

We gratefully thank the reviewers for the constructive comments and suggestions to improve the manuscript. Below are the detailed responses to their comments. The reviewers' comments are listed in black italics and our responses and changes in the manuscript are shown in blue. The changes in the revised manuscript are also highlighted.

Referee #2 report

Review of

Measurement Report: Potential of MAX-DOAS and AERONET ground based measurements in Montevideo, Uruguay for the detection of distant biomass burning

by Matías Osorio et al

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Summary: This study reports observations during an episode of long range transport of smoke using a MAX-DOAS instrument located in Montevideo, Uruguay. The observations reported are complemented with ancillary standard information available such as Aeronet aerosol products, HYSPLIT model runs to trace back the source regions and satellite products from the TROPomi sensor. The novelty of this study is the first-time description of a smoke event in this region of the world with a combination of instrumentation that has become standard in the northern hemisphere but are notoriously lacking in the southern hemisphere. Studies like this one are very much welcomed. I view this study as a reference work where future research focusing on smoke transport in South America can compare with. This fact makes this study worthy of publication and I recommend to it. I do have general comments about writing style, and suggestions for figure improvement and clarifications. Not addressing them should not be a reason to prevent the publication however, it would make a more enjoyable reading and useful information for future studies as I detail below.

We appreciate the positive opinion of Referee #2 about the study and the manuscript. Please, find below the response to the detailed comments.

Detail:

1) While this is a measurement report and there is no expectation of new discovery reporting or lengthy discussions and speculation, the general tone of the paper is too tentative in the sense that it is written as if the observations must demonstrate that what it is been observed by the MAX-DOAS is smoke. I think the emphasis should be changed to a different tone where established techniques are used to track a smoke event. The goal is to report how these observations and analysis perform in a region/environment where there hasn't been many application of such techniques. In other words, it is not a matter to prove that smoke was arriving at Montevideo but the goal is to report how a smoke event is observed in Montevideo with these commonly used techniques in the northern Hemisphere but not in this region. For example expressions like:

lines 204-205: *"The main interest in this analysis is to assess the effectiveness of formaldehyde and glyoxal as plume tracers from biomass burning events and their detection in Montevideo."*

Unless there is an alternative reason to suspect that formaldehyde and glyoxal are coming from somewhere else and given that it already been seen in smoke events elsewhere, it is not necessary to be so tentative to prove the smoke is coming from where the satellite images and model clearly show. This tone pervades the paper and it distracts from the real novelty which is to highlight how such event is seen by a tool that is not common in the South America. So, I highly advice to do some style editing.

We followed this suggestion and the text in the new version of the manuscript was modified accordingly, with which we hope that the new version will better define the objective of this work.

2) With regards to readability the paper is perfectly understandable and fine. However, there are some mistakes particularly regarding the use of prepositions. I will not detail those because it is too tedious, but I advise to have it check by a native speaker.

We reviewed and corrected the new version of the manuscript, as suggested.

3) Figures: overall I found the figures clear and appropriate, except for couple of cases I mention below

Thank you for the positive comment and feedback.

4) line 225. Can you clarify how those percentiles are computed? they are not explained in the satellite section and as far as I know, there are not part of the standard satellite products.

We defined a region of interest as a circle of 25 km radius centered in Montevideo. Values over water and invalid data were discarded. We calculated percentiles 25th, 50th and 75th with the values of the pixels in the region of interest after the screening.

We added the following statement in the manuscript (from line 236):

“We calculated percentile values for the corresponding TROPOMI-S5P products in a circular region with a radius of 25 km centered in Montevideo. These are shown as horizontal lines in panels a, b, and c.”

5) line 228-230 , figure 8b is a scattering plot for which there is linear fit plotted. It is not a correlation plot. Please correct.

Done.

6) Paragraph starting 236 , Not clear to what plot or figure this explanation is referring to , so it is difficult to verify.

This explanation refers to the results presented in Figures 7 and 8. To improve the text, we added this information to the start of the paragraph:

“On this particular day, the AOD values derived from BOREAS are also above those from the days before and after November 24, qualitatively following the behaviour of the AERONET values (see Figure 7(d)). However, the latter are approximately 30% higher, as shown in Figures 8 (a) and (b).”

7) full text in page 15 (starting line 248). This is a difficult description to follow because the data for Nov24 shown in figure 7 is too cramped. I think it would be more illustrative to add a figure displaying a time series of hourly observations for 22 and 24/Nov to illustrate this paragraph.

Aside from those two dates, in Figure 7 we aim to display VCD values in Montevideo that were not affected by the BB event. Therefore, we decided to maintain the extended period in Figure 7.

8) incidentally, I do not think that figure 9 is very useful because there is little independent information to verify the profiles. I think that replacing it with the above suggested new figure would be more useful.

We agree with the Referee’s statement that independent information is not available, and that the verification of the shown profiles is therefore not possible. From a plausibility perspective, the spatial and temporal evolution of the species are nevertheless reasonable. We believe that figure 9 is worth keeping in the manuscript, as it is the only figure which shows the differences in altitude for the individual specie, as all other figures show integrated values only.

Figure 9 might also help future authors to compare with their results and to further analyse the different spatial inhomogeneities within larger plumes at other biomass burning events.