

Interactive comment on “Quantifying the impact of BOREal forest fires on Tropospheric oxidants over the Atlantic using Aircraft and Satellites (BORTAS) experiment: design, execution and science overview” by P. I. Palmer et al.

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Received and published: 24 May 2013

First, we would like thank both reviewers for their useful comments. Below we provide a response to each comment (marked in italics).

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Response to Review 1

This paper provides an overview of the BORTAS experiment. Papers such as these should satisfy two basic functions. One is to provide a service to the reader who is looking for broad information on the project and an idea of companion papers that are most relevant to their interests and offering deeper detail. The other is to catalog important information of common interest to the project, thus providing the writers of the companion papers with a reference to avoid excessive and redundant descriptions of the project. This manuscript does a good job on both accounts.

I only have a few errata that the authors need to address before publication.

1. *Figure 3e is incorrect (currently same as figure 3d)*
Noted and corrected in the revised manuscript.
2. *In Figure 7, the black diamond referred to in the text and figure caption is not readily apparent. It would help to have an arrow pointing it out. It would also help to show a line along the axis of the occultation since it is not a simple point measurement.*

Noted. We have reproduced this figure with a larger diamond.

3. *In Table 2, the method for measuring speciated reactive nitrogen is listed as "Light Induced Fluorescence (LIF)" but I assume that thermal dissociation is also involved.*

The reviewer comment is correct, in the paper we used the short name LIF, but

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the instrument used is a thermal dissociation laser induced fluorescence system (TD-LIF): we have corrected this in the revised manuscript.

Response to Review 2

The paper of Palmer et al. provides a description of the BORTAS experiment. Basically the paper reports a short description of the meteorology based on geopotential anomalies, three examples of satellite observations during the campaign, a long discussion of the project strategy and some highlights of published analyses. There is an undoubted interest in publishing an overview paper for a measurement campaign but I consider that this article should be largely improved and extended in some parts. Based on my experience, the main point of concern of overview papers is to ensure to bring a novel and meaningful scientific innovation as required in a publication in ACP. It is certainly worth to resume the main scientific results of the campaign but it may be also important to provide an introductory publication giving for instance (1) the scientific basis, (2) an accurate description of the meteorological context and possible specificities of the campaign period (3) background conditions of atmospheric composition deduced from the observations (4) an overview of peculiar observations that will be discussed in deeper detail elsewhere and (5) results that will not be reported in specific publications.

This is a fair point but the philosophy we followed was to introduce the main objectives of the proposal, discuss briefly the meteorological and pyrogenic environments, report how we ran the campaign, and summarize some of the studies that have been submitted and are ongoing so the reader can get an overview of the campaign without being affronted by a lengthy manuscript or compromising the reporting of results in specific papers. This was particularly important since BORTAS-A and BORTAS-B includes deployment of ground-based, aircraft, and satellite data as well as a number

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of complementary modelling activities. Our philosophy is shared by many other campaigns.

The manuscript addresses the above issues presenting a large wealth of potential information but without a desirable (at least to my opinion) in-depth analysis. The quality of the presentation may be also improved to better focus on the main message(s) of this paper. The manuscript is quite long on the observational and modeling strategy but it often occurs that the reader is left wanting more of the main results and methodologies. There are several repetitions and, sometimes, we get the impression that parts manuscript resemble more to a campaign wash-up report than to an overview publication. I do understand that it may be difficult to have a meaningful analysis leaving room for the other publications but I suggest to take a look to what done for instance in Reeves et al. ACP, 2010 that gives a complete view on the AMMA-chemistry aircraft campaign, of the mean observed concentrations and a close look to specific data that are analyzed in detail in other publications. Similar approach was chosen (for a fraction of the wider campaign described by Reeves) by Cairo et al., ACP 2011 that, in addition, provides a detailed assessment of the meteorology, atmospheric transport and their variability during a 2 weeks campaign in 2006.

See above for our response. We note with interest that at least one review of the Reeves et al overview paper suggested using our approach.

One possible way to clarify the presentation is to provide an introduction that could serve for other BORTAS papers, strengthen the description of the meteorological and emissive condition and extend the discussion of the results. In the following I will provide some suggestions section by section.

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Abstract may be revised (and somewhat shortened if necessary), reducing the focus on the campaign activities and highlighting the results. For instance the campaign re-scheduling due to the Icelandic volcanic eruption should be kept (if needed) in the campaign description section.

This is a valid point about focusing on the results. The revised abstract now reflects this.

Re-scheduling of the campaign is important because this effectively splits the campaign into two phases, A and B.

Introduction: Large part of the introduction deals with the description of the campaign and the related activities. This smears out the necessary synthesis of the current state of the art on the role of forest fires on atmospheric composition and why BORTAS brings (or is expected to) novel information. In particular it would be desirable to have a status on the knowledge on the main (expected and already published) science foci presented in section 4. Discussion on VOC oxidation and RO production processes may be shortened and figure 1 skipped.

We are fortunate that two papers have recently been published that summarize current thinking: Jaffe and Wigder (Atm Env, 2012) and Monks et al (Atm Env 2010) and decided for the sake of brevity to cite the first paper; we have now cited the second review of chemistry in this paper. We have placed more emphasis on the novel aspects of BORTAS in the revised manuscript.

