing clouds" becomes very disconcerting. It is not clear what the author is referring to here and relation implied to what is used in the OMI retrieval. Furthermore it is not clear in the manuscript how the clear distinction is made (and found) between the effect of aerosols on the cloud retrieval and vice-

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versa (it is certainly a two-way relation but it's not made understandable). To imply that one affects the other in the sensitivity study performed it is important to, in the first place, how aerosols affected the cloud parameters as well and, if this effect has been excluded state clearly how it was so. This is introduced at the end of section 6 but should be better explained how in fact it was possible.

9) In P6658, L10-11, it is said that NO₂ remains in the boundary layer only. Please provide a reference for such general statement or rephrase and focus on this particular case. What boundary layer height is assumed here? The full 2km where NO₂ is present? In some events NO₂ might in fact be transported to higher altitudes due to convection within the fire plume. It is not uncommon to find injection heights of smokes to the free troposphere. To my knowledge it is still not clear how high NO₂ is injected and how this change across different events. Compared to aerosols and other trace gases the short life of NO₂ complicates the analysis of this species in fire events.

10) P6660, L11 the author says that one can see that uncertainties are smaller than those associated with missing biomass burning sources. At this stage is not clear what is being compared to what. At the end of the same section the author fails to mention and explore the potential of ground based measurements when it refers to only data from other aircraft campaigns. Campaigns provide very useful datasets but are still often limited to specific events with very particular characteristics.

11) The author claims to have found a correction method but it does not clearly elucidate the readers if this method can actually be used on a near-real-time retrieval as automatic correction (and how would that be done) or just post-retrieval correction for specific cases. How would those cases be identified or selected? "In practice, this methodology could be extended to any type of NO_x source, with possible adaptations of the formula over areas with very different retrieval characteristics (e.g. surface reflectance)." Can this study based on one particular event provide a solution with one formula that will solve the retrieval problems? These events are very different from one another, so is the solution found on this study applicable to other and all biomass burn-

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ing events? What information or data is missing to adapt the correction to other events such as desert dust storms? Or peak urban pollution events? The idea presented is very interesting and important subject for progress in satellite retrieval but the author fails to properly explore and explain this.

Technical corrections

1) It is awkward to read the whole paper as it is a group work (words “we” and “our” are often used) although there is just one author and no team collaboration is even acknowledge. In general the paper would be better written in impersonal form.

2) Suggestions for figures and tables: Combine table 1 and 2 as they provide the same information but for different campaigns. The same can also be done with Fig. 1 and 2 joined into one alone. (as in the text, the references mentioned in the tables are missing in the reference list)

Figures 3 and 4 can also be combined, and a legend would help to identify which line is what. Maybe presenting the median and mean is not so useful (even if we all know that often these differ this difference is not really highlighted in the text). It is also not clear why for one case SSA is shown and for the other the scattering coefficients.

Figure 5: the different colorscale in the aerosol correction does not help the comparison so please change that. You might also want to delete the extra “aerosol correction” in the figure for Canada.

Figure 6: please change the figures so that the y axis has the same height for both Canada and Africa. Also use the same colorscale for the extinction coefficients. From this figure and with such lower values of extinction it's hard to understand how for the African fires the emissions are much higher. Maybe the AOD can also be calculated so that it can support the statement made in section 4. In the previous figure the values are averaged over time, and here? Please make that clear in the text or caption of figures. Please correct also in this caption and next one that the overpass time of OMI

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is 13.30LT or 1.30pm.

If I understand Figure 7 correctly it can be simplified by basically having the NO₂ (ppb) in the x-axis instead of shape factor value.

In Figure 8 it's not clear what values are kept constant and which are changed. Is it the case that the reference has: SSA=0.91, SZA=40°, and surface reflectance=0.03? This info should be more clear in the text (specially) or figure.

3) Although the chemical compounds are normally known to the scientific community it is good practice to write the name such as for HO_x, PAN, HCN, etc. The same for used acronyms or variables such as: Slt, OMI DP GC, TM4 (P6659, L5, L22, L23, respectively), MODIS (P6660, L2), GOES (P6662, L26)

4) Typos to be corrected across the paper such as, aircraft (twice at least), understanding (P6650, L11).

5) Throughout the paper often I came across incomplete information such as: P6646, L11: which emission inventory is that?; P6658 L5, what NO₂ climatologies? (at least a reference should be mentioned).

6) I believe it is more correct and complete to say “solar backscatter radiation”, “extinction/scattering coefficient”, “aerosol correction factor”.

7) P6646, L2: NO₂ AMF depends on more than just the 2 mentioned issues so for completeness, add “among others” or “for example” (same for P6647, L14)

8) P6646, L20-21: rephrase as it looks like aerosol itself is sensitive to surface reflectance

9) P6647, L5: replace “space and time variability” by “spatial and temporal variability”.

10) P6647, L20: several CTMs already represent fire events, therefore rephrase as “not resolved” is not fully true.

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11) P6650, L10: to my understanding the AMMA is a project rather than just a campaign

12) It is confusing to often read “shape factor” without the NO₂ behind it.

13) P6655, L16: replace “has negligible” by “usually, has negligible” – in some occasions the differences verified are not so negligible.

14) P6655, L17: “over urban areas” might lead into mistake. The study of Leitaó et al. (2010) uses a NO₂ profile typical for urban areas. If similar NO₂ profiles are found in other locations with other aerosol plumes (such as those of biomass burning events) than the study is valid as well. The point here is if the profiles used in these two studies are similar (which it seems to be the case) then the conclusions are valid for both, i.e., these recent findings are in line with previous ones.

15) P6655, L20: add that the AMF is almost insensitive for this particular case because it's not always the case

16) P6661, L2: replace winter by austral summer

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

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