Meteorological data: It would be desirable to have also mean wind fields with superposed the fire position and campaign locations and if relevant precipitation. Retroplumes (or more simply back-trajectories) from flight area may also be reported

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here as done in the following subsection for the PICO observatory. Concerning discussion of figure 3, a sentence on how representative BORTAS may be for mean July conditions would be useful. Why climatology is restricted to 1979-1995 ? Would it be useful to extend it to 2012 ? Here it would be useful to present retroplumes (or retro-trajectories) for the BORTAS flights and connect them to the fire spots. This section may also include the background information on the chemistry and emissions. So, discussion from line 7-26 page 4147 may go very well here.

Good suggestion. We have included a Figure showing the mean vector wind fields during BORTAS-B with accompanying text and included a Figure showing the mean daily precipitation during the same period, which helps us to link with two papers that investigate the role of precipitation in aerosol transport during the campaign. We have revised the geopotential height Figure to encompass 1981 to 2010. We have decided to discuss the meteorology separately from the chemistry and emissions but making links between the two; otherwise the text becomes difficult to read.

Campaign description: First part of the introduction (lines 4-27) may be included here. The altitude sampling may be added to the flight path to fully assess the representativeness of the observations. This may also be the place to include the list of models given in 3.5 that are now slightly out of context. It is not clear where data from Pico observatory are used here. If worth, the information on how Canadian fires impact the free troposphere in the Atlantic may be discussed in more details with one plot in the last section. In general I suggest to reduce this section and try to report the discussion of results in a proper subsection (see below).

Including the altitude sampling will not necessarily address the representativeness of the observations which is beyond the scope of this paper. The campaign description does not require information about any models until we discuss forecasting and

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deployment; modelling is only one activity within BORTAS. We have included a description of Pico and how these data have played a role in intercepting Canadian boreal fire plumes in previous years and state clearly it played a smaller role in 2011.

Satellite data: Analysis of satellite data from IASI and ACE are presented as an example of what may be observed or how the comparison with aircraft data may look like without fully exploiting the information that may come from it. The comparison with TES profile (for a single day) looks a bit out of context here. For instance it would have been desirable to provide mean fields for the whole BORTAS period or for all the flight days than few single days. Is it possible to compare them to GEOS-5 fields that may have been introduced beforehand?

We showed the satellite data to illustrate the initial products and to qualitatively put these data into context of the whole campaign. Substantial analysis is required to integrate these data in a quantitative sense, which is beyond the scope of the overview paper, which is now stated in the main text.

First results and science overview: This section should be clarified. I would add a couple of sentences presenting the results and clarifying that perspectives and on-going work are reported in the conclusions. I warmly suggest to break it in subsections. One natural division may be: 1 Identification of CO plumes 2 Emission ratios for organic compounds 3 Ozone photochemistry 4 Source attribution referring to the already published material at the beginning of each section.

Good suggestion. We have broken up this section into subsections.

Subsection 1 may be extended with one or more plot. It would be desirable to see the

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data from flight B625 compared to the ones with interception of plumes to strengthen the discussion on the threshold used for plume identification. This is an important information to be given here. Despite the fact that the flight strategy was plume-hunting oriented, it would be useful to know which fraction of data are available for in-plume studies.

The data from B625 is reported and discussed at length in Parrington et al. Different amounts of data are discarded depending on whether the constraint is CO, HCN or CH₃CN. The CO constraint removes the largest amount of data (85

Subsection 2 may report a selected plot or at least the ER values from Lewis et al. 2013 and a more thorough discussion. Again, quantitative results from Purvis 2013 (that is not yet published in ACPD) may be anticipated here mentioning that there is a paper in preparation.

We have provided a description of the Lewis work and refer the reader to the paper. The Purvis paper is described, but as discussed above we feel it is inappropriate to present key figures that form the basis of work being prepared for publication.

Subsection 3 should include as well a plot for the L-shape O₃-NO data.

We remain unconvinced that reporting these data in one Figure will significantly improve the readability of this paper, and for the sake of brevity we prefer to direct the reader to Parrington et al.

Discussion on source attributions in PM_{2.5} is incomplete. Table 4 is a list of observations and more robust comparison should be presented with a plot. I guess this may

be taken from Gibson, 2013.

Gibson et al, 2013 had not been submitted at the time of submitting the overview paper. We have included a plot of levoglucosan and PM2.5 to reinforce the work described in Gibson et al.

Conclusions: This part should be completely rewritten to include a synthesis of sections 3 and 4 and to list the on-going analysis. In this context I am not sure to understand what is meant by the sentence at page 4153, lines 14-17 that seems too vague.

We have revised the conclusions to emphasize some of the findings from the campaign.

Minor points:

- *Line 9 page 4146: define the m/z ratio. Done.*
- *Could you provide a reference for the sentence in lines 11-14 page 4148 ? Done.*
- *Add the journal for Gibson, 2013. This and other journal updates will be done by ACPD.*
- *Figures 3 and 11 are difficult to read. ACPD please note.*

Interactive comment on Atmos. Chem. Phys. Discuss., 13, 4127, 2013.

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